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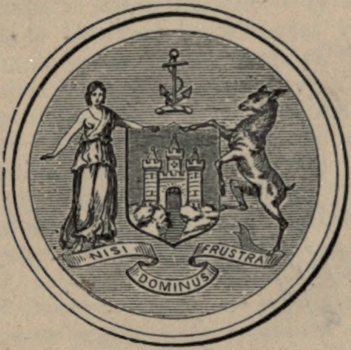

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On some Malformations of the Organs of Generation.*

By WM. TURNER, M.B.Lond., F.R.C.S.E., Senior Demonstrator of Anatomy, University of Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 7th December* 1864.)

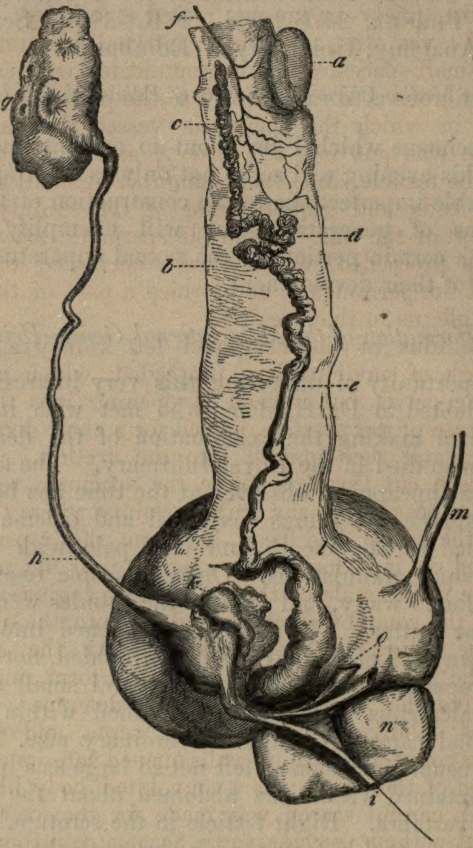
THE four specimens which I am about to exhibit and describe to the Society this evening will serve not only as illustrations of some very remarkable imperfections in the construction of the male and female organs of generation, but will exemplify some of the changes which certain portions of the sexual apparatus go through in the course of their development.

CASE 1. *Malformation of the Male Internal Genito-Urinary Organs.*

For the opportunity of dissecting this very interesting preparation I am indebted to Dr Haldane, who met with it about three years ago, when making the examination of the dead body of a man æt. 32, who died in the Royal Infirmary. The following note of some of the appearances observed at the time has been furnished me by Dr Haldane:—"Lungs congested and œdematous. Right kidney weighed 10 oz., smooth, and very pale, with a few indistinct semi-opaque granulations. On microscopic examination, the vessels were found waxy, and some of the tubules were filled with finely granular matter. The right ureter opened into the bladder in the usual way. Left kidney greatly atrophied, not so large as a prune; its proper structure absent, but several small cysts, one of which had calcareous walls, were contained within its capsule. Left supra-renal capsule of fully the ordinary size. Right renal artery of the usual dimensions; left not so large as a lumbar artery. Left testicle retained within the abdomen about the level of the third lumbar vertebra. Right testicle in the scrotum."

The left testicle and kidney, the bladder, with the adjacent structures, were the parts submitted to me for dissection. Situated behind the peritoneum of the left side of the back of the abdomen, and covered by that membrane on its anterior and lateral aspects, was an elongated body, nearly 1½ inch long and about ½ an inch in its antero-posterior diameter. This body was the left testicle. Beneath its serous coat, a well-marked fibrous tunica albuginea existed, and when this was cut through, the characteristic tubuli seminiferi were seen both with the naked eye and the aid of the microscope. None of the tubuli, however, pierced the tunica albuginea, and it was evident that the globus major or head of the epididymis was completely absent. In relation to the posterior margin of the testicle, and towards its lower end, but quite separate from it, was a fine tubule, thrown into tortuous, convoluted folds, which had the same appearance as the body of the epididymis, and apparently represented

Fig. 1.



Explanation of Fig. I.—*a.* Left testicle. *b.* Parietal peritoneum. *c.* Body of epididymis. *d.* Globus minor. *e.* Vas deferens. *f.* Spermatic artery. *g.* Left kidney. *h.* Ureter. *i.* Bristle in prostatic orifice of ureter. *k.* Vesicula seminalis. *l.* Bladder. *m.* Right ureter. *n.* Prostate gland. *o.* Remains of right vas and vesicula. Figs. 1 and 3 have been engraved from drawings of the preparations made by my pupil, Mr J. W. Moir.

that structure. After a course of something less than two inches this convoluted tubule was doubled several times upon itself, and presented the appearance of the globus minor of the epididymis. The tubule, as it emerged from this body, gradually increased in size, lost its convoluted arrangement, and became much thicker in its coats, possessing, in fact, all the characters of a vas deferens. It then passed downwards behind the peritoneum into the pelvis, where it came into relation with the base of the bladder. Twice in its course it was thrown into folds, and when it reached the base of the bladder it became considerably dilated, formed cæcal pouches at it sides, and ended abruptly close to the base of the prostate in a short, finely-drawn-out, closed extremity. The dilated portion of the vas was subdivided internally by imperfect septa into several small spaces, which communicated with each other.

A slender fibrous cord ran parallel to the posterior margin of the left testicle, in which the spermatic vessels were situated. An injecting pipe was put into the artery, and through it injection passed into the testicle, and along a number of small arteries which supplied the body and globus minor of the epididymis, and the commencement of the vas. From the spermatic artery also many fine branches proceeded, which were distributed to the areolar tissue behind the peritoneum, and formed a part of the sub-peritoneal arterial plexus in that locality.[1]

From the hilus of the atrophied left kidney a ureter, well formed, though of small calibre, proceeded, which passed downwards into the pelvis, but instead of opening into the bladder it entered the base of the prostate, passed for a short distance through its substance, and then opened into the urethra. The prostate gland had been cut through before the specimen reached me, so that it was not possible to say with certainty where the exact termination of the ureter had been, but from the appearance of the part it seemed as if it had either opened into the utriculus—the representative in the male of the uterus and vagina—or, following the course of the common ejaculatory duct, into the urethra at the margin of the mouth of the utriculus. As the ureter lay at the base of the bladder, it lost its cylindrical form, and assumed an appearance like the lower end of a vas deferens. Its canal, for example, was subdivided by imperfect septa, and several small cæcal pouches connected with its walls opened into its cavity. Close to this portion of the ureter was a convoluted body like a vesicula seminalis. A careful search was made for the excretory duct of this body, but without success, for it became so intimately blended with the outer fibrous coat of the ureter that all attempts to separate them resulted in injury to the latter. In the cæcal pouches of this vesicula, an inspissated secretion was contained, but no spermatozoa could be detected in it on microscopic examination. Some of the pouches had their walls affected with calcareous degeneration. In close relation to the right half of the base of the prostate were two tubes, both of which had been cut short before the preparation reached me. They were evidently the right vas and vesicula. Their excretory ducts were impervious, and converted into slender fibrous cords, which ran along the lower end of the left ureter before it entered the prostate and were blended with the fibrous coat of the left vesicula as it surrounded the ureter.

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[1] For an account of the connexions and relations of the sub-peritoneal arterial plexus, I may refer to my Memoir on the subject in the Medico-Chirurgical Review, July 1863, p. 222.

In considering the peculiarities exhibited by this interesting preparation, it must be noted that not only was the left testicle retained in the abdominal cavity, but the vas deferens corresponding to it was a tube closed at both ends, having no connexion with the testicle at the epididymis, and possessing no opening into the urethra at its lower end. The atrophied condition of the left kidney, and the termination of its ureter in the prostatic portion of the urethra, are also worthy of notice. That one or other kidney may atrophy almost to complete disappearance is a fact so well known to pathologists that I need not adduce other examples in confirmation of this specimen. But cases of transposition of the lower end of an ureter are so rare that it may not be amiss to cite a few examples. And, not to speak of cases in which, with congenital absence of the bladder, the ureters opened either on to the pubic region or into the urethra,[1] I may here refer to a case recorded by Thilow (Meckel, Path. Anat., p. 742), in which the ureter went beyond the bladder and opened into the urethra; to a case recorded by Förster,[2] where the left kidney and ureter were wanting, and the right ureter opened into the left side of the bladder, at the spot where the left ureter should have terminated had it been present; and to a second case detailed and figured[3] by the same accurate pathologist, in which, in a female, the ureter ended in the left side of the uterus. If the ureter, in the case I have related in this paper, ended in the utriculus, then it may be compared with the second case recorded by Förster, for the prostatic vesicle is the homologue of the uterus and vagina. Sometimes the ureter ends inferiorly in a closed extremity. When on a recent visit to Oxford, Dr Acland showed me, in the pathological collection of the University, a preparation of the kidney and ureter of a pig. The ureter, about ten inches long, ended inferiorly in a blind pouch, which was distended with a brown fluid.

Cases of retention of the testicle in the cavity of the abdomen are by no means unfrequent; but cases of retention, conjoined with lack of connexion between the testicle and the vas deferens, and with or without closure of the vas at its urethral end, are amongst the most unusual specimens of imperfect development. Brugnone[1] saw a man, æt. 27, in whom the right vas was deficient from the entrance to the pelvis as far as the head of the epididymis. John Hunter,[2] in dissecting a male subject, found on the right side the vas deferens absent in the cord from the epididymis to the neighbourhood of the external abdominal ring, whilst, on the left side, the vas was present, but the body and globus minor of the epididymis were not developed. The vasa had no communication with the urethra. Mr Paget[3] relates a case in which, with imperfection of the left testicle, the excretory duct was deficient as far as the inner aspect of the external abdominal ring. He alludes to a case by Bosscha, where the whole of the left vas was wanting, except two inches that ascended from the epididymis. Other cases by Gosselin and Tenon are referred to by Mr Curling in his "Treatise on the Testicle." In all these instances of imperfection of the vas, or epididymis, the testicles had entered the scrotum. But in the museum of St Bartholomew's Hospital is a preparation which more closely, though not altogether, resembles Case 1; for the testicle is described as detained in the inguinal canal, the epididymis is absent, and the vas ends near the testicle in a cul-de-sac.

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[1] For specimens of this kind I may refer to the cases by Thilow, Blasius, and Binninger, cited by Förster, in his recent treatise, "Die Missbildungen des Menschen," Jena, 1861.

[2] Virchow's Archiv, vol. xiii., p. 275. 1858.

[3] Die Missbildungen, p. 127.

Some recent inquiries into the mode of development of the organs of generation may perhaps assist us in determining the changes which have taken place in the production of the peculiar malformation of the epididymis in the case before us. Embryologists agree in stating that the genital gland—testicle in the male, ovary in the female—arises as a thin white streak along the inner border of the Wolffian body, or primordial kidney. But the relation between the two is merely that of contiguity, for no continuity of structure exists between them. Two tubes extend along the outer border of the Wolffian body,—one the excretory duct, into which the tubular secreting structure in the interior of the gland opens; the other the Müllerian duct, which is not continuous with the secreting tubules, but simply lies in contact with the outer border of the Wolffian body, and terminates superiorly in a free closed end. Both the excretory duct and the duct of Müller open inferiorly into the sinus urogenitalis. If the changes taking place in these structures be traced in the male embryo, it will be seen that the Wolffian body atrophies, or at least does not increase in proportion to the growth of some of the other structures, and so, by comparison, seems to atrophy; whilst, as most embryologists admit, the excretory duct of the Wolffian body persists, increases in size, and becomes the vas deferens, globus minor, and body of the epididymis. Thus the excretory duct of the testicle is developed from a structure distinct from the gland itself, and has at first no connexion with it.[1] It is not until the globus major, or head of the epididymis, is formed, that the communication between the testicle and its excretory duct is established. From what, then, is the globus major developed? Kobelt held, and his opinion has been adopted, with some slight modification, perhaps, in the details, by many subsequent writers,[2] that the head of the epididymis consists of the persistent tubules of a part of the Wolffian body, which, whilst on the one hand preserving their continuity with the excretory duct, on the other acquire a connexion with the seminiferous tubules of the testicle. But this opinion of Kobelt's has been of late disputed by more than one observer. Thus Dr Cleland, in an account of the development of the testis, introductory to his prize thesis "On the Mechanism of the Gubernaculum Testis,"[3] states that the coni-vasculosi of the head of the epididymis are formed in a convoluted mass which is situated at a spot originally occupied by a part of the Wolffian body, but quite distinct from, and not composed of, the tubules of that organ. Still more recently, Dr William M. Banks, in his prize thesis "On the Wolffian Bodies in the Fœtus,"[4] confirming the observations of Dr Cleland, has described a new structure filling up the place in which the upper end of the Wolffian body, now atrophied, originally lay. "As the Wolffian body disappears, this new structure becomes more and more distinct." It eventually rests on the upper end of the testis, lies in the position of the globus major, and the tubes which it contains become continuous with the canal of the epididymis on the one hand, with the tubuli seminiferi within the testis on the other. By these observations, the statement originally made by Johannes Müller, that the Wolffian bodies are not converted in the process of development into any other organ, is strikingly confirmed.

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[1] Meckel, Path. Anat. vol. i. p. 687.

[2] Animal Economy, p. 31.

[3] London Medical Gazette, vol. xxviii. p. 817. 1841.

It will be remembered, that in the case the peculiarities of which I have detailed, the left testicle, the vas, the globus minor, and body of the epididymis, were all present, but that the globus major was entirely absent. Hence, in this case, the genital gland and the excretory duct of the Wolffian body, from which those parts are developed, had been formed in the usual way. The left Wolffian body, also, had in all probability originally existed, but its atrophy had been so complete that no remains of it, not even the corps innominé of Giraldès could be detected. With the atrophy, however, of the Wolffian body, no new structure had formed at its upper end, hence the globus major was altogether absent, and the testicle was dissociated from its excretory duct. It is not so easy to offer an embryological explanation of the mode of production of the malformation in its lower or prostatic portion. The closure of the vas at its prostatic end, the peculiar vesicular-like condition of the lower end of the left ureter, and the prolongation of that tube onwards in the course of the ejaculatory duct, are important features in the specimen. And although it would be quite possible to frame an hypothesis to account for the way in which the closed condition of the one tube and the substitution for it of the other tube in the prostatic part of its course have been brought about, yet I am doubtful if the present state of embryological knowledge will admit of one that would be altogether satisfactory.

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[1] It is but right to state, however, that some embryologists hold that the vas deferens and body and tail of the epididymis are developed from the Müllerian duct, and not from the excretory ducts of the Wolffian body. But the balance of evidence seems to me to be in favour of the opinion stated in the text, in accordance with which the hydatids of Morgagni, and the prostatic vesicle, are in the male the sole persistent remains of the ducts of Müller.

[2] Kölliker Entwicklungsgeschichte, p. 444.

[3] Edinburgh, 1856.

[4] Edinburgh, 1864.

In the absence of any history of the man from whom this preparation was obtained, it is not possible to say whether he had at any time possessed any sexual desires. So far as regards the construction of the left testicle and its excretory duct, it cannot be doubted that there must have been a complete want of any seminal secretion from that organ—the closure of the duct at both ends shutting off the gland completely from the urethra—and the defect is, without question, one of a congenital nature. But even if the canal between the testis and the urethra had been continuous and quite pervious, and the testicle still retained in the cavity of the abdomen, it would not necessarily have followed that the fluid secreted by the gland so situated should have contained spermatozoa; for the cases observed and recently published by Mr Curling,[1] seem to me to show that the seminal fluid in cryptorchismus is not unfrequently destitute of spermatozoa, even when the sexual desire and capacity for intercourse of the man are strongly developed. Of the condition of the right testicle I am unable to say more than that it occupied the right side of the scrotum. Its excretory duct was also closed at the prostatic end, so that on this side too, even if the secreting powers of the testis were perfect, there could have been no passage of seminal fluid into the urethra. But whether this closure of the right vas and its corresponding vesicula was congenital, or was occasioned by some inflammatory affection of the tube, it is now impossible precisely to determine, though I am inclined to think, from the manner in which these structures were blended with the left vesicula and left ureter at the base of the prostate, that on this, as on the other side, a congenital defect in development had occurred.

(*To be continued in our next Number.*)

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[1] Medico-Chirurgical Review, April 1864, p. 498.

ART. II.—*A Critical Inquiry regarding Superfœtation, with Cases.*

By GEORGE LINDSAY BONNAR, M.D., Cupar.

(*Read before the Obstetrical Society, 23d November* 1864.)

THE question of the possibility of superfœtation is one which seems to be still undecided in the medical world. Its occurrence is doubted and denied by many eminent authorities, while, on the other hand, its possibility is no less strenuously asserted and defended by many others equally deserving the respect and confidence of their brethren. It is undoubtedly true that the recorded cases of genuine superfœtation are few, and those are fewer still which have their *facts* so sufficiently distinctly recorded as to enable us to meet the many arguments which opponents may urge to explain them away. They have been mixed up, too, with many other narratives, related as instances of superfœtation, but which are obviously explicable on other and generally received grounds, and this perhaps has tended to cast undeserved doubts on the whole matter. But there still stand out a few recorded narratives which defy the explanations of all disbelievers, and which have necessitated them to resort to unwarrantable imputations against either the honesty or competency, or both, of the narrators, rather than give up some favourite theory, which, to be *right*, must necessarily depend on their making out the others to be *wrong*.

The subject is one demanding careful investigation, and a candid weighing of all the evidence for and against the possibility, the frequency, and the manner of its occurrence. Like many other medical doctrines, professional opinion seems at different times to have ebbed and flowed with regard to it. Our ancestors were firm in their belief as to its possibility—nay, comparative frequency. About forty or fifty years ago, a general scepticism seems to have pervaded the profession regarding it, and now, it would appear, the tide is again setting in towards a general recognition of its truth, especially since the mysteries of procreation have been better understood, by a more advanced physiology, and a more minute pathology.

Cases which have been recorded as instances of superfœtation resolve themselves into three kinds.

1*st*, Where two mature children have been born at the same time, but bearing evident marks of being the offspring of different parents. This fact is undisputed, but the cases are examples of *contemporaneous conception*, or *superconception*. The only question which concerns us here is, What is the period up to which this occurrence may take place, and beyond which, according to the opponents of superfœtation, it cannot? for it is obvious the latter and superconception are the same phenomenon, occurring only at different periods in relation to the primary conception.

2*d*, Where a twin has been aborted, leaving its fellow still in utero to be matured and given birth to in due time; or when a twin is destroyed, or arrested in its development and retained, until afterwards expelled with its mature fellow.

Cases of this description are by no means rare, and it is perhaps because so many of them have been recorded as instances of superfœtation, and so obviously erroneously, that the latter doctrine has fallen so much into disrepute. Probably the misleading proof of the impossibility of the blighted fœtus belonging to the same conception as its fellow, consisted in the complete absence of putridity or decomposition in connexion with it, thereby apparently showing that its death had been but very recent; but putrefaction in the womb does not necessarily result when death of the fœtus has taken place. Dr F. Ramsbotham says, while advocating this mode of explaining away many recorded cases of superfœtation, "The only point requiring explanation is the fact that in no instance, so far as I know, has a *secondary fœtus* been expelled putrid, although it had died some months previously; and this even may be accounted for by the powerful vital principle which is resident in the uterus, and which is in fervid operation for the purpose of bringing to perfection the living being it contains, protecting the dead mass from the ordinary changes of decay."[1]

Setting aside these cases, then, we come to the third division of our subject, instances of true superfœtation.

3d, Where a *mature* child has been born, and an immature fœtus, the product of a different conception, has either been left in the womb until its period of maturation, or, if expelled along with the other, has presented no marks of wasting, or of arrested development. I leave altogether out of consideration those cases where this phenomenon depends on double uterus, or extra-uterine conception.

In a case of genuine superfœtation, then, a woman must bear two (or more) *mature* children, with an interval of weeks or months between the birth of each; or if she part with the whole contents of the uterus at the first delivery, the difference of the ages of the fœtuses, or the mature child and the fœtus, as the case may be, must be unmistakable, and there must be the absence of all marks of blight of the latter, so as to leave no doubt that, had it remained in utero, it would have gone on to perfect maturity.

First, in regard to *mature* children.

Velpeau quotes the "Recueil de la Société de Médecine" for the case of a woman named "Arles," who, in 1796, gave birth to a child at the *full time,* and five months afterwards to another, which was also thought to be at the full time.

The only author I have met with who takes notice of this case, is Campbell,[2] who wisely makes no attempt to explain it away, but summarily dismisses it with the announcement, that unless "Arles was possessed of a double uterus, it is unworthy of belief,"—a mode of getting rid of a difficulty not the most philosophic, but a bold and dashing attempt to cut what he could not untie.

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[1] Medical Gazette, vol. xvi. p. 216.

[2] Campbell, p. 95.

In the fourth vol. of the London Medical Transactions, p. 161, Dr Maton communicated to the College of Physicians that Mrs T——, an Italian lady, was delivered of a male child at Palermo, on the 12th November 1807, which had every appearance of health, but lived only nine days. On the 2d February 1808, within a few days of three kalendar months, or 82 days after the birth of the first child, she was delivered of a second completely formed, and apparently in good health. Both children were born perfect and mature.[1]

This case has created a great deal of discussion amongst those whose investigations have been brought to bear on the subject. Beck, with his wonted candour, admits that he does not pretend to explain it. Campbell[2] presumes that the first child was immature, born at the close of the seventh month, and the second retained two or three weeks after the general period required for human gestation. Dr Granville,[3] in a paper he wrote in the Philosophical Transactions for the year 1818, on the Malformation of the Uterine System, says that this case "merely goes to prove the occasional co-existence of separate ova in utero, and proves nothing farther. The lady," he goes on to say, "whose prolific disposition is much descanted on in Dr Maton's paper, and with whom twin cases was a frequent occurrence, was delivered of a male child sometime in November 1807, *under circumstances very distressing to the parent, and on a bundle of straw*, and again in February 1808, of another male infant completely formed! Mark the expression," continues Dr Granville, "for it was not made use of in describing the first. The former died, Dr Maton tells us, without any apparent cause when nine days old, the other lived longer. Now," reasons he, "can we consider this otherwise than as a common case of twins, in which one of the fœtuses came into the world at the sixth, and the other at the ninth month of pregnancy, owing to the ova being quite distinct and separate? Had this not been the case, the *distressing circumstances* which brought on the premature contraction of the womb, so as to expel *part* of its contents in November, as in the simplest cases of premature labour, would have caused the expulsion of the whole, or, in other words, of both ova in that same month, and we should not have heard of the second *accouchement* in the following February, which led the author of the paper in question to bring the case forward as one of superfœtation, in opposition to what he has called 'the scepticism of modern physiologists.' Had it been proved that the child of which the lady in question was delivered had *reached the full term of utero-gestation*, and that she had brought forth another child one, two, or three months afterwards of equally full growth, then a case something like superfœtation would have really occurred, and scepticism would have been staggered."

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[1] Campbell, p. 95; Beck, p. 88; Paris, vol. i. p. 262.

[2] P. 96.

[3] Paris, vol. i. p. 263.

Caspar,[1] in his efforts to disprove the doctrine of superfœtation, to which he seems to have a great repugnance, takes the same method of explaining away Maton's case. "It is evident," he writes, "that this case of a woman who bore twins twice is nothing more than a third conception of twins. The 'proper maturity,' which is an uncertain expression, might very well be possessed by the first born child at the end of, say two hundred and ten days; and as the other twin was born eighty-one days subsequently, it, as a late born child of two hundred and ninety-one days, would certainly be 'perfectly mature,' and the whole case would resolve itself into what has been so often mistaken for superfœtation, namely, a twin pregnancy, one of which (alive or dead) has been born prematurely, and the other some time after."

Now, as the case must stand or fall by the degree of maturity to which the first child had reached, and as the three authorities whom I have quoted, indirectly, and one of them, indeed, avowedly, hang their objections on the want of details in the original history of the case on this very point, and take full advantage of the omission, it is obvious that further information obtained from the first source must determine the exact nature of the case in dispute. Accordingly, and in consequence of the observations already quoted from Dr Granville's paper, Dr Paris made *personal inquiry* of Dr Maton for a further explanation of those particular points on which the merits of the case would seem to turn, and he states, "the fact is, that both *the children were born perfect*, the first, therefore, *could not have been a six-months child.*" He further states, "Dr Granville seems to have fallen into an important error with respect to the *distressing circumstances* which attended the delivery."[2] They were not the *cause* of the labour, as Dr Granville insinuates, but the natural result. The lady could not obtain proper accommodation at the time, although it was not so sudden but that the accoucheur was in attendance, and it was not premature, for it was thought by all that the natural time of utero-gestation had been completed; and Dr Paris concludes by stating, that there cannot exist any good reason for questioning the veracity of the husband, who furnished Dr Maton with all the particular circumstances of the case, nor the justice of the conclusions arrived at.

Unless we charge the narrators with dishonesty, and refuse to believe their statements, we must admit that this case proves our point.

The narrative, however, which has been the principal battlefield of the advocates of superfœtation and their opponents, is the following, which we shortly give, in order to notice the objections which have been urged against it by Campbell and Caspar.

Dr Desgranges,[3] of Lyons, says :—"Madame Villard, of Lyons, had married at twenty-two, and five years afterwards became pregnant. She had a miscarriage at seven months, on the 20th May 1779. In about a month thereafter, she conceived again, and on the 20th January she brought forth a living child. No milk appeared in her breasts, the usual discharge was absent, and the abdomen did not seem to diminish in size. Two surgeons who were in attendance became puzzled, and called Dr Desgranges, who declared there was a second child in the womb. This opinion was much doubted, yet three weeks after her delivery she felt the movements of the child, and was delivered of another living daughter on the 6th July 1780, five months and sixteen days after the first birth. Dr Desgranges adds, although there seems little necessity for it, that it was impossible the last infant could have been conceived after the expulsion of the first, 'car le mari n'avait renouvelé ses caresses a sa femme que vingts jours apres,' which would only have given to the second infant four months and twenty-seven days." This case has been the subject of great controversy, and exhibits great ingenuity on the part of the opponents to the doctrine, inasmuch as the difficulties to be overcome in setting it aside are formidable.

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[1] Caspar, Forensic Med., iii. p. 374.

[2] Paris, Med. Juris., p. 264.

[3] Campbell, p. 94; Beck, p. 89; Paris, p. 263; Caspar, p. 373.

Beck, while admitting his inability to explain the case, says that it is one of the strongest yet adduced in favour of the opinion, and that its credibility would seem to be established from the character of the reporter, and the publicity attending it.

We are told in the narrative that the mother and both children, two years afterwards, appeared before a notary at Lyons[1] to attest the fact in a legal manner, and because, it is stated in this declaration, that it was also "partly to prove her (the mother's) gratitude to Dr Desgranges, and partly to give women who may find themselves in a similar predicament, and whose husbands may be deceased previous to the birth of both of the children, a precedent in *favour of their virtue* and the legitimacy of the child, Caspar, without one reference to the *facts* of the case as recorded by Desgranges, and which are detailed by him altogether irrespective of any further connexion with the woman herself, dismisses the whole thing in this summary way—" For my part, I confess that this remarkable proceeding of this woman, so 'virtuous,' and so grateful to her doctor for having delivered her (!) renders the whole case perfectly incredible."

It is surely not unreasonable to expect something like *argument* in opposing the establishment of a disputed point such as this; but where an author and authority so justly looked up to as Caspar condescends to let his opinions be guided by circumstances so foreign to the real merits of the case, as in this instance, he only strengthens the position of his opponents, by making it manifest that he has not one word of any value wherewith to confute them.

Campbell dissents from believing this case on other grounds.

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[1] Caspar, p. 374.

He at once assails, not the character of the mother, as Caspar, but the veracity of the historian, and states that "we may conclude either that it is misstated, or that the woman possessed a double uterus," and as a ground for the imputation, he assumes the following hypothesis as fact—"We are told," he writes, "that she was impregnated a month after her premature birth; but this is very improbable, if not impossible, supposing that she had but a single uterus; for the organ could not be so easily restored to its healthy condition as to have enabled this woman to conceive in such rapid succession. I think it would be difficult," continues he, "to produce a properly-attested instance of a woman conceiving in so short a period as a month after delivery." I cannot see any force in this conjecture of Dr Campbell's; and, setting aside the questions whether parturition is itself but the return of a catamenial period, and, if so, whether the bursting Graafian follicle is then about to discharge an ovum, which it is quite possible to vitalize by contact with the spermatozoa,—questions which yet hang trembling in the balance of medical theory,—I proceed to prove by citation of facts that Campbell's statement is wrong; and as it is a matter of much interest, and of considerable medico-legal importance, I shall examine the question with some care.

Were the statement true that the female procreative organs were not capable of exercising their functions for at least 30 days after parturition, and reckoning the period of utero-gestation to extend from the 274th to the 280th day, it would follow that no woman could bear a mature child sooner than the 304th, or from that to the 310th day after the date of her last confinement. Let us see.

Every medical man of any standing has, within the range of his own experience, had instances of repeated confinements within the 365 days.

An interval of 353 days elapsed between the births of the two eldest of the present Royal family.

The source of the following instances is principally Blackett & Hurst's edition of the Peerage, which seemed to us a ready and an authentic authority for supplying materials for not only refuting Campbell's objection to the truth of Desgranges' case, but also of adducing many facts in support of the doctrine of superfœtation itself:—

Lady Margaret, second daughter of the 5th Earl of Balcarres, was born on the 14th February 1753, and her brother on the 25th January 1754, being an interval of 345 days.

Between a younger son of the same nobleman, the Hon. James Stair, who was born on the 16th December 1758, and his brother, Hon. William, born on the 21st November 1759, there is an interval of only 340 days.

The Hon. and Rev. Augustus George Legge had two sons, William, born on the 29th July 1802, and Henry, on the 29th June 1803, showing an interval of 335 days.

The present Baron Stourton, County of Wilts, was born on the 13th July 1802, and his elder sister on the 16th August preceding, 331 days being the interval.

The present Earl Annesley, born on the 21st February 1830, had a brother stillborn in the previous April, date not recorded, but allowing it to be early in the month, say the 2d, this would bring the interval down to 325 days.

The Earl of Carlisle had two younger sisters, one born on the 24th June 1803, and the other on the 10th May 1804, leaving an interval of 320 days.

The two youngest brothers of the Marquess of Normanby, one born on the 7th December 1808, and the other on the 18th October 1809, had only 316 days between them.

The two eldest children of the present Baron Clarina, Anna Emily and Hon. Eyre-Challoner-Henry, were born on the 23d June 1829, and the 29th April following, respectively, bringing down the interval to 310 days.

I pause here to notice an objection which may be taken to my illustrations, Am I prepared to authenticate that the second births of the instances related above were those of mature children? I am not. But the examples from which I have culled the above list are *so numerous*, that I can conceive the objection to have little force.[1]

Lest, however, it be urged, I refer to a case in which I know and testify that both children had arrived at the full period of utero-gestation. The Rev. John B., in Fife, had a daughter born on the 9th February 1827, and a son on the 11th December of the same year, their being an interval of 306 days between the births, only two days beyond the minimum period required to refute the objections of Dr Campbell.

The late Earl of Buckingham had a daughter born on the 16th September 1832, who died on the following day, and another on the 17th July 1833, who attained maturity, diminishing our last interval by another day.

The present Earl of Beverley had two elder sisters, the first born on the 3d June 1776, and the other on the 31st March 1777, who survived two years, this being an interval of 302 days.

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[1] After noting 13 instances of intervals between consecutive births, varying between 340 and 350 days, they appeared to be following each other so frequently, that I did not think it worth while to pursue the inquiry further between these periods.

The instances between 330 and 339 days are 20, the second children all having arrived at maturity.

Between the 320th and 329th days the instances are 23, all of whom attained maturity.

We have 13 instances between the 310 and 319 days, all the products of the second birth having attained maturity.

Between the 300 and 309 days we have ten cases, all the second births having lived more than six months, and the majority having reached adult life.

From 290 to 299 days we have two instances; both attained maturity.

From 280 to 289 days we have four instances; all attained maturity.

The intervals less than these I have noticed afterwards individually.

The lady of the Hon. and Rev. Charles Dundas, rector of Epworth, had a daughter born on the 18th May 1834, and a son, Henry, on the 8th March 1835, who attained manhood, showing an interval of 294 days.

The Hon. Lieut.-Col. Dawson, who was killed at the battle of Inkermann (5th Nov. 1854), left a son, Vesey John, who was born on the 4th April 1853, having been preceded by a brother, still-born, on the 29th June 1852, being 289 days previous. The same interval exists between G. W. Hill and his brother, Clement Lloyd, of Omberley, Worcestershire, the former of whom was born on the 20th July 1843, and the latter on the 5th May following. Both attained maturity.

An interval of one day less occurs between the births of Mabel and Gerald Anthony Addington, grandson of Viscount Sidmouth, who were born on the 14th November 1853, and the 29th August 1854, respectively.

The present Earl of Ellenborough had a brother born in July 1804, day unrecorded, but giving it all latitude, and taking it for granted that it was the *first*, his sister, the Hon. Frederica-Selina, was born on the 6th April following, which reduces the interval to 280 days. The latter lived, and married in August 1829.

This reduces the interval to the natural period of gestation, and it follows either that there had been no cessation at all of the procreative function; that the phenomena of parturition were merely the renewal of a true catamenial period, leaving the organs immediately thereafter susceptible of impregnation; or, that the second product was born a short time before the completion of maturity.

Even allowing that at parturition there is a detachment of an ovum by bursting of a Graafian follicle, as occurs in menstruation, although I am not aware of any pathological proofs on the subject, yet I think that the pathological changes which the uterus itself has undergone during gestation, and the abnormal condition in which it is in relation to that which is usually termed its unimpregnated condition, it will generally be conceded that a certain time must elapse ere it is rendered a fit receptacle for an impregnated ovum; and as, in the instances adduced, I have no positive evidences that they had arrived at the full period of utero-gestation, I will conclude that they were immature.

I think I have ere this fully demonstrated the fallacy of Dr Campbell's first objection to Madame Villard's case, and rather than follow him to refute his second and only other objection, which is pure unsupported asseveration, viz., that the mother must have had a double uterus, I rather pursue my present inquiry, which becomes increasingly interesting and important.

At this stage, I have to dispose of two questions before I can proceed further with my citations.

What is the shortest interval likely to elapse after parturition before impregnation can again take place? And at what age of uterine life is it possible for a child to be reared to such a period as to show that it had at least outlived the accidents and liabilities necessarily attendant on premature birth?

With regard to the first, there are, especially in medico-legal works, many cases detailed of the rapidity with which all signs of recent delivery disappear. So long, however, as the indications of such an occurrence are perceptible to sight and touch, I presume it will be conceded that impregnation, physiologically, cannot take place. I, of course, allude to such signs as affect the internal organs, the most important of which are these three: the tender and swollen vaginal canal; the enlarged uterus, with its open and relaxed os; and the lochial discharge; and they usually disappear in the above order.

The state of the vagina is a sign on which little dependence can be placed, for the size of the child, the frequency of previous births, the constitution of the mother, and a variety of other considerations which will suggest themselves to every mind, may so influence a delivery, and cause so slight a disturbance of the natural conditions of this canal, that in the course of a few days it may have regained all its former size and firmness.

The state of the uterus is of great importance in this inquiry. Within the first few days it may be felt enlarged above the pubis. In eight or ten days it generally disappears into the pelvis, the cervix retracts, and the os regains its usual size; so that, in so far as the *dimensions* of that organ are concerned, they would then indicate a return to its usual conditions, and fitness for being the cradle of a new fœtus.

But I consider that so long as the lochial discharge is secreted, this function of the womb cannot, in the nature of things, be exercised. Great variations are observed in regard to the time of its cessation, and from one to three or four weeks seems to be the period during which this discharge may in general be said to exist. It is usually observed that where nursing is dispensed with, it continues longer and more abundant than in other cases, and in the instances under our consideration, we may presume that the function of lactation was not encouraged; I think I cannot be overstating the time when I say that the *fourteenth* day after delivery is the earliest period at which the functions of the uterus can be so restored as to render it again capable of performing its part in procreation.

The *second* question is one not so readily settled. When Dr Wm. Hunter was interrogated on this point, he replied, "a child may be born alive at any time after three months; *but we see none with powers of living to manhood*, or of being reared, before seven kalendar months (210 days), or near that time; at six months it cannot be." With this opinion medical authorities generally have agreed, although a few cases have been put on record where at earlier periods of gestation children have been born and reared. To these I shall afterwards recur, but in the meantime I shall take 210 days as the earliest period at which a child then born can be reared, and shall deduct from the intervals between the consecutive births in my illustrations the period of *fourteen days*, being the shortest possible time necessary to fit the uterus, after delivery, for a new impregnation.

In the following citations I have been most particular in reference to the correctness of the extracts, corroborating their authenticity as taken from Lodge's Peerage, by a reference to subsequent editions of the same work, or to Burke and Debrett; and I especially refer to the length of time the *second child survived* in each case, as taken from the same sources.

It will be remembered that I brought the last interval down to 280 days, or more truly, deducting the 14 days, to 266 days. To continue—

The Hon. George, fourth child of the first Lord de Blaquiere, Londonderry Co., who married Miss Dobson of Annegrove, Co. Cork, was born on the 27th July 1782. His younger brother, Hon. Peter Boyle, was born on the 26th April 1783, and was alive in Upper Canada in 1859. This is an interval of 273 days, or, less 14, 259 days.

The Hon. and Rev. Thos. J. Twistleton, D.D., Archdeacon of Colombo, had, by his first wife, Charlotte-Ann-Frances Wattell, a daughter, born on the 17th October 1789, who reached maturity, and a son, Francis-Henry-Thomas, born on the 26th June 1790, who died on the 12th July 1792, having survived upwards of two years. This interval consists of 252 days, *minus* 14, or 238 days.

The Hon. Arthur Cole-Hamilton, second son of first Lord Mount Florence, married in 1780 Letitia, daughter of Claudius Hamilton, Esq., and had a son, born on the 7th July 1781, who lived to maturity, and a daughter, Letitia, born on the 5th January 1782, who lived, and married Major Stafford; this interval being one of only 182 days, which, diminished by 14, leaves 168; *and is less by 42 days than we have seen it is generally esteemed possible to rear a child born prematurely.*

But the accumulation of facts having materially altered opinion regarding the *earliest* period of utero-gestation at which a child can be reared, this question must be determined ere the last quotation from the Peerage, or any others which may be adduced, can be set down as cases of superfœtation. I have collected and tabulated all the instances of premature births I could collect, as follows:—

Tabulated View of Cases of Premature Births of Living Children.

Those marked * died sooner than 24 hours after birth; those with † survived more than 8 days.

AUTHORITY.	Where Recorded.	Where referred to.	Date of Birth.	Date of Death, or Age if Living.	Date of Gestation in Days.
	Brit. and For. Med. Rev., ii. 236,	Taylor's M. J., 570,	...	Born alive.	120
*M. Maisonneuve,	Jour. de Méd.; Med. Gaz., xxxix. 97,	Do.,	...	L. 6 h.	125
†Dr Rodman, Paisley,	Ed.M.J., vols xi. & xii.	Kinghorn Trial,	Ap. 19/15	L. 21 mo.	133
*Mr Smythe,	Med.-Chirur. Rev., July /44, 266,	Taylor's M. J., 571,	...	L. 12 h.	147
*Collins, Dublin (1),	Dub. Ly.-in Hosp. R.	Kingh. Tr., 100 B. C,	...	L. 3 min.	150
*Collins, do.,	Do.,	Do.,	...	L. 2 h.	150
Dominico Meli,	An. d'Hygiène Pub.	Kingh. Trial, 127 A,	...	...	150
Do.,	Do.,	Do.,	...	Lived.	150
Do.,	Do.,	Do.,	...		150
†Brouzet,	Own writings,	Do., 146 B. C.,	1748	L. 16 mo.	150
Dr Rüttel,	Henke Zeitschrift der S. A., 1844, 241,	Taylor's M. J., 572,	...	L. 24 h.	150
†Dr Barker, Dumfries,	Med. Times, Sept. and Oct. 1850,	Do., 572	...	L. 3½ y.	158
*J. B. Thompson, Alva,	Med. Gaz., xix. 665,	...	Jan. 27/37	L. 3½ h.	165
†Prof. Fleischman	Henke's Zeit., vi. 12,	Kingh. Tr., 147 C. G,	...	L. 8 d.	168
†Rev. F. Jardine, Kinghorn.	Kinghorn Trial,	Taylor's M. J., 573	Aug. 24/35	L. 7½ mo.	174
Zittman,	Do., 99 B.,	Kingh. Trial, 99 B.,	...	...	177
Ammon,	Do.,	Do.,	...	...	178
Velpeau,	Do.,	Do.,	...	...	180
*Collins, Dublin,	Dub. Ly.-in Hosp. R.	Do., 100 C.,	...	L. 5 min.	180
*Collins, do.,	Do.,	Do., 100 C.,	...	L. 5 min.	180
*Collins, do.,	Do.,	Do.,	...	L. 1 h.	180
*Collins, do.,	Do.,	Do.,	...	L. 6 h.	180
*Collins, do.,	Do.,	Do.,	...	L. 9 h.	180
*Collins, do.,	Do.,	Do.,	...	L. 10 h.	180
*Collins, do.,	Do.,	Do.,	...	L. 12 h.	180
*Collins, do.,	Do.,	Do.,	...	L. 16 h.	180
Collins, do., FIVE	Do.,	Do.,	EACH	L. 1 d.	180
*Whitehead, SEVEN	On Abortion,	Taylor's Med. J.,	EACH	L. 6 h.	180
*Dr Rüttel, one of TWINS	Henke's Zeit., /44, 241,	Do., 572,	...	L. 3 h.	180
†Dr Rüttel, TWINS	Do.,	Do.,	...	L. 1 y.	180
†Dr Halpin, Cavan,	Dub. Q. J., May 1846,	Do., 574,	...	L. 4 mo.	180
†Belloc,	Cours de M. Légale,	K. Tr., 143 G., 126 F.,	...	L. 15 y.	100
†Belloc,	Do.,	Do., 145 D., 126 F.,	...	L. 15 y.	180
†Thatcher, THREE	Kinghorn Trial, 97,	Do., 97 A.,	1835 EACH	L. 2 y.	180
†Ar. Millar, Edinburgh,	Do., 110,	Do., 110 D.,	Ap. 9/38	L. 4. mo.	180
†Js. Aitken, Ceres, Fife,	Do., 124,	Do., 124 A.,	Sep. 12/35	L. 6 w.	180
†Whitehead,	On Abortion,	Taylor's M. J., 572,	...	L. 10 d.	180
†Wm. Mayne, Glasgow,	Kingh. Trial, 131,	Kingh. Trial, 131 B.,	June 13/63	L. 11 d.	180
Kopp,	Med. Jurisp. (J. of).	Do., 146 D.,	...	L. 5 d.	180
†Dr W. Buchannan, Greenk.	Kingh. Trial, 116,	Do., 116 C.,	About 1799	L. to adult.	183
†T. Hutchins, dentist, Edin.	Do., 108,	Do., 108 B.,	1824	L. 14 y.	187
	Bucholtz's Beitrage, ii. 104,	Do., 145, foot-note,	...	L. 50 h.	189
†Dr Ontrepont, Bomberg,	Henke's Zeits.,	Do., 144 D.,	About 1822	L. 11 y.	189
†	Lancet, 1851, 177,	Taylor's M. J., 573,	1850	L. 4 mo.	190
†Mr Annan, Kinross,	Med. Times, Sept./48	Do., 572,	...	L. 4 mo. 1 w.	190
†M. Velpeau,	Gaz. des Hospitaux, 1851, No. 174,	Brit. & For. Med.-Chir. Rev. ix. 558,	Dec. 27/50	L. 16 mo.	190
†Dr M'Whirter, Edin.,	Kingh. Trial, 122,	Kingh. Tr., 122 A.,	1820	L. at 18½ y.	190
*	Pupil of Dr Taylor's,	Taylor's M. J., 576,	...	L. 14 h.	195
†Capunon,	M. Lég. des Ac., 162,	Do., 571. K.T. 145 F,	1818	L. 2 y.	195
†Capunon,	Do., 168,	Do., Do.,	...	L. 10 y.	195
Prof. Fleischman, TWO	Henke's Zeit., vi. 12,	King. Tr., 145, note,	EACH	L. 2 d.	196
†Merriman,	Baillie's divorce bill,	Do., 53 B.,	...	L. to mat.	198
Scotch gentleman,	Taylor's M. Jur., 573,	Taylor, p. 573,	...	L. to adult.	210
*Collins, Dublin, FOUR	Dub. Ly.-in Hosp. R.	Kingh. Tr., 100 C.,	EACH	L. 5 min.	210
*Collins, do.,	Do.,	Do.,	...	L. 15 min.	210
*Collins, do.,	Do.,	Do.,	...	L. 3 h.	210
*Collins, do.,	Do.,	Do.,	...	L. 4 h.	210
*Collins, do.,	Do.,	Do.,	...	L. 6 h.	210
*Collins, do.,	Do.,	Do.,	...	L. 9 h.	210
*Collins, do.,	Do.,	Do.,	...	L. 12 h.	210
*Collins, do.,	Do.,	Do.,	...	L. 13 h.	210
Collins, do.,	Do.,	Do.,	...	L. 26 h.	210
Collins, do., THREE	Do.,	Do.,	EACH	L. 36 h.	210
Collins, do., SIX	Do.,	Do.,	EACH	L. 1 d.	210
Collins, do., FOUR	Do.,	Do.,	EACH	L. 2 d.	210
Collins, do., THREE	Do.,	Do.,	EACH	L. 3 d.	210
Collins, do., SIX	Do.,	Do.,	EACH	L. 4 d.	210
Collins, do., TWO	Do.,	Do.,	EACH	L. 6 d.	210
Collins, do., TWO	Do.,	Do.,	EACH	L. 7 d.	210

105 cases,—69 survived upwards of one day. Of these 1 was merely born alive; 1 lived 3 min; 3, 5 min.; 1, 15 min; 1, 1 h.; 1, 2 h.; 3, 3 h.; 1, 4 h.; 4, 6 h.; 2, 9 h; 1, 10 h.; 3, 12 h.; 1, 13 h.; 1, 14 h.; 1, 16 h.; 15, 24 h.; 7, 2 d.; 3, 3 d.; 6, 4 d.; 1, 5 d.; 2, 6 d.; 2, 7 d.; 1, 8 d.; 1, 10 d.; 1, 11 d.; 1, 6 w.; 4, 4 mo.; the following lived, or were living at date of report,—1, 7½ mo.; 2, 12 mo.; 2, 16 mo.; 1, 21 mo.; 4, 2 y.; 1, 3½ y.; 1, 10 y.; 1, 14 y.; 2, 15 y.; 1, 18½ y.; 3, maturity; 6 "lived," not stated how long.

From all the information I have been able to acquire, and from my own experience, I think that before a child can be said to have outlived the immediate obstacles to independent animal existence, and before the respiratory and digestive organs be pronounced fit for sustaining the life and promoting the growth of the premature infant; in other words, before an immature child, born alive, can be pronounced *viable*, a period of at least eight days must be allowed to pass.

In the foregoing table, the first case calling for attention is that of Dr Rodman of Paisley. On examining into its details, it will be found that the only criteria he has for assuming that the infant had only arrived at the 133d day of gestation, were the statement of the mother's impressions, and the premature appearances of the child. But, unfortunately for the truth of the latter, he gives two facts which go to show the impossibility of its being so premature a production. He states that three weeks after birth (its life having been sustained with such difficulty that the presumption is, it could not have grown at all during that time, especially considering that it then laboured under the gum and the thrush, and was dosed once or twice daily with castor oil) it was weighed and measured. The weight was 39 ounces, without clothes, and the length 13 inches, and these are the average weight and measurement, according to Taylor and Caspar, of a small production of the 7th month, or 210th day; so that we must conclude that a grave mistake has been made by this practitioner in his estimate of the period of uterine life of this infant.

The next cases are three, by Dominico Meli, in the Annales d'Hygiène Publique, who dates their conception 150 days before birth; but as we have not the particulars of these cases, nor the length of time they survived, I must pass them over without further notice.

Dr Barker, of Dumfries, has recorded a case in the Medical Times of September and October 1850, in which a child was born 158 days after conception, and lived 3½ years. The weight and measurement are given. The former was one pound, the latter eleven inches. These are, with the development of the nails and hair, according to Taylor, those of a large child of six months, or 180 days; according to Caspar, scarcely so much; and all the data Dr Barker seems to have for fixing the age, is that it was so long after "intercourse." But this is, to say the least of it, very unsatisfactory evidence, unless he had unlimited confidence in the good faith of his patient.

Brouzet, of Fontainebleau, physician to Louis XVI., narrates a case in which a woman gave birth to a child just six months after another delivery. Allowing a fortnight in this case, as in our previous citations, for the uterus to fit itself for again becoming the receptacle of the ovum, we have a child born, and living for 16 months, which had arrived at 165 days only of utero-gestation. The details of this case are given so circumstantially, and the date of the previous delivery fixes so accurately the period of the commencement of gestation, that there is no room for doubt in respect of the facts, and the only question which may be raised, is, whether it is not a case of superfœtation. In the narrative it is implied that the product of the previous confinement was mature; had it been an abortion of a fœtus in a very early stage there might have been a conjecture that it had left a fellow behind, which continued other six months in utero; but assuming that the former was at or near the full time, I think, in the absence of all marks, weights, and measurements to guide us, that it is the least of two marvels to look on this child as the second product of a case of superfœtation, than as the one solitary instance on record of a fœtus surviving for any length of time, born so early as the 165th day of utero-gestation.

The next case demanding notice is the famous Kinghorn one, in which, it was alleged, the birth of a child occurred 174 days after marriage, and survived for 7½ months. We have not, as in the last case, the alternative of superfœtation to account for this extraordinary occurrence, but those who disbelieve in the doctrine, and give credence to Brouzet's narration, and the conclusion he himself arrived at, can have no difficulty in homologating the verdict which absolved the defendant on the occasion alluded to.

Passing over one or two cases, about which we have no details, we come to such a number of instances of children surviving for periods between 8 days and 15 years (12), having been born at the conclusion of the sixth month, or on the completion of 180 days, that I deem it unnecessary to pursue the investigation further in an upward direction. I cannot think that all the cases originated in mistake, and although the ideas of the parents in calculating the dates of impregnation may be, as they generally are, erroneous and untrustworthy, yet the idea that *all* the instances in the tabular statement can be deceptive, is inadmissible, especially when the calculations of the mothers were checked by the opinions of intelligent medical men.

We thus arrive, then, at the conclusion, that, under favourable circumstances, *when the child is well developed and healthy*, the period of 180 days may be set down as that at which, AT SOONEST, a child may be born and reared.

But it will be recollected that before this digression, I brought down the interval between consecutive births to 182 days, and the age of the fœtus (*minus* 14) to 168 days, being 12 *days less* than the least possible period at which, according to all experience, a premature child can be reared.

I have only two other instances to quote, and to these I would draw special attention.

William, first Baron Auckland, married Eleanor, second daughter of Sir Gilbert Elliot, Bart., and sister of Gilbert, first Earl of

Minto, by whom he had fourteen children, amongst whom the fourth was the Hon. Caroline, born on the 29th July 1781, who lived 60 years, and the fifth, the Hon. William-Frederick Elliot, who was born on the 19th January 1782, who survived 28 years; this is an interval of 173 days, from which fall to be deducted 14 days as before, leaving 159 days as the period of gestation in this instance, being 21 days less than the point of infantine viability.

Lord Cecil James Gordon, brother to the present Marquess of Huntly, married the eldest daughter of Maurice Crosbie Moore, Esq., County of Tipperary, Ireland, and had a child on the 19th September 1849, Evelyn, and a son, Cecil-Crosbie, on the 24th January 1850, who are alive now; the interval between these two births being only 127 days, which is further reduced to 113, by deducting 14 as formerly, being less by 67 days than the said point of viability.

Of course we have no details here of the weights and measurements of these three last children; these are unnecessary. The previous births are quite sufficient to limit the period of conception far more accurately than the mere notions and feelings of the mother, however these may be verified by the appearances of the offspring; for the latter may vary as widely at an early age, and, speaking comparatively, as they are known to do in respect of children carried to maturity.

I cannot conceive how these last three cases can possibly be explained except by the doctrine of superfœtation. In these and all the other instances adduced, where the interval between the births was less than the natural period of utero-gestation, I have taken it for granted that the second children were premature, although there was no proof whatever that this was really the case. And I have gone on diminishing the period of intro-uterine life necessary for the viability of the child to the lowest possible point, and have only allowed 14 days in my calculations for the womb to be rendered fit again for its procreative functions after delivery; and yet I have adduced one,—of the *three* instances last quoted,—in which the child survived and lived to maturity, whose period of intro-uterine life, unless it be looked on as a case of superfœtation, must have been *less by* 12 *days* than the shortest period it has ever been proved that a prematurely-born child survived; another, less by 6 days, than the only unequivocal case on record, of a viable child having been born so early as the 165th day after impregnation, that of Brouzet; and a third, less by 20 days than the only case on record, of a viable child *thought* to have been born on the 133d day after conception,—but which case has been pronounced by universal consent to be fallacious, viz., that of Rodman of Paisley,—and by 67 *days* less than the earliest point of infantine viability. In my argument, I think I have, by concession, disarmed controversy. Aware of the weakness of uncertain premises, I have retreated step by step before such objections as I thought might be reasonably urged against the doctrine in which I believe, and which I sought to prove, rather than do battle on physiological grounds, well knowing that if the doctrine be first made sure by *facts*, much trouble and many words will thereby be rendered unnecessary.

In support of this doctrine, which I have taken rather an out-of-the-way method to prove, yet a method which I think cannot be fallacious, I would, in conclusion, and for the sake of completeness, adduce such other cases of superfœtation as I have met with, and sift such objections as have been raised against them.

In Velpeau's Elements of Midwifery there is a case related of a Madame Begaud who, on the 30th April 1748, gave birth to a living male child, "small and delicate," but which survived for 2½ months, and on the 17th September following, was delivered of another child, "living and lively," which lived twelve months.

The only comment I have met with on this case is in a small paragraph in a footnote of Campbell's Midwifery,[1] where he attempts to explain it away by saying that, "admitting that the case is correctly stated, the first child must have been born about the middle of the *sixth* month, and the birth of the second protracted about two months beyond the usual term," *i.e.*, a pregnancy of 343 days! When one has a purpose to serve, or a theory to support, it is wonderful how many inconsistencies creep into his views. The same author, in his chapter on the "Duration of Pregnancy," says "the term (of gestation) is by no means restricted to nine kalendar months; on the contrary, it may be exceeded by a week or *a month*." "I have for several years," he says, "devoted much attention to the subject. In four cases where the evidence was clear, in one of them pregnancy was protracted *eleven days*, in a second *thirteen*, and in a third *eighteen* days." And yet, to suit his theory, he extends the period in Begaud's case to sixty days!

In many authors numerous fabulous stories are detailed of instances of pregnancy twelve, fourteen, sixteen months, up to three years, of which last a case is gravely detailed by Petit in his "Collection of Instances relative to the Question of Protracted Gestation," published at Amsterdam, 1766; but the opinion of all modern jurists is, that 293 is the *ultimum tempus* to which human gestation has yet been beyond doubt ascertained to extend. The Code Napoleon allows 300 days; the Prussian law 302 days. A case has recently been decided in the United States, and recorded in the American Journal of Medical Science for October 1845, in which gestation was said to be protracted to 317 days. Professor Simpson, in the Edinburgh Monthly Journal for July 1853, records four cases occurring in his own experience, in two of which every fallacy calculated to mislead in forming a correct estimate of the actual duration of pregnancy was avoided, and in these labour came on, in the one case, 336, and in the other, 319 days after the last catamenial discharge; and allowing 23 days as the average time between the cessation of one catamenial period and the commencement of another, at which time impregnation may have taken place, the actual minimum period of utero-gestation in these instances would be 313 and 296 days respectively. But these fall far short of the term to which Campbell refers in his untenable explanation of the above case.

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[1] Campbell's Mid., p. 95, footnote.

In the Recueil de la Société d'Emulation, there is a case cited by Churchill, of a woman of Strasburg, æt. 37, who was delivered of a lively child on the 30th April.[1] The lochia and milk were soon suppressed. On the 17th September of the same year, about four and a half months after the first delivery, she brought forth a second apparently mature and healthy child. It is stated that after death the uterus was found single.

The following case occurred to Madame Boivin, accoucheuse in the Maternity at Paris, and is related by Cassan.[2] "On the 15th of March 1810, a woman aged 40 gave birth to a female infant weighing about four pounds. As the abdomen still remained bulky, Madame Boivin introduced her hand, but could find nothing in the uterus.[3] But her examination led her to suspect that there was another fœtus, either extra-uterine, or contained in a second cavity in the womb. At length, on the 12th May, a second female infant was born, weighing not more than three pounds, feeble, and scarcely able to respire." The mother assured Madam Boivin that she had had no connexion with her husband (from whom she had been some time separated) except thrice in two months, viz., on the 15th and 20th July, and on the 16th September 1809; but as this case is explainable by concluding that the woman had a double womb, I shall pass it over without comment, merely remarking, that Caspar (iii. 336) refuses to admit it either as a double uterus case or one of superfœtation, but explains it as a common twin case, the date of conception being on the 16th September, and the birth of the first twin being the 15th of March, and the second on the 12th of May, being six and eight months children respectively; seemingly forgetful of his own tables, which set down the weight of a six-months fœtus at from one pound and a half to one and three-quarters, while the first born child in the instance before us weighed four pounds, whereas the second, which, according to Caspar, was born in the eighth month, and ought to have weighed five pounds, was only three.

In the Gazette Medico-Chirurgicale[4] it is recorded that C. F. L., æt. 32, tall, thin, and well formed, had never born any children. She had regularly menstruated up to the month of June 1845, when the menses were suppressed. In August they returned twice at a fortnight's interval—a circumstance which removed all idea of pregnancy. Vomiting and sickness appeared, with other more positive signs, which caused her medical attendant to return to his former opinion of her state. On the 28th of February she was delivered, after a slow labour, protracted by premature rupture of the membranes, of a full-grown but stillborn female child. Nine hours after accouchement she experienced the sensation of some substance descending through the vagina. The midwife, summoned in haste, found a second fœtus, aged four months and a half or five months, furnished with a cord, placenta, and a complete set of membranes. This was also a female fœtus.

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[1] Quoted in Edin. Med. Journal, iii. 539.

[2] Ibid.

[3] Caspar, iii. 376.

[4] Medical Times, vol. xiv. p. 206. June 1846.

There is a case of superfœtation recorded in the Dublin Quarterly Journal, Feb. 1859, p. 221, to which I can only refer, as I have not a convenient opportunity for procuring the details.

It is reported in the Lancet, August 2, 1862, that at the Obstetrical Society of London, Mr Langmore exhibited a supposed twin abortion, with the following history.[1] A lady aborted on May 22d; a fœtus of about four months gestation was expelled. It was flattened, more or less atrophied, and had been dead some time. The placenta was removed, and afterwards a smooth soft body was peeled off the upper part of the uterine cavity, which proved to be a second bag of membranes. The chorion and amnion were unruptured, healthy, and transparent, and through them an embryo of about five or six weeks could be plainly seen floating in clear liquor amnii. The embryo appeared fresh and perfect, and not at all atrophied. Was this an instance of twin pregnancy or of superfœtation?

Drs Harley and Tanner were directed by the Society to investigate the question. Their report concludes as follows:—"We are led to assume that the case under consideration is an example of superfœtation for this reason; if the second healthy six weeks ovum were the product of the same conception as the first four months fœtus, which had been dead some time when expelled, and manifested symptoms of putrefaction (decay?), yet the small second ovum died when six weeks old, was retained for about ten weeks afterwards, and, nevertheless, when removed, was perfectly healthy, and did not present any trace of decomposition (decay?)." The editors of the Year Book of the Sydenham Society for 1862, from which this last case is extracted, conclude their notice of the case as follows: "As theoretically we see no physical obstacle to the occurrence of superfœtation during the first three months of pregnancy, so we think the specimen now reported upon proves, as far as anything of the sort can prove, that superfœtation is a positive fact."

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[1] Year Book, Syd. Soc. 1862, p. 322.

ARTICLE III.—*Typhoid Fever in Penicuik.* By GEORGE THIN, M.D.

FOR many years a disease has prevailed in Penicuik, during the autumnal months, which has been designated by the resident medical men and the inhabitants generally as typhoid, and, more commonly, gastric fever. As this is a disease about the etiology of which very different opinions are entertained, and as it is of much public importance that sanitary questions should cease as soon as possible to be matters of controversy, I have very willingly assented to a request made to me, whilst on the eve of leaving this country, to record any facts I have observed that seem to bear on the subject.

My observation of disease in Penicuik dates from 1859 till October 1864, and, with the single exception of the past summer, I have each year seen more or less of endemic fever. In 1861 and 1863, the cases were so numerous that a very considerable alarm was created in the minds of the community.

All the cases I have seen were instances of the fever so variously known as typhoid fever, enteric typhus, pythogenic fever, etc. There was always more or less gastro-intestinal irritation evidenced by tympanitis, characteristic diarrhœa, cæcal tenderness, and in several cases (only one of which has in my experience proved fatal) by intestinal hæmorrhage.

During the years 1857 and 1858, I was conversant with typhus as it is seen in Edinburgh. So far as I know, there has been no case of that disease in Penicuik during my residence there.

Dr W. T. Gairdner and Dr Warburton Begbie, who had frequently charge of patients sent by me to the Edinburgh Infirmary, are in a position to testify to the identity of fever as it exists in Penicuik with typhoid fever.

I took no systematic notes of the cases while under treatment, and am not therefore in a position to give the histories of individual patients; moreover, any such detailed account would be useless for the object of this paper, which is not to elucidate the pathology or treatment, but the etiology of the disease.

The village of Penicuik lies ten miles south from Edinburgh, at the foot of the Pentland Hills, in the valley of the North Esk, at a very considerable height above the level of the sea.

The air is cold and bracing, and the climate generally is well fitted to produce vigorous health. Phthisis is strikingly rare, in comparison with any other town or village of Scotland with which I am acquainted. On the other hand, neuralgic, rheumatic, and other complaints very amenable to treatment by quinine, are at some seasons excessively common, and are due, I think, to malaria generated in a large morass that stretches for seven miles to the south-west.

The village, to which my remarks exclusively apply, contains about 1800 inhabitants. It consists of two well-defined sections—

the village proper, and several irregular blocks of cottages and dwelling-houses adjoining the paper-mills, known as Valleyfield. The village may be roughly sketched as being composed of the High Street, S, lying east and west, and two smaller streets *a* and *b*, joining S at right angles, at about a third part from either extremity, each stretching off in opposite directions.

The left perpendicular *a* represents the thoroughfare from Valleyfield which lies southward, the opposite and right perpendicular *b* points northward towards Edinburgh. Any one, drawing with a pencil the straight lines as I have indicated them, will be able to follow my description without difficulty; and as my present purpose chiefly consists in showing why some parts of Penicuik suffered from fever while others escaped, it is quite necessary to bear in mind the general topographical arrangement.

The last epidemic I observed in Penicuik occurred during the summer and autumn of 1863. As the facts connected with it are still fresh in my memory, I have selected it as the subject of the following narrative.

All the cases were treated by Mr Fauchie and myself. During the months of July, August, and September, I had upwards of sixty fever patients under my own care, and as these pretty accurately represent the whole in all important particulars, especially as regards locality, my subsequent remarks apply to them exclusively.

The first case that occurred was that of a boy aged nine years, who lived in a cottage situated in the outskirts of Valleyfield, in a family of characteristically clean and orderly habits. He took ill in the beginning of April, and died in June. No member of the family had been away from home, nor had any visiter been living in the house; in short, no trace of possible infection could be discovered. The cottage is provided with neither privy nor water-closet.

During his illness, his sheets and bedgowns were washed in a cottage situated in the middle of Valleyfield, tenanted by a family of four, the husband, his wife, and two children, and the washing necessarily took place in the single room occupied by them all. The husband took to his bed ill of fever on the 24th of May, followed within a fortnight by his wife and both his children.

During June, and in the beginning of July, several cases occurred in the immediate vicinity of the cottage last mentioned, but up to the third week of July, there was, as far as I know, no other case in any other district of Penicuik.

For several weeks there had been no rain. In the end of July the weather became unusually hot and close, and within a few days as many as ten or twelve persons took ill in three new localities. Separated from the cottages previously alluded to by the North Esk, is another district of Valleyfield, consisting of a single row of two-storied houses, and about sixty yards farther down the stream is a large two-storied block of dwelling-houses tenanted by four families. Fever appeared simultaneously in both, and in a few weeks many members of the respective families became infected. Of the four families last mentioned, three were ravaged by a very severe type of the disease.

At the same time, several cases occurred in the village, all of them being in houses situated at one point of the street *a*, which I have spoken of as leading from Valleyfield into the main street of Penicuik, but at the extremity farthest removed from Valleyfield. It is instructive to point out how very localized the contagious influence was that produced these.

As the street *a* of which I am speaking approaches within eighty yards of the High Street S, it diverges on the right-hand side at right angles to the extent occupied by two double cottages. A narrow lane, leading to a joiner's workshop and two cottages concealed from the view of the street, separates the last of the double cottages from four two-storied houses which take up and continue the original line of the street. Two workmen went home ill from the workshop, six cases occurred in the two cottages hidden from the view of the street, one in the double cottage next the corner of the lane; and in each of the two-storied houses, leading from the other corner to the High Street, one or more families were affected. Of these cases the most virulent were those residing near the lane (including the two joiners). Three of them died: a young man of twenty; a girl of eighteen; and a young woman of twenty-three, who, after having apparently recovered, sank from a sudden attack of peritonitis, originating in the ilio-cæcal region.

Six cases occurred on the left side of the street *b*, which strikes off the High Street S at right angles, in the opposite direction from *a*. These were of minor importance, and were distinguished by two points of difference. Each case occurred in a family of which none of the other members became affected; and, secondly, though in two instances the illness was protracted, in none of them did the danger especially consist in the severity of the bowel symptoms.

I shall now enumerate the parts of the village in which no cases occurred.

The High Street on both sides and one side of the street *b*, were, so far as I know, completely exempt. If there were any cases, they must have been few and unimportant. On the left side of the street *a*, the only case that came within my knowledge was that of a joiner previously mentioned, who obviously caught the disease simultaneously with a fellow-workman on the other and affected side of the street. I am within the mark when I state, that those districts which were free from fever comprise three-fourths of the total population of Penicuik and Valleyfield.

The epidemics of previous years which I have witnessed had also certain centres of virulence from which the poison seemed to radiate, the position of which, however, did not at all correspond with those of the epidemic of 1863. I have thus two things to account for: first, why Penicuik should be notorious for fever at all; and, secondly, the varying conditions that regulate its local distribution. I shall consider these two questions in their order.

I have previously stated that I consider the natural climate of Penicuik to be, with a trifling exception, salubrious. The inhabitants are well and constantly employed, and are consequently comfortably housed, fed, and clad. In general comfort, indeed, they contrast favourably with those of any other Scottish village with which I am acquainted. Such being the case, we are compelled to look for the predisposing causes of disease in the adventitious circumstances specially incident to the conditions of life affecting them.

Avoiding any statement open to controversy, I take it for granted that no intelligent physician will dispute, that one of the ascertained causes concurring in the production of many cases of typhoid fever is exposure to the effluvia of decomposing fæces. Any one who has read the mass of evidence collected on this point by Dr Murchison and Dr Budd would probably grant more than this, but so much as this even Dr Christison grants. In the address delivered by him at the meeting of the Social Science Association in 1863, after exhausting the arguments that can be urged against the connexion between sewer gases and fever, he says, "foul air merely favours its invasion." Why, that is as much as any one ever can say of a supposed cause of an unknown impalpable blood-poison. We all know that other conditions than that of "foul air" are also necessary, but whoever will take the trouble to examine the evidence of facts will find that to be one of the most constant.

In nearly every village of Scotland there is scarcely a cottage which has not attached to it a potato garden, in some corner of which (beside the pigstye, if there is one) is the dunghill or "midden." All the filth of the family is here accumulated for the benefit of the next year's crop, and generally becoming incorporated with earth and ashes, is thus rendered harmless to health. This primitive and natural state of things has long ceased to be the rule in Penicuik. The success of a large paper manufactory has so conduced to the prosperity of the village, that land became too valuable to admit of the old cottage system; large houses of two and three stories, with several families in each flat, have been built to accommodate the increased population. Only one public privy exists in the village; and I speak to what is strictly within my own knowledge when I say, that the private privies have been too few in number, and generally kept in a filthy condition. A pail, with the accumulated excrements of the females and children during twenty-four hours, is a utensil in daily use in very many families.

I believe this state of matters to have been the determining cause of many of the epidemics of fever in Penicuik.

That the point at which this fever breaks out is not determined by the permanent qualities of any particular soil or situation is well illustrated by the topographical description I have given above. During the previous years of my experience, Valleyfield had been singularly free, while the street *b*, which in 1863 had only a few solitary cases on one side, in former years deservedly bore the reputation of being the chief centre of fever. The first, and for six weeks the only case, occurred, as I have previously stated, on the outskirts of Valleyfield. Why the disease should have originated in that particular cottage I am unable to say. It was situated, as regards cleanliness, etc., neither better nor worse than those adjacent. It can be only attributable to a high degree of that susceptibility in the patient which must exist in every case before the poison can take effect.

The second I consider to be a clear case of infection by fomites. All the fæcal discharge from the first patient was emptied in a wood opposite the cottage, and quite out of the way of the second family. Between the first patient's house and that in which his clothes were washed there are two rows of cottages, in none of which there was as yet any fever, so that it is not difficult to believe that the disease was thus conveyed.

The next cases that occurred were numerous and simultaneous in their appearance, and can, I think, be satisfactorily accounted for.

No amount of fæcal accumulation will produce fever without the co-operation of certain conditions of the atmosphere. What these conditions may be I do not pretend to say, but heat and stagnation are certainly parts of them. There is no reason, for instance, to believe that the sanitary arrangements of Penicuik are any better this year than they were last year, and yet, up to the time I left in October, there had not been one case of fever in the village. This is easily attributed to the weather, which, although dry there as elsewhere, was never, with the exception of three days in May, close or sultry; strikingly contrasting, in this respect, with the stagnant heat that preceded the outbreak in July 1863.

It was immediately after the heat that the cases occurred. The cottages were badly accommodated as to privies, and I ascertained, by personal inquiry, that the excreta of the adult women and of young children were thrown at night into the bed of the Esk. During the months of June and July, the volume of water in the river was so small that it was all carried away by the mill-lade at a point higher up, the bed of the river presenting simply a succession of shallow, stagnant pools. The fæcal matter not being carried away by the stream, became decomposed by the heat of the sun, and the resulting effluvia must have been inhaled more or less by all the residents of the cottages.

The square block of buildings which I have described as being about sixty yards farther down the stream, and in which three out of the four families inhabiting it suffered severely, is provided with water-closets. These discharge by a single pipe into the bed of the river immediately in front of the dwellings. At this point also the river was stagnant.

A very similar case to the above is recorded by Dr Ogier Ward, as having occurred at Birmingham in 1838. A small stream, used by those living in its neighbourhood as a common sewer, had dried up. About fifty persons took fever in consequence.

I believe that typhoid fever is contagious, but its course at the time of which I am speaking was due to causes of quite a different nature. There was nothing in the way of personal communication which could in any way explain its being localized in special centres. The cottages in Valleyfield had daily communication with the shops in Penicuik, and a great number of the workers in the Valleyfield paper-mills live in Penicuik, and pass and repass several times daily, but this intercourse did not affect the distribution of the disease. It is worthy of remark, that though the mills are in the immediate vicinity of the infected houses, the workers who lived in other than the localities indicated, as a rule escaped.

The source of the poison that produced the cases in the street marked *a* was not far to seek. Immediately behind the last of the two-storied houses, several horses, cows, and pigs are kept in a very limited space; and connected with these and some large families of human beings inhabiting the house, there was a constant accumulation of filth. The liquid part of this, after being conveyed in a close drain for a few yards, flows in a small open ditch through a piece of sloping boggy ground, in order to be conveyed to the river. The smell from this drain was abominable. During the summer, I heard many complaints on that score from those living near it. It is situated immediately below the fronts of the two concealed cottages; the workshop and the two-storied houses open to it from behind, and all the inhabitants of these houses were necessarily exposed to its effluvia. It is necessary to add, that there was no possibility of the cases in *a* being produced by the sewage from the cases in Valleyfield, as there is no link by which to trace any connexion between the two centres of disease. The cases in *a* occurred at the extremity farthest removed from Valleyfield. Besides, as I have before remarked, the cases began simultaneously in both.

I have said that six cases occurred, in six different houses, in the street spoken of as *b*. In three of the houses I could discover nothing special in the drainage. It was different with the other three. Two of them were contiguous, and between them there was an offensive drain. This was immediately rectified, and no more cases followed at that point.

The remaining case is interesting. I had been in long attendance on the mother of the family for an ailment of a chronic nature. Every time I entered the house, I perceived a very offensive odour proceeding from the window sink, the drain from which was very short, and had no communication with the drainage of any other house, and I repeatedly prophesied that if it was not rectified it would produce fever. The odour was not fæcal, and no fæces had ever been thrown down. Shortly afterwards one of the children took fever.

The offending sink remained in use, and within two months the only two children in the house took diphtheria,—the first cases of that disease in Penicuik for at least twenty years.

I think the facts I have just briefly related afford useful evidence to any one anxious to form an opinion on the disputed points connected with this fever. These points, as far as I know, relate chiefly to its origin and contagious properties. The controversy in regard to the first of these has, I think, been chiefly due to different observers attaching too much importance each to the facts of the cases specially observed by himself, and on the strength of these proceeding to too hasty a generalization. Whether or not there be a ponderable but impalpable essence which must in every case of typhoid fever be present as the proximate cause, I think we can never know; but even granting the existence of such an agent, there is no evidence to show that the secondary causes co-operating to its origin are uniform.

That certain effects of atmospheric agency on decomposing sewage have, in many instances, been the chief factors in the production of this fever has been proved, over and over again, on circumstantial evidence as conclusive as any ever brought before a jury. But we all know that identical conditions of filth and weather existed in Scotland for years without a case of this fever ever being heard of. Clearly, then, there must be present an unknown something, a "change of the type of disease" if you like, though the phrase is apt to mislead. It is very important to bear in mind this *tertium quid*, and bring it prominently forward, if we wish to carry conviction in pressing sanitary reforms. Dr Murchison, Dr Budd, and other writers, have done an immense service in showing that fæcal accumulations can in certain conditions produce fever; but by exalting the relative importance due to these theoretically, inquirers of a speculative turn of mind have rebounded into the mischievous extreme of denying that they have any specific action at all. Dr Christison, in the address alluded to above, says, "I suspect, then, it must be allowed of this disease, as in respect to most other epidemic diseases, that we do not yet know its cause—that foul air merely favours its invasion; but that its true cause is something more specific—some ζύμη or ferment which has hitherto eluded our search. In that case, while we shall do well to encourage better drains, more and better water-closets, and better ventilation of dwellings, still we must not count upon thus extirpating enteric fever."

The unimportant part which Dr Christison here assigns to defective drainage is in direct variance with facts open to examination by any one who chooses to take the trouble. It might be said, with almost equal force, that the true cause of ague is some ζύμη or ferment, that fens and marshes merely favour its invasion, and that, drain and cultivate them as we may, we must not count upon thus extirpating it.

In classifying typhoid fever with other epidemics, Dr Christison is guilty of a very palpable fallacy. There are no epidemic diseases common in this country which have the same evidence to connect them with special local conditions. It does not at all affect the question to point to individual instances of the disease occurring in houses whose ventilation and drainage are perfect. These instances, exceptional even in number, do not in any way invalidate conclusions drawn from a large number of facts, and may easily be explained on the ground of unusual susceptibility on the part of the patient, who may have been exposed to some other source of the poison without his knowledge. To found any argument on a few solitary cases like those adduced, would be as reasonable as it would be to deny the connexion between tetanus and wounds, because a few cases of tetanus had occurred in which no wound could be observed.

Dr W. Budd of Bristol has observed and recorded a very interesting series of cases, in which he supposes a number of individuals to have been infected by the alvine discharges from a patient suffering from fever. Reasoning from these, he has advocated the opinion that sewage can produce fever only when it contains discharges from those previously diseased.

In the Lancet, Nov. 9, he assigns two reasons for this belief, both of them very inconclusive. First, he gives instances in which cloacæ, after having existed harmlessly for years, have suddenly become sources of disease, concluding therefrom that they must have received some new noxious element. Dr Budd's views are so materialistic that he forgets to assign any part to the *tertium quid*, of which I have spoken as always co-operating to produce fever. Dr Christison can see nothing else than his ζύμη, but Dr Budd cannot see it at all. This is the real new element which has rendered so many cloacæ fruitful. His second reason is stated thus: "That this fever is caused by something far more specific than the putrescent compounds which exhale from common sewage, seems to be further plain from the significant, not to say decisive fact, that of the two kinds of fever, the intestinal is the only one that originates in sewer influence."

The only fact that that shows is simply what is stated, that typhoid fever has a relation to sewage which typhus has not. It is not even the shadow of a reason for or against the point at issue. It would be easy to multiply instances which are quite inexplicable by Dr Budd's theory. Of these the following is an example :—During the summer of 1862, only two cases of fever came under my care. The first of the two was that of a young married lady, who occupied a house from which I had removed the preceding May. There was a bedroom in the ground-flat of the house, separated from the water-closet by a stone wall. Some defect in the construction of the sewer permitted a communication with the flooring of the bedroom. The consequence was a decided odour, which in damp weather became very strong. For this reason I had, while living in the house, caused the furniture to be removed from this room, and kept it shut up. I warned my successors of the danger of using it, but, on account of its other conveniences, my remonstrances were disregarded. In six weeks the lady took typhoid fever, and swam for her life through a long illness.

No possible source of infection could be traced. No others of the family took ill, the greatest precautions being afterwards used to avoid the source of danger. The house was a new one, and as I happen to know all the tenants who have occupied it, I am able to state that there had never been, up till that time, a case of fever in it. When this lady took ill there was no case of fever in or near Penicuik.

I shall relate one other instance. In 1863, simultaneously with the outbreak of fever in Penicuik, a number of cases occurred in the Wellington Reformatory Institution, situated on rising ground about two miles south from Penicuik. At that time the buildings consisted of an old two-storied house in the centre, inhabited by the superintendent and his family, and a new building joining it at right angles, in which were two dormitories for about ninety boys. Adjoining, and in a line with the other gable, was a cottage in which the grieve or farm superintendent and his family dwelt. The first case that occurred was in the grieve's family. His daughter, a girl of sixteen, was a domestic servant in a family in Penicuik, but visited her father's house regularly. She was quite out of the way of any infection while in Penicuik. While on one of her visits to her father she took ill. *At the same time* several of the boys belonging to the Institution began to droop; and within six days more, sixteen of the boys, two others of the grieve's family, the superintendent's wife, and two children, had taken ill. There were no deaths.

There were two distinct sources of disease, each of which was sufficient to account for the outbreak. The superintendent's wife informed me after her recovery that she had been for months conscious of a bad smell about the house, which was always worst in damp weather. Accordingly, the flooring was lifted, and it was discovered that in and around the house there were old cesspools which had been full for a period of many years during which the house had been uninhabited.

The other source was the cesspools belonging to the boys' privies. These overflowed into a ditch, between which and the Institution there was only the breadth of the highroad. In passing along this road, the smell in hot weather was quite perceptible. There was no possibility of infection from the fæces of a fever patient in this case.

That alvine discharges from a fever patient should be as active, or even more so, in originating the fever poison as ordinary sewage, it is very reasonable to suppose. But facts are quite against the supposition that they are always or even frequently the source. Were such the case, an epidemic of typhoid fever should become more virulent the longer it lasted, because the source of the poison would daily become more abundant. Whereas, at least in my experience, the contrary is the case: a number of cases always breaking out at the same time, the subsequent cases being less numerous and often milder. Besides, the theory breaks down altogether when applied to the numerous instances in which persons take this fever who have never been in any possible way exposed to such a source. Though the exciting cause of fever is, in very many instances, connected with sewage, I think those who limit it to that are in error. It is highly probable that other kinds of organic matter in a state of decomposition can originate the poison, though not so readily. The case of the child who took fever from the foul air of a window-sink is, I think, in point. Neither urine nor fæces were ever put down, the smell which enabled me to prophesy fever probably arising from the decomposed food which was carried into it by the dish-water.

I think the case of the stinking drain which infected so many families in the street *a* bears in the same way. The filth of which that drain was the bearer consisted chiefly of the excrementitious discharges of cows and pigs. Yet I consider that the probability of that drain being the cause of the fever in its neighbourhood is almost as near certainty as such a thing can be. It was the only distinguishing sanitary condition between the affected houses and others in their immediate vicinity. Compare them, for instance, with those on the other side of the street. The inhabitants drank the same water, bought their provisions in the same shops, and were as well housed and clad.

It was not due to infection, for there was quite as much intercommunication with the opposite side of the street as there was with the houses adjacent. None of them had water-closets, so that it could not be a choked sewer or a poisoned drain. The circumstances tie us down to discover a local cause, and no other than the one I have given can be found at all proportionate to the effect.

Two things seem to me to have a strong analogy to typhoid fever.

Students and others who are much in dissecting rooms are liable, especially if from any cause they become weak, to a slight feverish attack, which generally passes off in a few days. The symptoms are depression of spirits, loss of appetite, a slight amount of fever, and diarrhœa, with considerable flatulence. The gas passed by the bowels is fœtid, and can often be distinguished as having the special odour of the subject which the student has been dissecting. Clearly, in this case, the gaseous results of decomposition, after circulating in the blood, are thrown off by the bowels, exciting general and local irritation in their passage. It is highly probable that typhoid fever owes its local symptoms to the inflammation of the glands, caused by the passage of some similar but more virulent poison.

Epidemic cholera is another instance in which a poison produces local bowel symptoms. The causes of cholera and typhoid fever are identical, the different results being the special influence of climate. The epidemics of cholera, which have ravaged Shanghae for several summers, were first produced by the influx of country people, who fled for protection from the Taepings. They arrived in multitudes which far exceeded the house accommodation of the native part of the settlement. The houses, which they rented by the month, were each packed by about a hundred persons, and when they were turned out at the month's end, the floors were often covered with from one to two feet of excrement.

Excluding cases in which the disease may be conveyed by the patient's stools, the question as to whether it can be communicated by exposure to influences arising from contact or proximity to the persons of the sick, is still undecided. That it is not so contagious as smallpox or typhus is abundantly evident from the way in which it selects certain houses, or sets of houses, and passes over others. That it can originate without contagion can be shown in many instances in which solitary cases of the disease spring up when there is not another within many miles. But that it is infectious in a certain degree, and especially in certain conditions, I firmly believe. The circumstances of the case render this very difficult of proof. It is evident that before any person can be exposed to contagion from the person of a fever patient, he must be for a longer or shorter space of time in the house in which the patient resides. If fever follows the visit, you cannot prove to your opponent that it was a case of infection, for he immediately replies that the same original sanitary defect that produced the first case produced the second. The possibility of this objection being urged prevents my quoting many instances which I believe to have been due to personal contact with the sick, but three cases which seem to me to exclude it are, I think, worthy of being recorded.

The first is the case whose history I have given above; the second that occurred in 1863. The husband, who was the first to take ill in that family, had never been exposed to any source of the disease as far as was known, except the bed-clothes brought by his wife from the cottage in which the boy lay who first took ill. There is a considerable distance between the two cottages, and the stools from the boy were emptied on a dunghill close to the house.

The second case was that of a young man who went several times to see his sweetheart who was lying ill, and caught the disease from her. She had been sent home from her master's house, where she had been seized, along with one of his children. As there were neither privy nor watercloset in the cottage in which she lived, and as there was no fever where the young man lodged, I have no doubt that the infection which seized him was due to the ordinary exhalations from her person. His case was slight.

The third instance which I shall give is a very singular one.

In 1861, there lived near Penicuik, in a large new house, with perfect drainage, a boy aged about 12 years, whom, for the sake of reference, I shall name A. A. had a cousin of about the same age, who had come from Edinburgh to spend a few days. The children of an officer in Greenlaw Barracks, which are near the house, were sent to spend the day with A. and his cousin. Three soldiers were sent to erect a small tent for the amusement of the boys. The following day A. was indisposed, and in two days afterwards took to bed ill of fever, from which he died, after a long illness. A.'s cousin took ill in Edinburgh some days afterwards, and recovered, after passing through a very decided course of the disease. The three soldiers took ill, and one of them died.

There were no previous cases of fever among the soldiers at Greenlaw. The nearest case of fever at the time was in Penicuik, where it was very prevalent. A. had been several times in Penicuik, and the only possible explanation I can give of so many people catching fever at one fortuitous interview is, that A., who was the first to take ill, had the infection about his person. The turf on which the tent was pitched was removed from the influence of drainage of any sort.

A young lady who called at the house the first day that A. was in bed, finding no one at home at the time, spent an hour in his bedroom. She took ill of fever that day week. There was nothing wrong with the drainage of the house in which she lived.

These and similar cases have confirmed me in the belief that whether we are quite certain of the personal contagion of this disease or not, the only safety, practically, consists in acting rigidly as if we had decided in the affirmative. I think it highly probable, that persons living in conditions which favour the access of this fever, are much more apt than others to become infected by contagion, while, conversely, those living in a pure air, while more able to resist contagion, are more amenable to the influence of the poisonous gases. This opinion is, I grant, very speculative, and I apply it exclusively to young people, who are the most frequent subjects of the disease.

It seems to me to be certain, that typhoid fever is the product of several very distinct causes.

First, there must be a susceptibility on the part of the patient. The way some persons withstand constant exposure to the disease, without taking it once, shows this very clearly. This susceptibility scarcely exists after the age of thirty.

Secondly, a cause which is most tangible, and which has therefore been most talked about, is the emanations from decomposing organic matter, nearly always sewage. This has been shown over and over again. To deny it shows either ignorance or prejudice.

Thirdly, heat and stagnation of the atmosphere nearly always exist, though not necessarily.

Fourthly, a thing or things unknown, probably due to a condition of the atmosphere. To this cause must be attributed the varying cycles of the disease, the preceding conditions being much more constant than the prevalence of fever has been.

It would be interesting, but fruitless, to speculate as to how many of these causes must co-operate in its production; whether, if some of them exist in a high degree of force, a less degree of the other or others suffices; or whether, in a still more exaggerated state of energy of one or more of them, the others may, in exceptional cases, be absent. It will be productive of more benefit if medical men will set themselves to the removal of the source of disease which is within their power—filth in all its forms.

ARTICLE IV.—*Rickets and its Pathology Clinically Considered.* By WILLIAM STEPHENSON, M.D., F.R.C.S. Ed.; Extra Physician to the Royal Hospital for Sick Children, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 16*th November* 1864.)

RICKETS is a subject of which no special notice is taken by West, Churchill, or Meigs, in their works on diseases of children; yet it is a disease more peculiarly limited than any other to the period of childhood. Our authors on the practice of medicine are silent on the subject, yet it is as purely a constitutional disease as tuberculosis. The only niche which has been allotted to it is in our books on surgery, where it is classed among diseases of bone,—a position as erroneous as would diseases of the joints be for rheumatism. The scanty notice which is there taken of it is wholly inadequate to enable our students to form any proper idea of the malady, leaving as they do most of the characteristic constitutional symptoms unnoticed. An accurate description of rickets is not to be found in any of our text-books. It is not to be wondered then, that it should hold the anomalous position we have here indicated, and that many erroneous ideas regarding it should be prevalent in the profession. Dr Schœpf Merei,[1] in his inquiries as to its frequency in different parts of the country, collected the opinions of a large number of practitioners. From these he informs us that rickets is of *rare occurrence in Scotland.* Now, if there has been one fact more than another impressed upon my mind by my experience in the Hospital for Sick Children, it is that rickets, as a

[1] Disorders of Infantile Development and Rickets. London, 1855.

constitutional disease, is more frequent than any other among the poor of this city.

Again, with regard to the pathology, progress has been retarded by the neglect shown to clinical observation of the disease as a whole. The remarkable lesion of the osseous structure was too promising a field to be overlooked. Our literature is accordingly rich in discussions upon the histological changes that take place. But the morbid anatomy of rickets being stated, nearly all has been said which is known regarding its pathology. Numerous hypotheses have been started to account for this remarkable lesion, but they have all this in common, that the osseous change has been separated from the other symptoms, while the microscope and crucible have been too exclusively looked to for information. I wish, therefore, to leave the beaten track, and by a careful *clinical* survey of the disease, endeavour to obtain a clear idea of what are the symptoms which truly belong to the rachitic diathesis, and inquire whether experimental evidence will not enable us to understand many of its complex phenomena.

For a careful clinical examination of rickets the profession is greatly indebted to Dr Jenner of London, who, in a series of lectures, has given the result of his observations.[1] To him I am indebted for much of the information I possess regarding the disease, my experience having but confirmed the facts and opinions there stated. Without the aid of his vast experience, I could not have arrived at the opinions I now hold, nor been able to express them so confidently.

The following history of a family may form a text for my remarks :—

James R., aged eleven months. Mother states that he was a fine healthy child till about eight months old, when he gradually became delicate, and his flesh soft. He was weaned at ten months, after which he got gradually worse. He seemed to suffer much from teething; lost all strength, so that he could not sit up. Cried much, and often seemed pained to the touch. From being a lively child, he now seemed afraid when he was approached. For some days before I was called in, he had refused to take his food, but would drink constantly; his bowels were very loose, with a strong, rotten smell; his skin was hot; he scarcely slept at all, and was constantly crying. Upon further inquiry, his mother stated that he had always perspired greatly about the head when he slept—"the pillow is soaked in the morning"—and that he could not bear to be covered with the clothes; she had constantly to watch him to keep him covered. He had never attempted to stand or walk.

General Appearance.—He is rather a small child, pale, and of a dull complexion; eyes large and bright. When held in the arms the back is bent, and he stoops forward in sitting. Head of moderate size; anterior fontanelle large; bones otherwise firm. There is a distinct enlargement of the bones at the wrist; legs straight; slight lateral compression of chest; sternum and cartilages prominent and rounded throughout the whole extent.

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[1] Medical Times and Gazette, vol. i. 1860.

Family History.—The father is a strong and healthy blacksmith. The mother of an average constitution, though evidently weakened by having had her children rather rapidly, and almost constant nursing. Has had five children, and three miscarriages. The eldest, a girl, now 9 years of age; the next, a girl, died when 2 years old; a boy died at 5 months, of bronchitis; Esther, now 3 years; and James, 11 months old. All these children have been very delicate; they all have sweated greatly about the head during sleep, and all along have preferred to lie almost naked, even in cold weather.

Margaret, the eldest, was extremely delicate, and was affected in the same manner as the youngest child. She could not walk till she was four years of age, and her legs were then very much bent. She is now, however, as strong and healthy as a mother could wish; her legs are perfectly straight, and she is exceedingly powerful. At three years she could only say a few words, and though intelligent in her look, is still very dull at her lessons.

The girl who died was also very delicate, and was double-jointed at the wrists; her chest was very narrow. She died from the effects of a severe burn.

Esther, now three years of age, was not so feeble as the rest; she is a strong, active child, but her legs are so much bent that she waddles about when she walks. She has convergent squint of one eye.

These two elder children still get very hot and feverish in the skin at night, the faces flushed, and the head moist with sweat. There is a slight flattening over the anterior fontanelles, indicating late closure. They cut their front teeth about the usual age, but suffered much, and were long in getting the back teeth.

Here we have a very characteristic history of rickets. Taking first the symptoms, which were more immediately the cause of my being called to see the youngest child, viz., diarrhœa, great restlessness, crying, thirst, heat of skin, and refusal of food, we have in them a picture which is daily met with among children at this period of life. Tell the mother that the child is teething, and she is at once satisfied. These attacks are often induced by dentition, but in every such case we look upon it as the exciting cause only, there being always a primary one, which in this case we shall find to be the rachitic constitution. Children suffering from this constitutional state are not only late in cutting their teeth, but also are apt to suffer much during the process. Mothers frequently look far too slightingly upon these derangements, and often congratulate themselves that "it is only the teeth." Far otherwise should they be regarded. They are, as we shall see, the indications of the morbid tendency of the constitution, and the medical man should make them his study, and learn to take from them a warning, as diligently as the seaman watches the face of the heavens, and the indications of his barometer. Mothers will come and say, "I don't know what has come over my child, he was always strong and healthy; but these last few days he won't sit up, he'll moan all day, and won't take his food; his bowels are loose, and he is so pained I can't touch him; he cries if you go near him." In such cases, upon inquiry, the other constitutional symptoms of rickets are almost certain to be found. This point gained, other disorders of dentition may be watched for, such as strangury or œdema of the insteps, as pointed out by Marshall Hall, together with laryngismus stridulus, and other convulsive diseases.

Except these occasional derangements, the health of the child, in general, has not yet created any anxiety in the mind of the mother. The child is but suffering from teething, or it has been lately weaned, and will recover. The amount of carelessness or ignorance that is evinced on the part of mothers towards a child's health at this period of life is much to be regretted. I have had children brought to me on account of diarrhœa merely, with their spines bent, their chests compressed, and far advanced in the internal complications of rickets, the mothers of whom were surprised when told that the child was seriously ill. They see the child is not strong, but think it is the teething only.

Most of the symptoms to which I have as yet alluded are common among children, who may, nevertheless, not be rickety. It is of importance, therefore, to inquire whether there are any we can look upon as specially diagnostic of the disease. To return, then, to our patient. In addition to the more immediate ailments from which he was suffering, I found that I at once elicited the surprise and confidence of the mother by inquiring about other symptoms, which had often caused her wonder. The child, from almost its very birth, manifested, while sleeping, a great disinclination to be covered, and whenever he did sleep, perspiration broke out over his head, and "soaked the pillow." Her other children, the mother remembered, were affected in the same manner, and in the two still living, the skin gets hot and feverish, and the face flushed, during sleep. Together with these is another symptom, common during the exacerbations, which ever and anon occur, as in this little fellow. He seems pained when he is touched. The mothers express it, "He is tender all over;" "If I but touch him he cries." The child who once delighted to be played with, and dandled on the knee, evinces the greatest terror on the approach of one who has been in the habit of thus amusing him. These symptoms are of constant occurrence in rickets, and are to be regarded as specially indicative of that constitutional condition which leads to the other manifestations of the disorder.

Some medical men to whom I have spoken on this point, doubt the accuracy of this opinion, and consider that these symptoms occur in other constitutional derangements. I have accordingly made this a point of special inquiry, and I can with confidence confirm Dr Jenner's opinion, that "they at once mark the nature of the disease, render the diagnosis easy, and enable us to predicate that the bone affection will show itself." It must not be forgotten, however, that we may have the general cachexia well established without the bones being *perceptibly* affected, so that if the bone affection alone is to be taken as the mark of the disease, the constitutional condition is very apt to be set down to struma or tuberculosis. Sweating is common to other constitutional derangements, but it supervenes at a later period, and not as in rickets long before the health of the child is much impaired, nor is it of so local a character. The kicking-off of the clothes is also common, but there is a difference between this and the simple restlessness of children. The constant desire to be cool evinced by the subjects of rickets is indicated by the expressions of the mothers, "He cannot endure the clothes on him;" "I have constantly to cover him;" "If she had her will she would never have a stitch on her."

When a child is suffering from these symptoms, there is often great vascular excitement; the carotids are seen visibly to pulsate, and at times even a pulsation in the large veins of the neck can be perceived.

Concerning the alvine secretions, diarrhœa is of frequent occurrence, but one character is always present, viz., a strong, putrescent odour, like that of rotten eggs. It is, moreover, extremely obstinate, and often resists all ordinary astringent mixtures, being checked only by the improvement of the general health.

Retarded development is common in rickets. The teeth are slow in making their appearance, and the interval between each group is often long. The anterior fontanelle is generally large, and late in closing. I must, however, differ from the writers who state craniotabes or soft occiput "to be very common, and to be in general and extreme rickets the first manifestation, so far as the bones themselves are concerned."[1] In the numerous cases I have examined I have failed to detect any such condition of the cranial bones. We require to be on our guard in judging of the state of development of a child with regard to rickets, as I have frequently seen a child affected with the malady, who has cut his teeth early, and whose fontanelle was almost closed at the twelfth month. When, however, a child is late in cutting his teeth, although he may appear healthy, we should be on our guard, and make a thorough examination of the history of the family.

The child in whom these symptoms I have noticed have manifested themselves may present all the plump and rosy appearance of health up to 9, 12, 15, or even 20 months. But gradually he begins to fall off. His temper changes, his appetite gets capricious, and thirst becomes constant. He may have been beginning to stand alone, or may have been running about, but he "falls off his legs," and will not go off the knee. He ceases to care for his toys, and soon inclines to lie constantly in bed. His face is now pale; his muscles become soft, though there may be but little emaciation, and soon the mother discovers that he cannot raise himself, or turn in bed. When taken on the knee, the back refuses to support its weight—he sits "all in a lump"—and if the back is examined, the lower dorsal vertebræ are found to be arched and prominent. The abdomen is large; the chest is compressed laterally, and the sternum projecting; the wrists are thickened; the face has a dull, weary look, with a heaviness and fulness of the lower portion; the anterior fontanelle is open; and the head somewhat large and angular.

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[1] British and Foreign Medico-Chirurgical Review. July 1856.

I have now traced the disease up to its full manifestation in the constitution. Into the question of the lesion of the osseous structure I shall not enter. Concerning its histology, personal observation gives me nothing to offer. The general character of the lesion, moreover, is sufficiently known for the purposes of this paper. Had space permitted, I should have made some remarks as to the various manifestations of the disease in the skeleton, and the numerous complications which accompany the malady; but this part of the subject will be better reserved for another paper.

Pathology.—In the family whose history I have given, all the members have been affected with rickets. This points directly to a hereditary element. I do not mean by this that the disease is transmissible from parent to child. We have as yet no facts to support such a view. But that there is a morbid tendency in the constitution derived from the mother, is evident from its occurrence in some cases in all the members of a family, and from the general rule, to which there are but few exceptions, that where one rickety child is born, though the previous children may have been healthy, the subsequent children will be rickety also. Such being the case, this should naturally form the starting point in our inquiry concerning the pathology of the disease. For the nature of this predisposition being found, we shall have obtained the key-note, which will help us on to the better understanding of the phenomena which follow. Writers on the subject have endeavoured to account for the origin of rickets by the multitudinous anti-hygienic influences to which children are subject; but the fact that all children, alike exposed, do not become rachitic, has led to the belief in a constitutional predisposition. Its nature, however, has not been determined. To this point, then, let us turn. In what does this diathetic condition consist? For an answer to this question we must look to the earliest symptoms which are found to manifest themselves in rachitic children. If we can then discover a cause which will not only account for such symptoms, but will also explain the occurrence of all the other phenomena, we shall be warranted in concluding that that alone is the true origin, and the explanation of the pathology.

The first indications of the rachitic constitution, as we have seen, are vascular excitement and increased temperature of the whole body during sleep. To experimental pathology we can now turn for an explanation of these symptoms, and, probably, they have already recalled to mind the experiments of M. Claude Bernard A summary of the results of his experiments I here subjoin. He has shown—

I. That, through the action of the vaso-motor system, we are able to modify at pleasure the nutritive and vital activity of all the tissues of the living animal.

II. That the bloodvessels are placed under the control of two distinct classes of nerves, the one derived from the sympathetic, producing contraction of their calibre, the other connected with the cerebro-spinal system, presiding over their dilatation.

III. That there are two classes of nerves destined to supply all glands; the one derived from the sympathetic, which presides over the contraction of their vessels and diminishes the flow of their secretions, the other taking their origin in the principal motor branches, which ramify in the immediate vicinity of the glands. The latter class are destined to produce dilatation of the vessels, an abundant flow of aqueous fluid, more or less charged with the ordinary materials which characterize the secretions in each particular case.

IV. That after division of the sympathetic the following phenomena occur:—

1. Dilatation of the vessels, and increased rapidity of the circulation.

2. The interchange between the blood and the tissues is impeded, so that the venous blood retains much of its arterial character.

3. There nevertheless results an increase of temperature.

4. The process of absorption is increased.

5. Muscular irritability is higher than in the physiological state.

6. The brain participates in the general disorder. Its temperature rises, its vascularity increases, general sensibility of all the cerebro-spinal nerves is considerably increased.

7. The secretory functions are deranged.

8. Animals in which large sections of the sympathetic have been divided do not suffer so long as they are kept in good condition. All the functions are performed, and they have even become pregnant and brought forth young. But whenever health is reduced, by starvation or otherwise, inflammation and its consequences occur in those organs withdrawn from the action of the organic nervous system.

Let us now return to the phenomena of rickets, and compare them with those I have just enumerated. The first to manifest itself in the child is increased vascular action, shown by the rapid pulse and the visible pulsation in the carotids. This is accompanied by an increase of temperature of the whole body, especially during sleep, with local sweating,—which also admits of a nervous explanation,—together with a desire to lie cool, indicated by kicking off the clothes, a condition no doubt increased by the general nervous excitability. These symptoms manifest themselves long before any others, and are quite compatible with a healthy performance of all the functions.

The close connexion between these phenomena and those which have been shown to arise from division of the sympathetic, at once draws our attention to that system whose functions we know to be in full activity, and, as it were, predominating at this period of life. It suggests, moreover, that to impairment of its functions we may look for an explanation of that constitutional condition which I am discussing. To the nerves we can trace characteristic peculiarities among the various classes of animals. Animals of the same species differ from one another in this respect. The higher breeds of horses and dogs possess a more irritable, sensitive, and, so to speak, more highly organized nervous system, than the common and hardier classes. There is a marked difference in the nervous susceptibility of the inhabitants of town and country, and between civilized and uncivilized nations. Nay more, in the same individual at one time and at another. Acquired constitutional peculiarities are transmitted to the offspring. We can readily understand, that a mother suffering under impaired nervous functions, as we often see after lactation, will be liable to transmit to her future children a weaker nervous system than that possessed by her previous children,—a system in no way diseased, but it may be less vigorous in its function, extremely sensitive to impressions, and more liable to exhaustion. Some such difference it is that is indicated as existing in and conferring upon certain children their predisposition to rickets.

I have stated that this peculiarity is quite compatible with health. I have frequently met with it in children plump and active in body. But in the best of such cases some indications of retarded development will be found to confirm our opinion. This brings me to the next point in the inquiry. Together with the vascular excitement and increased temperature, we have seen that the changes which take place in the capillary vessels between the blood and the tissues are impaired, that the venous blood retains much of its arterial character. The necessity, moreover, of a proper nervous supply to the tissues, for development, growth, and nutrition, is a point so fully acknowledged, that I need not enter further upon it. In this way the backwardness of the vegetative process in rickets is readily understood.

Other agencies now come into play, and it would seem that they are in a measure *necessary* to produce the other phenomena, just as we have seen that animals in which large sections of the sympathetic nerves have been made, do not suffer until other agencies are brought to bear upon them. The child is subject to innumerable morbid influences. Every tooth it cuts is a new trial to its over-sensitive frame. Its food is often highly unsuitable both in quantity and quality; or in other ways it is exposed to the innumerable anti-hygienic influences which surround it. The standard of health is deteriorated, the secretions become easily deranged, its flesh becomes soft. The nervous system participates in the disorder; the child is cross and excitable; nervous sensibility rises; there is hyperæsthesia, and the mother is thrown into alarm by an attack of laryngismus stridulus, or general convulsions. Or the disease may take another course, and inflammation of the serous membranes set in,—hydrocephalus being of frequent occurrence in rickets. Measles, hooping-cough, or other infantile disease may attack the child, when it is rare indeed to find it escape without some serious inflammatory complication.

Thus the two classes of phenomena run closely parallel one with the other, and will bear the strictest scrutiny.

Our treatment, derived from experience alone, now appears to be founded on truly rational principles. We give cod-liver oil and iron, and improve the hygienic conditions; the nervous sensibility falls, the attacks of laryngismus cease, the muscles regain their tone, secretion is improved, and while with advancing age the tendency to the disease is becoming less, the complications are warded off, and the child recovers its healthy appearance.

Thus far has a careful study of the constitutional symptoms of rickets confirmed us in the theory brought forward. But what now of the bones? To reason from the deranged nutrition in other parts of the body, and conclude that the osseous structure is also likely to partake in the disease is not sufficient. It is a special derangement of structure which alone will answer, and appeal must be made to direct experiment. Fortunately we are enabled to supply this remaining link to the chain. M. Claude Bernard remarks, in his lectures,[1]—"If you tie the nerves entering the nutritive foramina of a bone, you will very soon see the cells of the lamellar structure increase in size, the vessels become more numerous, and all the phenomena of rickets follow in rapid succession. We can even bring about these results in part of a bone, without interfering with the remainder. This experiment has been successfully carried out by M. Schiff of Berne."

I have been unable to find any more detailed notice of these experiments, but reliance can be placed upon two such names as Bernard and Schiff. The fact thus noticed gives an interesting confirmation to the views I have been led to adopt by the clinical survey of the disease. Much still remains to be worked out regarding the subject. But whatever progress may be made, the considerations I have here brought forward warrant us, in the *first* place, in looking to the influence of the vaso-motor system upon the development and nutrition of the body for an explanation of the remarkable symptoms which characterize rickets. *Secondly*, we may conclude that the primary cause of the phenomena lies in an impairment (paresis) of the vaso-motor system of nerves. And, *Thirdly*, that the predisposition to the disease lies in a condition of the nervous system more sensitive, more liable to exhaustion, less robust in its organization. The practical bearing of this view is one of the greatest importance, not only in the treatment of rickets itself, but of any other disease which may supervene.

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[1] Med. Times and Gazette, Jan. 21, 1860.

Rickets, as I have before said, is limited in a remarkable manner to the period of infancy. The general cachexia manifests itself before the end of the second year, and after the fifth year, often before it, the constitutional strength is gradually regained. The eldest girl in the family whose history I have given is a marked illustration of this. The vigour and strength of such children are often the subject of remark. Are we to suppose, then, that this deeply-seated and, as we have seen, inherited peculiarity passes away, and plays no important part in the pathology of after-life? The osseous lesion having hitherto been the only recognised symptom of rickets, and those cases where some deformity has become permanent, alone being recognised in after-life as such, it is evident that the constitutional peculiarity of the vast majority of persons who, in childhood, have manifested rachitic symptoms, has been lost sight of. There is here opened up a new and wide field for future observation. The very nature of the predisposing cause would lead us to expect its influence to be of a permanent character. But the physiological changes which take place after the fifth year of life warn us to look for a change in the pathological indications.

Are there, then, any lesions which are found common amongst rachitic children, which have not yet been ascribed to the rachitic constitution, but which may find an explanation in the view here proposed regarding the peculiarity of the constitution? Dr Schœpf Merei has already pointed out that he has frequently met with enlargement of the thyroid gland and hypertrophy of the tonsils in children who have suffered from rickets.

Concerning the hypertrophy of the latter organs, several examples of which he had an opportunity of examining, he states that the enlargement was not due to any exudation, but consisted chiefly of enlarged bloodvessels: precisely the condition we should have expected, looking upon them in the light of our pathology. I have also met with cases of the same enlargement in rachitic families. The observation of the enlarged thyroid is of interest in relation to a paper which Dr Laycock read before this Society. In it he traced the anæmic exophthalmos and bronchocele to a similar lesion of the vaso-motor system. I have met with one example of that peculiar condition of the eye in a young girl, who, it is highly probable, was to some extent rachitic in childhood, and whose younger brother was distinctly rickety. Before anything can be determined, however, regarding these points, much careful observation requires to be made, and I prefer to stop rather than enter on the hypothetical. Enough is already established, however, to indicate that many important truths may yet reward research in this direction. The belief in the influence of the vaso-motor system in the production of disease is daily gaining ground. Should the remarks I have here made advance the truth in this direction, however imperfect they are in themselves, the labour will not have been in vain.

ARTICLE V.—*On the Closure of Congenital Fissures in the Hard and Soft Palates.* By THOMAS ANNANDALE, F.R.C.S., Lecturer on Surgery, Edinburgh.

(*Read before the Medico-Chirurgical Society, 7th December* 1864.)

CASE 1.—A. T., æt. 15, an intelligent boy, from Dundee, came to me on the 22d of June 1864, to seek advice about a deficiency of his palate. The lad was extremely anxious to get on in business, but found his defective speech a serious obstacle. On examination of his mouth, I found that the hard and soft palates were widely fissured through their whole extent. The fissure in the hard palate was fully half-an-inch wide. The hard palate was very highly arched, and the soft palate was not only fissured, but also deficient. The condition of the hard palate is shown in fig. 1, which is a drawing—reduced about one-third—from a cast of the mouth, taken for me by Mr Wilson, dentist. The dotted lines at the alveolar margin of the hard palate represent the direction of the incisions which were made at the operation. Besides this deformity, the upper jaw was cleft through its alveolar margin in front, the fissure being situated at the outer margin of the left central incisor tooth, the lateral incisor on this side not being present, and the patient had had also a single left harelip, which had been operated on in infancy. The margins of this harelip had been united, with the exception of a portion at the upper part, which opened into the left nostril. The lip was also too lax, and a finger could be passed under it into the nostril, and so out on to the face. The patient's speech was very indistinct, and it was often difficult to interpret the words he employed. In speaking, the upper lip became curiously contracted and distorted in endeavours to close the opening in its upper part. As the boy was most willing to submit to any operation for his cure, I determined to endeavour to close the fissure, and for this purpose to make use of the periosteum and other soft tissues, as proposed by Warren, Mr Avery, and Langenbeck.

On the 25th June, assisted by Messrs Hume and Robbs, I proceeded to perform the operation. Having seated the patient in a good light, I first pared the edges of the fissure in the hard and soft palates. This was a matter of some difficulty, owing to the high arching of the hard palate, and the inversion of the soft textures at the margins of the cleft. I then, with Langenbeck's own instrument, freely separated the periosteum and soft textures from the whole of the hard palate, with the exception of an attachment in front and behind, at the point where the bloodvessels and nerves enter these structures. An incision, extending from the canine tooth in front to the last molar behind, was then made through the soft textures on each side, along the alveolar ridge. In this manner two flaps, composed of periosteum and soft tissue, and only attached in front and behind, were formed, and the inner edges of these two flaps were readily brought together by means of silver-wire sutures, so as completely to close the fissure. The pared edges of the soft palate were also brought together by means of silver-wire sutures. The patient, who, up to this time, had borne the operation bravely, became faint, and could not any longer keep his mouth open, so that I was unable to adjust the last stitch through the uvula. With the exception of this, the operation promised to be most successful, as the edges of the whole wound were well together, and there was no tension on any part of the flaps. The patient was ordered to keep his bed, and not to talk or take any solid food.

June 26th.—The patient's mouth and jaws are so stiff that it is impossible to get a sight of the wound. He feels otherwise comfortable.

June 29th.—The patient has been able to take a little beef-tea and milk occasionally since the operation. The wound in the hard palate is apparently quite healed. Three stitches were taken out.

July 4th.—All the stitches were removed from the hard and soft palates, the wound being united along its entire length. The wounds at the alveolar margins are healing by granulation.

A few days after, the patient returned home, a slight improvement in his speech being already perceptible.

September 29th.—The patient returned to Edinburgh to-day to show himself, and I at once noticed a further distinctness in his articulation.

On examination of the mouth, I was much pleased to see the improvement which had taken place in the palate since the operation. The hard palate had lost its high arch, and was almost horizontal in position. The new material over the original fissure had apparently become ossified, and could not be distinguished from the rest of the hard palate. The soft palate was also perfect, with the exception of the uvula, which was cleft for a little less than a quarter of an inch. The patient complained of the looseness of his upper lip, and was anxious to have something done for it. I therefore re-divided the lip by cutting out the portion of it corresponding to the cicatrix of the old operation, and having pared the edges well up into the nostril, brought them together by means of silver-wire sutures.

Immediately before doing this, I had pared the edges of the bifid uvula, and brought the two halves together. Both operations succeeded well, and in the course of a fortnight the patient was perfectly cured of his deformities. The only thing he complained of was a slight separation and irregularity of the upper jaw in the position of the left lateral incisor tooth, and for a short distance behind it. Mr Wilson, however, was able, by means of an artificial tooth and small plate, to remedy this; so my patient again returned home with a marked improvement in his articulation. Fig. 2. is from a cast of the mouth taken four months after the operation.

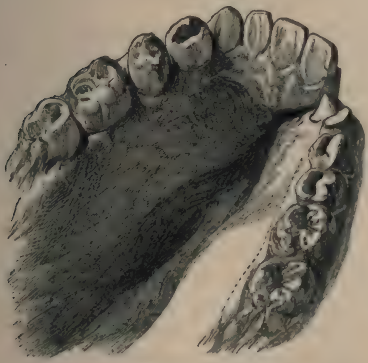

Fig. 1.

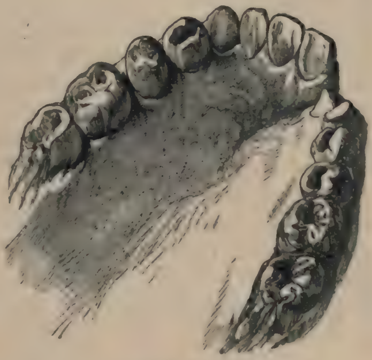

Fig. 2.

CASE 2.—Mr ——, æt. 24, recommended to me by Mr Syme. This patient had more than the posterior third of his hard, and the whole of his soft palate fissured, and finding his indistinct speech an impediment to his commercial success, was wishful to have some operation performed for its cure.

I accordingly, with Mr Syme's kind assistance, operated on this patient on the 22d September 1864. The operation was performed in a similar manner to that which I have described in Case 1., but the periosteum and soft textures were only separated from the posterior half of the hard palate.

September 27*th*.—Two of the upper stitches were taken out. The whole of the wound has apparently united, with the exception of a small portion about the centre of the soft palate, where one of the stitches has become loosened.

October 1*st*.—All the stitches taken out. The entire wound is healed, except the small portion already referred to.

October 3*d*.—The patient returned to the country; the fissure being quite closed, except a small opening the size of a grain of wheat, which remains in the centre of the soft palate, but which does not extend through the whole thickness of the tissues.

October 22*d*.—The patient came to see me to-day, and I found that the cure was perfect. The opening in the soft palate had contracted to the size of a small pin-head, and the rest of the wound was firmly healed. There was already a marked improvement in the patient's speech.

Remarks.—Fissures of the hard palate have been successfully closed by operations in England; first by Mr Avery, and more recently by other surgeons; but I can find no recorded account of any case in which a fissure, involving the entire hard and soft palates, has been cured by operation in Scotland.

The labours of Dieffenbach, Warren, Langenbeck, Pollock, and others, have done much to improve and establish the operation for the cure of this deformity, but still the usual method of treating fissures of the hard palate in this country is by fitting silver or other plates into the mouth, so as temporarily to close the cleft. It need scarcely, however, be said that the advantages resulting from operations such as I have just described are infinitely superior to any temporary or palliative methods of treatment.

The operation performed for the relief of fissures of the hard palate, as practised by Avery, and subsequently by other surgeons, with some little alterations, consists in first paring the edges of the fissure, and then dissecting off the soft tissues from the hard palate, so as to secure a flap on each side, the inner margins of which flaps are brought together over the fissure and adjusted to one another.

Langenbeck recommends that the periosteum of the hard palate should be separated along with the other soft textures, in order that the fissure may become in time closed by bone. From the nature of the soft textures over the hard palate, and from the manner in which these were separated by Avery and other surgeons, it is probable that the periosteum was thus separated by these gentlemen in their operations. Mr Fergusson of London first suggested the division of the muscles of the palate in order to facilitate the operation for closing fissures of the soft palate, and prevent any traction on the edges of the wound during its healing. This proceeding has been practised in a large number of cases by Mr Fergusson, and by some other surgeons, with certain modifications in the manner of its performance. Surgeons are, however, by no means agreed as to the propriety of this proceeding, or as to the particular muscles which it is necessary to divide. For my own part, I consider the division of the muscles of the palate an unnecessary complication of the operation. These muscles were certainly not divided in either of the cases just recorded, and yet the result of these cases was most successful. It may be said that the incisions practised in my operations were the cause of their success; and although the incisions undoubtedly tended very much to the success of these operations, by allowing the flaps to come together without tension, they did not do so by dividing the muscles of the palate, as they were not made in the situation or direction of the muscles.

Having then carefully studied the operation for closing fissures of the palate, as practised by Mr Avery and others, I proceeded to operate on Case 1., making, however, much freer incisions along the alveolar margins of the flaps than have, I believe, ever yet been practised in such operations. This I did because the fissure was wide and extensive, and I found that it was the only way to separate the flaps to that extent which would permit of their being brought together without any tension over the fissure. In order to avoid wounding the posterior palatine artery or its branches, the incisions were made quite on the alveolar ridge within a short distance of the free edge of the gum. Incisions along the alveolar margins have been practised before in this operation, but these incisions have been limited, owing to a fear that the soft tissues of the palate would die if their attachments were too much separated. The result of my first case proves that the soft tissues and periosteum over the hard palate may be almost completely removed from their connexion with surrounding textures and yet may live, heal in their new position, and no injury to the bone result. All that appears to be necessary is to leave a narrow attachment in front and behind at the points where the nerves and bloodvessels enter, by means of which the separated tissues may receive their nourishment. The length of these incisions will require to be in proportion to the extent of the fissure. In Case 2. the incisions were not carried so far forward as in Case 1., because the anterior portion of the hard palate was not cleft.

In separating the periosteum and other soft textures from the hard palate, I employed the blunt instrument recommended by Langenbeck for the purpose, and much prefer it to those curved or angular knives which are generally used in this country.

Operations for the closure of fissures, which involve the whole extent of both the hard and soft palates, have usually been completed at more than one sitting; and although the cases just recorded were both finished at one operation, with the exception of the adjustment of the last stitch in Case 1., I would agree in some instances with Mr Pollock, who thinks that it is better to close such fissures on different occasions, the fissure of the hard palate, and as much of the soft palate as possible being united at the first operation, and the remainder some little time after. If the patient is not too exhausted, it will, I think, be advisable to complete the operation at one time, as it is unnecessary to subject the patient to two operations when one may be sufficient.

The bleeding during these operations was easily checked by washing out the mouth occasionally with cold water.

The parts operated on do not appear to be sensitive. The separation of the soft textures from the bone was the only part of the operation which seemed to give pain. Some surgeons still prefer to use thread sutures to unite the edges of the wound in cleft palate, but there can be no doubt that the employment of silver-wire sutures is a great improvement in the treatment of this and other wounds. Having passed a thread through each edge of the wound separately by means of a sharp-pointed needle fixed on a handle (the needle I use is like the ordinary sharp-pointed aneurism needle, only made much finer, and a little more curved), these threads were united together in the way described by Mr Syme in his "Principles of Surgery," page 526, a piece of silver wire was then readily drawn through by means of the united thread, and twisted or adjusted so as efficiently to keep the margins of the wound together. In this manner as many wire sutures as may be necessary can be accurately introduced. A little difficulty was experienced in adjusting the stitch through the extremity of the uvula in both cases, but this difficulty was overcome by using the ends of the stitch above as a means of steadying the parts. For this reason, the ends of the last stitch but one were left long, and not shortened until the stitch in the uvula had been applied.

The stitches in the soft palate were not interfered with until the 9th or 10th day after the operations. The stitches in the hard palate were removed much sooner.

If the stitches in the soft palate are not causing much irritation, they should be allowed to remain even longer than ten days, a fortnight at least. For although the wound may be united before this, the union is not strong, and may be ruptured by excessive movements of the parts. There does not appear to be the same risk in regard to the wound over the hard palate, and therefore the stitches in it can be safely taken out at an earlier period.

The patients in both instances were allowed to take fluids, such as beef-tea, water, milk, or thin gruel, two or three times a-day for the first ten days after the operation, and after that time a firmer diet was given, but nothing which required much mastication was permitted till the end of three weeks after the operation. The patients were confined to bed, and forbidden to talk for the first week; but at the end of this time they got out of bed for several hours daily.

It has been observed, that after fissures of the palate have been successfully closed, the speech usually becomes improved immediately. This improvement may not, however, take place all at once, but some little time may be required before the patient becomes accustomed to the altered condition of the mouth. In both my cases the friends of the patients noticed a marked improvement in their pronunciation soon after the operation. This improvement was especially observed in Case 1.

ARTICLE VI.—*Notes on the Medical Organization of the Army of the Potomac.*[1]

HAVING lately had the opportunity of visiting the army of the Potomac, I have thrown together the following observations regarding some of the medical field arrangements of that army. And I may at once state that, taking into consideration the large force employed, the proportion of sick and wounded necessarily resulting therefrom, and the limited experience of the State medical authorities in field arrangements previous to the present unprecedented struggle, it may fairly be allowed that great praise is due for the very able and energetic manner in which all details have been carried out.

The sick and wounded soldier of no other service has been so well cared for in regard to his creature comforts as the Federal. What with the Sanitary, Christian, and State Commissions, acting independently of the Purveying Department, the requirements of the army hospitals have been well supplied. These Commissions act as adjuvants to the Medical Department; not professing to control or interfere in any way with the arrangements thereof, but only assisting in ameliorating the distress of the sick and wounded. The Sanitary Commission, from its influential connexions, has been of great and material assistance to the army; aiding the Medical Department in every way, either when short of supplies or in want of means of transport, and it is now so well organized, that at a moment's notice it can send to the front, or to any other place, from the base of field operations, any quantity of necessaries. The Commission is continually receiving from all the large cities in the Union, as well as from private sources, supplies, either in money or materials, which are constantly arriving at the base of field operations, and are obtainable for hospital use on requisition made by brigade and divisional surgeons. The internal organization of this Commission is remarkably well arranged, and reflects the highest credit on those connected with it.

The other Commissions, compared with the foregoing, are still in their infancy, and are deficient in that organization which renders the Sanitary Commission so complete, as also in the wealth which has been so profusely contributed towards that charity. Each State has contributed towards its "State Commission," according as it has furnished a certain number of regiments. And these State Commissions supply the sick man with such of the small necessaries of life as he may require; but they do not aim at furnishing the same unbounded supplies as the Sanitary Commission. The Christian Commission follows in the wake of the State Commission as far as necessaries are supplied; but in addition they supply spiritual advice to the sick and wounded. Clergymen go to the hospitals without receiving any remuneration—remaining there for a longer or shorter period according to circumstances, doing their utmost for the hospital inmates, spiritually and physically. These latter Commissions do not profess to give such aid as the Sanitary. All, however, render assistance, which is dispensed with no niggard hand.

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[1] We have been favoured with the following Notes by an officer in Her Majesty's Service.—ED. MED. JOUR.

It may be well to premise, before entering into the medical field arrangements of the Federal force, that the authorities have followed European Continental Powers in the division of their armies, first creating army corps, averaging from 30,000 to 40,000 men, and subdividing these again into divisions and brigades.

The medical officers of the Federal, or so-called United States army, may be classed in three divisions :—

1*st*, Those of the regular army,—a very small proportion in comparison with the entire number of medical officers employed throughout the Federal force.

2*d*, Surgeons of Federal or United States Volunteers,—members of this and the previous class, holding their commissions from the head of the Government.

3*d*, Medical officers of "State" regiments,—commissioned by the Governors of States, and only employed while their regiments are mustered in service,—a period of from two to three years.

Lastly, From the scarcity of medical officers, the Government has been obliged to inaugurate another class, under the name of "Contract-Surgeons," employed only for a limited time at a contract rate,—the engagement being generally for a period of three months.

Each army corps has a medical director of its own, chosen from one of the two first classes, whose duties are somewhat similar to those of a Deputy Inspector-General of Hospitals in the British service. This officer appoints the divisional surgeons, and selects the operating staff of the various hospitals in his command.

Each corps consists generally of three divisions, the hospitals of which may be in close proximity to, or separated some distance from, each other. The divisional surgeon superintends the medical arrangements of his division, as also those of the hospital funds. The hospital fund is an account between the Hospital and the Commissariat Departments. So many men being in hospital, the medical officer can either draw the usual ration, or is allowed its equivalent in money, and to the amount of that equivalent can indent on the Commissariat for any article in the way of extras he may require. His fund, therefore, may either be in debt or credit to a considerable amount. In the latter case, the Surgeon-General has the power of placing the amount in the hands of the Commissariat Department, and of paying the debt of some other hospital not so fortunate.

The Purveying Department does not furnish diets to the sick in hospital—it is restricted to furnishing medicines, extras, such as brandy, ice, etc. etc., and such other hospital necessaries. Another fund, the "Slush Fund," is entirely under the charge of the surgeon, and he can dispose of it in any way he pleases for the good of the hospital inmates. This fund is furnished by the sale of the broken bread and victuals of the hospital; also the sale of straw, bottles, etc., and any other refuse or broken stores not coming under the head of the Purveyor's Department. Each brigade furnishes its own operators, selected, as before stated, by the corps medical directors, in the proportion of one surgeon and two assistants, all being located at the divisional hospitals, which are either at one place, or separated as the line of battle extends, the site being selected by the corps director; and these operators, on an emergency, may be sent from one divisional hospital to the other. The hospital is generally stationed about half a mile in rear of the line of battle, advancing or retrograding in accordance with the divisional column,—a preference in site being given to proximity to a stream of water, and every possible shelter.

Each army corps has a double hospital organization,—namely, one for field service and one located at the transportation base. The base hospital is designed for those cases that cannot be treated in the field hospitals, and to which all sick not likely to recover in a few days are forwarded, as likewise all wounded as soon as operated upon, if such be necessary, and from the base hospital are forwarded, either by land or water carriage, to Washington and other places in the Union.

The tent in use for hospital accommodation consists of the usual wall-tent, with flies, supported by two upright poles and one centre horizontal ridge-pole. By turning up the front walls, and joining two or more such tents together, a large and commodious ward is formed. When it is necessary to economize canvas, the outer flies can be removed, and placed between two wall-tents, the intervening space being either left open, or walls can be formed of fascines. With such an hospital, which is continually on the move, and where it is impossible to carry about hospital furniture in sufficient quantity, *bunks*, or boards and tressels, at times may be put up, but oftener twigs and branches of trees are strewed upon the ground, over which is spread a blanket, forming a tolerable bed for the sick and wounded. When the field force remains sufficiently long in one place, bunks can easily be put up, the surrounding country being profusely wooded.

The transportation allowed for divisional field hospitals has been limited, thereby economizing space as much as possible. Three waggons only are allowed for 1500 men, and one waggon additional for every 1000 in the brigade. Each brigade has one medicine waggon, supplies for which are procured from the Purveyor's stores at the transportation base; these waggons being required to be kept up to their fullest extent, so that they may be always able to supply divisional hospitals.

The sick and wounded are carried from the field by the ambulance corps, to which they have been conveyed by the litter carriers. This corps, at a convenient time afterwards, transports the sick and wounded to the base hospital. The ambulance corps, only recently established by an Act of Congress, works well, and is under the control of the Medical Department. It is proportioned at the rate of three two-horse ambulances to each "regiment of infantry of 500 men or more, two to each regiment of 200 men and less than 500 men, and one to a battalion of less than 200 men. The medical director of each army corps, acting under the control of the chief medical director of the army to which such corps belongs, has the superintendence of all ambulances, medicine, and other waggons, and fixtures appertaining thereto, and of all officers and men who may be employed to assist him in the management thereof. It is also the duty of the medical director, previous to a march, and previous to and in time of action, or whenever necessary to use the ambulances, to issue the proper orders to the captain for the distribution and management of the same, for collecting the sick and wounded, and conveying them to their destination. This corps is officered on the following basis:—One captain, as commandant of the ambulances attached to each army corps, who sees that the men are properly instructed in their duties, and also that the ambulance waggons, etc., are at all times fit for service, carries out the instructions and suggestions of the medical director, and acts under his immediate division. To each division is assigned a first-lieutenant, who performs somewhat similar duty, as far as the division is concerned, and acts under the command of the captain of the ambulance. He is also the acting quartermaster of that portion of the corps, and is held responsible for any deficiency that may pertain thereto. To each brigade a second-lieutenant is assigned."

The foregoing is an epitome of General Order No. 18 of the ambulance system in use in field service of the army of the Potomac. It works harmoniously, although under the direction of the Medical Department. At first it met with great opposition, from the repugnance of the authorities to place such an organization in the hands of the Medical Department; but though it has been in existence only a short time, it has not only been able to gain the confidence of all, but even the approbation of its most strenuous opponents. It has been of the very greatest service as a means of transport for sick and wounded. It is ready at all times and seasons; and in such a corps none are so able to judge of the wants of the sick and wounded, and of any deficiency that may arise in such transport, as those into whose hands the immediate control and direction of the interim management of such a desirable institution has been placed.

Part Second.

REVIEWS.

The Principles of Surgery: Clinical, Medical, and Operative. An Original Analysis of Anatomy Systematically Conducted, and a Critical Exposition of its Guidance, at the Bedside and in Operations. By FREDERICK JAMES GANT, F.R.C.S., Surgeon and Pathological Anatomist to the Royal Free Hospital, etc. London: Churchills: 1864.

No one at the present day would be disposed to deny that enormous benefits have accrued both to Medicine and Surgery by the cultivation of morbid anatomy. Without a knowledge of the changes induced by disease, the physician and the surgeon must work to a great extent in the dark. They may be intimately acquainted with the structure of the various organs of the body, and may be familiar with the function performed by each; but from this knowledge they are unable to infer what changes in structure can be effected by disease. They may even have had much clinical experience, and may have accurately marked the derangements of function produced under a variety of circumstances; but even this additional knowledge could not enable them to infer what changes the diseased organ has undergone. In order, therefore, that a due value might be assigned to the symptoms of disease, it was essential that an opportunity should be afforded for comparing the state of the diseased organ with the signs which had been observed during life. At the same time it became apparent that there was a danger in attaching a too exclusive importance to the revelations of morbid anatomy. It soon became evident that a material lesion could not always be detected to account for the symptoms observed during life; and it was found that even a fatal result might ensue, without any marks of disorganization having been induced by the disease. It was then remembered that on the dissecting table we saw not the disease itself but its results; that we saw the organs in a state incompatible with the performance of their functions, or with the life of the individual. A certain reaction against a too exclusive dependence on morbid anatomy was the consequence; and its extreme development, Rokitansky's doctrine of "blood crases," has been abandoned even by its founder. It was therefore not without some surprise that we read a work, the object of which is to show that physicians, and particularly surgeons, have hitherto been in error in not recognising in pathological anatomy the only true foundation of their systems.

Mr Gant objects to all surgical works hitherto written, that "anatomy alone was the acknowledged guide during surgical operations, and that the doctrines of general pathology were the accredited 'principles of surgery.'" To this statement we entirely object; the best surgeons have ever been the first to confess their obligations to pathological anatomy, and have lost no opportunity of cultivating it; while the doctrines of general pathology *must* be the original principles both of medicine and surgery. In one sense anatomy *is* the foundation both of medicine and of surgery; without it pathology could not exist, for a knowledge of the diseased necessarily presupposes an acquaintance with the healthy. Mr Gant has, we think, undertaken a somewhat unnecessary task; we entertained precisely as high a sense of the value of morbid anatomy in reference to surgery, before we had read his work, as we do now that we have risen from its perusal. Morbid anatomy alone will not enable us to lay down the general principles either of surgery or of medicine; of as great importance to the practitioner is an acquaintance with the vital phenomena of disease which may leave no trace behind them, and a knowledge of which is only to be acquired by an extended clinical experience. In his anxiety to magnify the importance of his favourite department, Mr Gant uses expressions which we consider unadvised, and which are certainly calculated to prejudice his readers against him. In reference to anatomy he thus expresses himself, "Formerly the error prevailed—not yet overcome—that anatomy is the very foundation of operative surgery; and that without the light of its guidance in surgical operations, the simplest proceeding—say, puncturing a pointed abscess—would be no less perilous and culpable than making a stab in the dark." At the risk of being considered heterodox, we must nevertheless express our conviction that anatomy *is* the foundation of operative surgery. We would remind Mr Gant that in operating, we generally do so through healthy, not through diseased tissues. In cutting for stone, the parts between the neck of the bladder and the surface are ordinarily in a healthy condition. In amputating, we cut through tissues which, if possible, we select as free from disease. In performing the Hunterian operation, we tie the artery at a point where we hope to find it healthy, and where its natural relations to surrounding tissues have not been disturbed. What but anatomy in these, and fifty other instances, is to guide us in planning and in carrying out our operations?

Again, speaking of diagnosis, Mr Gant thus expresses himself:—"Dissimilar lesions would be associated by the anatomical method, an error which also, *pari-passu*, would result from the guidance of physiology; and I have shown here, that lesions of a similar nature might thus inadvertently be regarded as essentially different. Anatomy and physiology are, therefore, blind guides in diagnosis, being quite incompetent either to recognise *identity*, or to distinguish differences, of pathological character." Of course, anatomy and physiology, which treat of healthy structure and function, are different from pathology, the subject-matter of which is disease, and it would be impossible by the former alone to clear up points connected with the latter. But it is the very reverse of the fact that anatomy and physiology do not aid us in diagnosis. We may merely allude to the diagnostic value of contraction of the pupil as a sign of some intra-thoracic aneurisms, for which we are entirely indebted to the recent advances made in the physiology of the nervous system.

Before proceeding to give a brief outline of Mr Gant's work, we have one or two general remarks to make regarding it. In the first place, the name, we think, is ill chosen. From the title "Principles of Surgery," readers would believe that the work is one of an essentially surgical character. This, however, is not the case. It is rather a work on pathology, illustrated by reference to surgery and medicine; the illustrations from the latter being almost or quite as voluminous as those from the former. Thus we have upwards of a hundred pages devoted to the exanthemata, including an elaborate inquiry into the diagnosis of typhus and typhoid fever. Then the book is a great deal too long; it consists of 860 pages; and although it contains much important matter, a great deal of it has but an indirect bearing on the subject proposed. Mr Gant is a diffuse writer, and indulges freely in digressions, which, however interesting in themselves, distract the reader in his attempt to follow the author's argument. Finally, the work is unprovided with an index; an omission which we consider unpardonable, particularly as the "Prospectus of Contents," though tolerably full, gives a reference to pages, only at the commencement of each chapter.

Having explained his views in an introductory chapter, Mr Gant proceeds to expound the elements of pathological anatomy. This chapter extends to one hundred and twenty pages, and we cannot but consider it as in a great degree out of place. The remarks on fatty degeneration, though interesting in themselves, are, for a work of this kind, too extended; and twenty-five pages on human parasites are an unusual introduction to a work on surgery. Mr Gant is a strong supporter of the exudation theory, and says nothing of the modern cellular pathology. Thus he says, "Plastic products have their local origin in the transudation from the capillary vessels of 'blastema,' which may be regarded as modified *liquor-sanguinis*. This nutritive material undergoes various chemico-plastic changes, which terminate in equally various forms of structure, apparently unlike healthy conditions." Again, "The elementary granules aggregate together and form compound particles of larger size—*nuclei* (Schwann and Schleiden) whose component parts are thus named nucleoli. A nucleus, having formed, may itself give rise to a cell; hence its name sometimes *cell-germ* or *cytoblast;* while the clear fluid from which it was derived is appropriately named cytoblastema, or shortly, blastema." In these extracts, and indeed throughout the whole section, Mr Gant entirely ignores modern researches, which tend to throw doubt upon this spontaneous or equivocal generation of morbid products, and to establish the great principle of continuous development, in other words the doctrine that new growths always arise from pre-existing tissues.

The next chapter is entitled "The Principles of Diagnosis, showing the *earliest* and *most exact* detection and discrimination of the structural condition, situation, and extent of diseases and injuries."

Mr Gant first lays down what he calls the NEGATIVE PRINCIPLE, that "Anatomy and Physiology are severally incompetent guides to an early and exact diagnosis." This principle is first illustrated in the case of wounds. Having defined an incised wound, he says, "guided by anatomical considerations, we should infer that incised wounds would differ in their pathological import and tendency, according to the particular tissues or parts which are divided and exposed; and that they ought, therefore, to be distinguished by the anatomy of the various regions in which they occur. The exact diagnosis of incised wounds would thus be determined by their situation, and their surgical importance also measured by the particular *parts* divided. In the early days of Surgery, anatomical knowledge—coarse indeed, but still this kind of knowledge—suggested the differential characters of all wounds." Mr Gant then very properly points out that the natural tendency of incised wounds is to heal by the first intention, and that this is the character they possess in common, by which they should be grouped. He continues,—"If, therefore, we were guided by anatomical characters in our diagnosis of incised wounds, and distinguished them by the anatomical nature of the parts which are divided, we should thus draw an artificial distinction between lesions, which are naturally associated, by their tendency to the same happy issue; and moreover, we should overlook the operation of the great law of adhesion, as the first principle of therapeutic guidance in the management of wounds. This argument illustrates also the insufficiency of functional, or physiological, distinctions in diagnosis. All textures and organs—skin, muscle, tendon, bloodvessel, bone, intestine, lung, etc., however various their functional nature and importance—are associated by the laws of primary union, and therefore should be so regarded in the surgical treatment of their wounds, irrespective of the particular functional character of the parts that have been wounded."

He then shows that contused, punctured, and gun-shot wounds have no tendency to heal by the first intention, but that their tendency is to suppuration or sloughing. He thus concludes,—"These results of inflammation became the great criterion of distinction, and the pathology of inflammation gave importance to circumstances which, in the opinion of the old surgeons, were frivolous. Distinction of wounds, says Wiseman, taken from the weapons, whether sharp or blunt, sword, rapier, or bullet, cut, prick, or tearing, are but accidental. Yet such distinctions were found to be all-important in the modern diagnosis and corresponding denomination of these lesions; for the terms *incised*, *punctured*, *contused*, and *lacerated*, point to the two grand distinctions of 'adhering' and 'sloughing' wounds."

Now, we really do not see that very much is gained by this substitution of terms. All modern treatises on surgery inculcate the same principles in regard to the various kinds of wounds; and whether they trace their principles to the results of clinical experience or to the doctrines of inflammation, appears to be of very little consequence. It also seems to us that these considerations with regard to which wounds are divided, would have been more in place in the chapter on prognosis, because, according to Mr Gant, wounds should be classified, not according to their mere appearance, but according to the course they will pursue.

The next chapter treats of the POSITIVE PRINCIPLE of diagnosis; the principle being that pathological anatomy is the guide, during life, to the *earliest* and *most exact* diagnosis. This doctrine Mr Gant endeavours to establish by a consideration of the various kinds of fracture. Allowing that anatomical considerations may lead to the diagnosis of the existence of a fracture, and of its situation, our author maintains that the various *kinds* of fracture cannot be discriminated except by a pathological knowledge of what will be their course and tendency. Mr Gant, accordingly, divides fractures into simple, compound, and complicated, and the result he arrives at is the following:—"The simple, the compound, and the complicated fracture represent certain conditions which promote or retard the formation of a permanent callus. The 'simple' fracture implies no important rupture of the surrounding soft tissues, and therefore suggests speedy union; the 'compound' fracture signifies their laceration, and therefore tardy or imperfect union; and the 'complicated' denotes important injury of contiguous parts, often essential to life, and, moreover, proportionate evil influence on the formation of callus. These are pathological distinction during life; that they are also the most exact and earliest reliable grounds of distinction. The diagnostic value of pathological anatomy is therefore fully verified." Regarding this passage, we would make a similar observation to what we made in the case of wounds. All surgeons fully recognise the importance of the distinction of the different kinds of fractures; but we think it is a misapplication of terms to use the word diagnosis in the sense in which it is employed by Mr Gant. The diagnosis of a compound from a simple fracture merely depends upon whether or not there is a wound of the soft parts corresponding with the solution of continuity of the bone; and the question whether the one fracture or the other has the greater probability of a speedy and satisfactory recovery is certainly one of prognosis. Additional illustrations of the diagnostic value of pathological anatomy are given by a consideration of aneurisms and dislocations, but upon these topics we cannot enter. The only other point under this head to which we can allude is in reference to what Mr Gant says regarding the diagnosis of morbid growths by microscopic examination of their minute structure, in some cases by puncturing during life, growths and deposits, in others by examining discharges and secretions coming from internal parts and organs. Now, we value very highly the information furnished by the microscope, and do not despair of ultimately arriving at greater precision of diagnosis in regard to tumours by its use; this result, however, has not yet been attained, and in the diagnosis of morbid growths we still trust more to what we may call the coarser signs, and to the information to be gained by an observation of their vital manifestations. One of the great difficulties attending microscopic diagnosis is, that morbid growths are not constant in their structure, and that they do not contain elements which can be absolutely discriminated from healthy tissues. On this point Mr Gant speaks as follows:—"All cancers present the same cell. This, at first colourless, pellucid, and consisting of a delicate envelope, contains a large clear nucleus or two, sometimes more, never less, within each of which is embedded one or two nucleoli, also large and clear. Such is the *cancer*-cell (Bennet). It assumes various shapes, being either round or more commonly caudate, fusiform, etc., from outgrowths in one or more directions." Were these characters invariable, the diagnosis of cancer by microscopic examination would be easy. But all the later experience has shown that it is not so, and even Mr Gant is obliged to modify his statement in the following manner:—"Guided by these observations respecting the structural individuality of cancer, as a distinct species of growth, we must conclude:—1*stly*, That, occasionally, the structural elements of cancer are absent in tumours, otherwise cancerous; and therefore, that such elements are inconstant. 2*dly*, That these elements may be found (and often, according to Forster) in healthy as in diseased tissues, and are, therefore, not peculiar to cancer." These propositions seem, in the present state of our knowledge, fatal to attempts to diagnose cancer during life by microscopic examination; and although Mr Gant endeavours to show that "apparent differences of structure of the *cancer*-cell can readily be reconciled, and its deviations referred to a common type of the law of structural retrogression," the practical difficulty remains, and we must confess that the microscope cannot as yet give us that certainty of diagnosis which was at one time expected of it, and which it is possible it may still afford.

The next section of Mr Gant's work is headed, "The Principles of Etiology, or the *earliest* and *most exact* detection and discrimination of the causes of diseases and injuries, individually, and of the operation of such causes." This is the most elaborate chapter in the book, occupying, in fact, one-half of the volume; but as the greater part of its illustrations are derived from medicine rather than from surgery, we cannot enter into a consideration of it. Mr Gant goes over a great deal of ground, and has brought together much valuable matter, and although on many points we differ from him, we give him great credit for the industry with which he has collected, and the skill with which he has arranged his materials. The only observation we have to make regarding it is, that Mr Gant appears to hold strongly the idea that many diseases originate in the blood, whereas the result of recent investigations is to show that in the great majority of cases, dyscrasiæ have a local origin.

The next section treats of prognosis, and we must content ourselves by making two extracts, with which on the whole we agree :—"The truth is, that although pathological anatomy supplies the *basis* of the earliest and most exact prognosis (through a similar diagnosis, and identification of internal causes; both of which departments of surgery are built, as we have seen, on pathological anatomy *applied* during life), yet such prognosis, or foreknowledge of the course and tendency manifested by morbid conditions of structure, can alone be *concluded* by long experience, through 'clinical' observation of the diseased *living* body, in all its moods and declensions. Prognosis, no less than etiology, in respect of the *operation* of internal causes, is an application of pure pathology; and depending immediately on a right interpretation of *functional* disturbance, is necessarily obscured by its *indefinite* character. Adequate prognosis thus becomes the most difficult department of surgery, or at least as much so as etiology, respecting the *operation* of internal causes." Again, "In seeking to discover the general principles of prognosis, we experience one difficulty at least throughout the course of our inquiry—the paucity of materials at our command, and the inadequacy therefore of the basis on which our generalizations are built. This deficiency arises from the fact that our present knowledge of the *natural* course and tendency of diseases and injuries is very limited. The 'natural history' of disease is yet a *desideratum;* and why? Because hitherto the career of disease has been rarely observed when *uninfluenced* by therapeutic interference of some kind or other. Consequently, being unable to foresee, in most instances, what might or would be the *natural* course and tendency of any given disease or injury, the prognostic conclusions, or principles, at which we arrive—by however extensive a survey of clinical observation—are necessarily ambiguous."

Next follows a long section on the Restorative Power, followed by a short chapter on "Therapeutics, Medical and Operative." This is chiefly occupied by showing that pathology is the guide in surgical operations; and the argument is illustrated by operations on aneurisms, on hernial tumours, and on the urinary organs. Though we think that, as in other places, Mr Gant exaggerates the importance of morbid anatomy, we willingly allow that there is much truth in what he says, and that to the operating surgeon a knowledge both of healthy and pathological anatomy is indispensable.

We extract the following passage, which forms the conclusion of the chapter:—

> "Need I say more for pathology—for anatomy attired in the garb of disease and life?—assuredly the only ground of confidence and comfort to the operating surgeon, of safety to the patient, and of the most enduring professional reputation. The tribute of respect, therefore, hitherto paid to anatomy, in relation to practical surgery, must henceforth be transferred to pathology; for the latter includes the former, and much more—this, too, of immediate practical consequence.
>
> "Without *such* knowledge,—I would say—'the operator is seen agitated, miserable, trembling—hesitating in the midst of difficulties, turning round to his friends for that support which should come from within, feeling in the wound for things which he does not understand; holding consultations amid the cries of the patient, or even retiring to consult about his case, while he lies bleeding, in great pain and awful expectation.'
>
> "But the old carpenter style of operative surgery, derived merely from the dissecting-room, is fast passing away; it belonged to a period when surgery ranked with the mechanical arts, and shared their honours. Pathology will henceforth be recognised as the polar star of operative surgery; and the brief review I have presented of this science in relation to the chief operations of surgery, establishes its guiding principle—that the twofold condition of diseased or injured and living textures and parts can alone guide our instrumental and manipulative proceedings in surgical operations.
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> "This guidance also fulfils the cardinal principle of therapeutics—viz., the *least* amount, no less than the kind, of interference and assistance which may be necessary to complete the operation of the restorative (or reparative) power of nature.
>
> "Pathology is therefore the foundation of 'conservative' operative surgery."

In taking leave of Mr Gant, while we award much praise to the diligence with which he has collected from all quarters materials in illustration of his doctrines, we cannot but think that the result has not been proportionate to the labour and talent bestowed upon it. The importance of pathological anatomy is too generally recognised to render it necessary in the present day to write a large book in commendation of it; and we hope that when we next meet Mr Gant it will be in a department in which he will give us the result of his own observations, rather than in one in which he brings forward illustrations of what we cannot but consider self-evident propositions.

Lectures on the Diseases of the Stomach, with an Introduction on its Anatomy and Physiology. By WILLIAM BRINTON, M.D., F.R.S., Physician to St Thomas' Hospital. Second Edition. London: Churchills: 1864.

DR BRINTON'S work on the stomach, the second edition of which is before us, forms a valuable contribution to medical literature. It is no mere compilation, no crude record of cases, but the carefully elaborated production of an accomplished physician, who, for many years, has devoted special attention to the symptomatology, pathology, and treatment of gastric diseases. The present edition has been considerably enlarged; in particular, two new chapters have been added, to which we propose briefly to direct the attention of our readers.

The work begins with an introductory chapter, giving a very clear account of the anatomy and physiology of the stomach. The first lecture treats of the general symptomatology of gastric disease; the second of gastritis, and some allied lesions; the third is devoted to ulcer of the stomach, a subject which Dr Brinton has made peculiarly his own; the fourth and fifth treat respectively of cancer and cirrhotic inflammation of the stomach; while in the sixth is contained a brief, but able, account of the various conditions embraced under the term dyspepsia. The seventh and eighth lectures are new, and to them we propose at present exclusively to refer.

The first of these (Lecture vii.) treats of gastric phthisis, and describes a condition which, though not of frequent occurrence, is of much interest. Dyspepsia, as is well known, is an almost constant attendant on phthisis, and is doubtless due to a variety of causes. Thus, as Dr Brinton points out, the dyspepsia sometimes causes phthisis; in other cases the dyspepsia and phthisis are due to the same common cause, a cachectic state of the system; while, more frequently, the dyspepsia and the other indications of the cachexia are due to the injurious influence of tubercular deposits on the system at large. Besides these forms, however, there is another class of cases which Dr Brinton groups under the head of gastric phthisis, and of which the following is a description:—

> "The outline of a typical case of this kind may be drawn somewhat as follows:—The patient, usually under thirty-five years of age, feels the first approach of the malady as an 'indigestion;' an epithet which, on inquiry, resolves itself into a pain beginning between the first and second hours after food, and going off gradually. At first such a pain often follows but one of the daily meals; perhaps oftenest a full morning repast. It rarely brings with it any flatulence; and is still more rarely relieved by eructation. As the malady advances, the pain becomes more frequent, and follows all the meals; only distinguishing, by attacks of unusual severity, those in which the food is more copious in quantity, or more solid and indigestible in quality. By-and-by, the sickening depression which has gradually been recognised as an element of the increasing pain diverges into distinct nausea; and this again soon provokes retching, which, in its turn, sometimes gradually deepens into vomiting. Often, however, the latter symptom remains long or permanently absent. If present, it is only rarely, or after long persistence, that it brings back from the stomach any of its alimentary contents; and even then scarcely ever unloads the organ, much less relieves the pain by which it is preceded. By longer continuance, the pain and retching become more severe, and more easily provoked; and therefore continually approach the period of taking food, so as not only to follow it by a shorter interval, but at length to limit the meal to little more than painful and unavailing attempts to take food, the suffering which immediately follows its deglutition becoming almost unbearable."

It will be readily understood that the diagnosis of this condition is by no means easy, and that the malady in question is liable to be confounded with a variety of morbid states. The principal diagnostic characters are derived from the amount of febrile reaction being much greater than in ordinary cases of gastric disease; from the character of the pain, which is much more capricious and uncertain than in organic disease of the stomach; from the period at which the pain occurs not bearing a definite relation to the stage of the digestive act; from the vomiting which, in the advanced stages, precedes all sensation of pain, and is throughout unattended with hæmorrhage; finally, there is the absence of the physical signs of tubercular pulmonary disease, at least in an advanced form. This condition, Dr Brinton considers as "a kind of neuralgia of the pneumogastric and sympathetic nerves; certain thoracic segments of these conjoined nerves forming the starting point of an irritative and morbid action, which is transferred to their abdominal connexions, and is manifested in the visceral branches of the solar plexus." Generally speaking, this condition, alarming and distressing as it is, is readily amenable to treatment, much more so than gastric disorders depending on an organic lesion. The treatment recommended by Dr Brinton consists essentially in the use of alkalies, with minute doses of iodine; bitter infusions, often advantageously combined with hydrocyanic acid, and a nutritious but non-stimulating diet.

The last chapter is headed "Gout in the Stomach." It was until very lately a firmly established opinion, that gout was very liable to attack the stomach, commonly in the way of retrocession or metastasis, and that this manifestation of the gouty inflammations was very often fatal. Under the more critical appreciation of the present day, cases of gout in the stomach have greatly diminished in number, and the tendency of the chapter before us is still farther to diminish them.

Dr Brinton inquires, "What do we see in actual practice suggestive of such an affection; and what is the exact nature of the cases thus grouped?" This question is answered under five heads. In the *first* place, there can be no doubt that many cases of acute dyspepsia are dignified with the appellation of gout in the stomach, and that such may terminate fatally, although having no specifically gouty character, and no necessary coincidence with the gouty habit. In the *second* place, cases of biliary colic have, no doubt, been arranged under this category; this is rendered the more probable because, in some of the cases recorded as typical examples of this form of gout, jaundice has been mentioned as having been present. *Thirdly*, disease of the kidney, whether recognised or not, accounts for some cases of this kind. Disease of the kidney is frequent in the gouty, and the gastric symptoms producible by renal degeneration are identical with those descriptive of gout in the stomach. In the *fourth* place, the cause of the symptoms of some such cases is to be found in valvular disease or degeneration of the heart, or in morbid conditions of the bloodvessels. The answer under the *fifth* head we shall leave to Dr Brinton himself—

> "Is there any 'gout in the stomach' left after the subtraction of these various affections? I can only say that I know of no such case: have never seen one: have never been able to get trustworthy evidence of one from some of the most accomplished physicians living, or from the best records. Yet, I would rather not deny its existence, save in the above qualified form. Just as, in acute gout and acute rheumatism, I have constantly found the stomach in such a state of tympanitic relaxation as suggests great irritation of the nerves of the organ, so I should surmise that it is quite possible there may be a more intense and exceptional form of this nervous irritation, productive of violent pain, vomiting, and cardiac disturbance, perhaps capable of giving rise to at least an hæmorrhagic effusion from the coats of the organ. That the dyspepsias of the gouty subject show a far-off approach to phenomena of this kind has already been stated; and, I may add, they are in this respect paralleled by the dyspepsias of convalescents from rheumatic fever, as well as from ordinary fever. But such cases, as causes of death, must be infinitely rare. And while I should demur to their being accepted upon any evidence at present in the possession of scientific medicine, I should question whether, if they occur, they would be rightly named 'gout;' whether, in short, without having some specially or exclusively gouty origin, and gastric situation, they would be entitled to more than a place in those anomalous disorders of the nervous system, to which their phenomena seem so strictly to refer them."

With the conclusion here arrived at we entirely concur.

It only remains to us again to recommend Dr Brinton's book as an important contribution to our knowledge of gastric diseases.

A System of Surgery; Pathological, Diagnostic, Therapeutic, and Operative. By S. D. GROSS, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia. Third Edition, Enlarged and Revised. 2 vols, pp. 2136; 1314 Illustrations. Philadelphia: Blanchard and Lea: 1864.

THAT a third edition of this very large and important work has been demanded within at most five or six years, bears testimony to the amount of favour with which the profession both here and in America have welcomed it; while the completeness of the revision which each edition has received, and the thorough manner in which (in practical detail at least) the last has been brought up to the level of the present day, speaks volumes for the industry of the author.

To attempt anything like a criticism of a work of this size and fulness, within the limits of a short notice, would be useless; but a very few words will explain the author's aim, and the manner in which he has fulfilled it. His aim has been "to give a systematic and comprehensive treatise on the art and science of surgery, considered in its broadest sense,—one that shall serve the practitioner as a faithful and available guide in his daily routine of duty,"—"to embrace the whole domain." We can fancy the author having before him, as he wrote, the idea of a young surgeon of limited means going off to some distant settlement with a library necessarily small, with no near neighbour to consult in cases of difficulty. For such a one a treatise on surgery to be useful must be at once complete, practical, intelligible, full in detail and, as far as possible, illustrated. For such a one no previous knowledge of detail is to be taken for granted, nothing that may be practically useful is to be omitted; and to render this possible, historical digressions, however interesting, must be curtailed; transcendental pathology, however entrancing, must be sacrificed. If the author has any favourite hobby,—and he is sure to have one,—he must forbear to mount it; if any original theories, he must not propound them; as they may be wrong, they are certain to be superfluous. And worse than this necessary self-restraint is the treatment which the author of such a work as this is too apt to receive from the critics. Such criticism is very easy. Thus, one says,—"His notions of pathology are extremely vague and undecided; we can hardly tell whether he is of the Vienna or Berlin school; connective-tissue corpuscles are hardly alluded to." Another, perhaps, may complain,—"How dare he call this a system of surgery; my researches on the subject of gunshot injuries of the hippocampus major are never once alluded to." A third will accuse him of plagiarism,—"He is not original, his diagrams are copies, his descriptions are very like those of my friend Jones in his *magnum opus*." While the fourth and last describes the immense size of the volumes, the number of pages, their weight in stones or hundredweight, and the necessity he lay under for purchasing a reading-desk.

We may almost leave such critics to answer each other; but in all seriousness, we are too apt to forget that in such a work as this is, we have no right to require the same interest or fulness of detail which ought to characterize a monograph on a limited subject. Our great new English system *is* a series of monographs, and the amount of references, and the historical accuracy we find there, would be out of place in a work like this of Dr Gross.

To be original in surgery is in many cases certainly to be wrong; to describe a lesion or a treatment in words which mean the same thing as did those of the great masters of surgery is in almost every case the only way to be right. So long as the work is written in the English language, certain adverbs of position must be used in every description of dislocations at the shoulder; and if two authors both describe the same lesion, and both correctly, the adverbs they use must be the same.

Fairly judged in this manner, Professor Gross's work will be found to have well fulfilled the promise of the preface. Open it where we may, we find sound practical information conveyed in plain language, and it would not be easy to cite omissions of any importance. Reference to a given subject is rendered easy by a full table of contents and an elaborate alphabetical index.

Many of the cases cited are, as is natural, well-known historical ones; but many original ones from Dr Gross's own practice are simply and modestly told, some of great interest. One very remarkable one deserves notice. Fifty-two tumours "unmistakably encephaloid," were removed from the mammary region of one patient within four years in twenty-three distinct operations. The general health was good all the time, there was no glandular involvement, and three years afterwards there had been no return. —Vol. i. p. 263.

After these long years of war, one turns naturally to the article on Gunshot Wounds for fresh information; but as we know that those men who *act* history rarely *write* it, or even read it when written, so we find that the men who are in the very centre of the experience of gunshot wounds, seem not to have the time or the quiet necessary to record that experience. A full description is given of Dr Howard's plan of hermetically sealing gunshot wounds of the chest, but Dr Gross does not fully commit himself to an opinion as to its value.

A word or two on the manner in which the work is divided. General Surgery occupies the first half of the first volume, including Congestion; Inflammation and its results, which are treated briefly and practically rather than pathologically; Tumours; Malformations; Scrofula; Syphilis; Wounds; Diagnosis; Minor Surgery; Operative Surgery, which is a chapter not on surgical operations, but on the qualifications and duties of an operating surgeon, with details as to the preparation for and the demeanour during a capital operation; Plastic and Subcutaneous Surgery; Amputation and Excision; and, lastly, a most admirable short practical chapter on Anæsthetics and their administration.

The remaining volume and a half embrace the Diseases and Injuries of all the different systems, regions, and organs of the body, very plainly and fully described, with simple yet detailed accounts of the operative and therapeutic procedures necessary for each.

Such an arrangement necessarily involves occasional repetition, but escapes the danger of omission, which, from what we have seen of the aim of the work, would constitute a much greater offence in the eyes of Professor Gross. This book is no mere provincial or even national system of surgery, but a work which, while very largely indebted to the past, has a strong claim on the gratitude of the future of surgical science.

The large volumes are well printed, on paper of a texture much superior to that of many American books, and are profusely illustrated.

A Manual of Practical Hygiene prepared especially for use in the Medical Service of the Army. By EDMUND A. PARKES, M.D., F.R.S., Professor of Military Hygiene in the Army Medical School, &c. &c. London: Churchill.

WHATEVER difference of opinion may have existed in the profession as to the expediency of establishing a special school at Netley for the instruction of men already possessed of the qualification for practice,—and we did not, at the time, conceal our own doubts on the subject,—there could be but one opinion as to the fitness of the selection when Dr Parkes was appointed to the entirely novel chair of Hygiene. To make a worthy and profitable occupant of this chair, there were desiderated a set of various and diverse qualifications, which were not likely to be found in one set of hands. His varied experiences in different parts of the world, his former connexion with the medical service of the army, his literary attainments and facility, and his high personal character, pointed out Dr Parkes as peculiarly fitted for the new office. He has now for some years justified his selection by the fulness and acceptability of his discourses and practical instruction, by his literary activity, and by the personal regard of his pupils. Now, again, he has still further vindicated his appointment by this excellent book.

The work is, in essential character, an exhaustive compilation. On all points affecting, or which might affect, personal or general hygiene, on water, air, food, drink, soils, dwelling-places, and on everything relating to the surroundings of the individual or the mass, Dr Parkes has collected facts, statistics, theories, and demonstrations from all sources, and over all he throws the broad light of shrewd and well-thought-out private opinion. As to the bearing of these influences on the soldier, he gives us, under each head, a summary of the existing regulations, and he also summarises practical advice, counselling the medical officer as to the steps most advisable under all conceivable circumstances or emergencies.

When specifying the regulations as they at present exist, the form of machinery prescribed for bringing sanitary knowledge to bear on the improvement of the soldier's environments, Dr Parkes contents himself with a dry recital. It was not necessary, nor, in his position, would it perhaps have been becoming, to indicate any opinion as to their practical value. No such obligation of silence is laid upon us, nor need we scruple to say that these regulations are very complicated, cumbersome, and, from their very nature, ineffective, while they are apt to cause much misunderstanding and embarrassment. They almost look as if they had been framed with a view to the multiplication of what the French call *obstacles*, checks which are, in a good system, necessary and useful, but which, in this instance, are needlessly complicated. There is the further objection that they invite, at every point, the meddlesome interference of "medical administration," which is the latest high-polite phrase for unprofessional meddling with the freedom of professional action. Further, they proceed on the erroneous theory that the officer in command of a regiment or division possesses some substantive power to carry out sanitary improvements, whereas, in point of fact, he has little or none. All, or nearly all, power in that direction rests naturally and necessarily with the central authority. It is the duty of the War-Office to make itself informed of everything which bears on the subject. That information the War-Office has, during the last few years, been acquiring with great diligence, and, no doubt, at great expense. It is there that all further responsibility rests. If preventable disease is still found in regiments and at stations, if the men are overcrowded, if they are insufficiently fed or inadequately clothed, if families are huddled together in a manner which outrages all the laws of health, and also common decency,—if, in one word, principles of health which are admitted to be good and imperative have not been fully applied, the responsibility rests with the Secretary for War, and with him alone. It is for the public, which has freely given such large sums, year by year, to know how far its generous confidence has been justified.

From no fault of the author's, but owing to the tone inseparable from studied compression, it is possible that the general reader may be led to think, from some parts of the "Duties of Medical Officers," that there lies in their hands a greater measure of direct power than is really the case. It would be a pity if such a notion should gain currency. It would be very dangerous to the weaker brethren, especially among military men. It is of the last importance to remember that there is a very broad line of separation between the duties and position of military and medical officers,—in War-Office slang, between combatants and non-combatants,—and that that broad line ought never to be overstepped. The military officer is responsible for what he does. The medical officer is responsible for what he advises. The latter is responsible only for his opinion. The more rigidly and precisely this distinction is observed, the greater will be the medical officer's influence for real good, the more completely will he be likely to illustrate the real dignity of his profession, and, most assuredly, the greater will be his personal comfort. Unfortunately, as we have hinted, there are weak brethren, and the present regulations invite manifestations of their idiosyncrasies. Regulations which require an officer of one of the civil departments to report on points in the interior economy of a regiment, and which require the surgeon of a regiment to report on the same points, and all, too, behind the commanding officer's back, are little likely to be acquiesced in by any military officer, unless he should chance to be possessed of temper and common sense to a degree which no system of competitive examination can provide. Hence misunderstandings. Hence the strange notion which seems to have taken complete possession of the mind military, that the medical officers wish to "command." Hence a confusion of ideas which is little creditable to the mental condition of its victims. Certain regulations require the medical officer to do certain things which are, naturally enough, distasteful to those who consider their authority infringed by them. Instead of endeavouring to get the regulations altered in a sense more consonant with their notions of discipline, they cry out,—with a cry, too, which is quite away from the real point,—against men whose sentiments, for aught they know, may be more antipathetic towards the system than are their own. Indeed, it would appear that it is the very men who are responsible for the regulations who are loudest in this strange sort of vicarious criticism. No doubt, there are faults on the other side. Some men seem to labour under the idea that the British army consists of a medical department, with some regiments attached to it as a *corpus vile*, supplied for the mere purpose of promoting epistolary activity. Where a medical officer's duty is discharged with sense, tact, and discretion, his power for good is immense. The more potential his voice may become, the more incumbent on him are care and even hesitation.

Dr Parkes gives us gratifying evidence of progressive improvement in the ratios of sickness and mortality effected within the last few years. We must be cautious in the estimation of the causes of these happy results. It will not do to credit them in all cases to corresponding improvements in the soldier's condition. There can be little doubt that, at some stations, the sick rates have shown progress out of all proportion greater than what little improvement may have been effected in barracks, or other sanitary environments. Dr Parkes very fairly looks forward to still further reductions of the sick and death rates. On this point it ought always to be remembered that the sick rates of soldiers cannot be compared with those of the civil population without a very important caution. A large proportion of the cases furnishing admissions into military hospitals would not appear in any returns relating to civilians. There is a sound rule, as regards soldiers, that every man ought to be either in hospital or doing his duty,—one of those rules which is best followed when it is allowed to be subject to certain exceptions. Most of the "phlegmons and ulcers," to which Dr Parkes refers, would hardly become objects of treatment among civilians; and there are many other cases,—in fact, the greater number of all the cases, which do not necessarily require confinement to the house, excepting where the above rule applies,—such as slight sprains, many cases of venereal disease, strictures, slight rheumatism, injuries of fingers, and many other cases. The best plan, towards a correct estimate, is to look on the sick rates as showing the ratio of *non-effectiveness*, which would be less misleading than a statement which invites an unconscious comparison with rates which are much less subject to disturbing influences.

As taking sides in an old controversy, and one which is still being hotly fought over, Dr Parkes, in so many words, avows himself to be a contagionist. It is remarkable that the modern school of sanitarians seems more and more tending to this side of the question. Miss Nightingale, it is true, with feminine impetuosity, utterly repudiates all sympathy with it; but her vehemence is characterized by much ignorance of the essential pathological conditions of the diseases of which she speaks. As has been said, the present tendency is towards contagion. For those who choose to investigate the question now, free from prejudice and remote from the dust and heats of the old conflicts, the issues are much altered, and in a direction which gives ground of hope for sound, well-settled conclusions. Formerly the facts under discussion were such that a man brought out of it little more than what he took into it. He entered with prejudices, prepossessions, and a certain turn of mind, inclining to one side or the other; and the evidence was so uncertain, that his inquiry only confirmed the original bent of his mind. The facts were always more or less capable of being read in either way. The story was always very much the same. Instances of real conversion, as that of the late lamented Dr MacWilliam, were very rare. Now, however, we are on the road towards well-assured results. We are gaining possession of facts which are really facts. The microscope and chemical agency discover things which are, or may be, true *contagia*. When we can see with our eyes and handle with our fingers palpable entities, an immense stride will have been made towards a sound theory of epidemic disease. The discussion will have passed to quite another stage. The reasoning will be from the premiss of an undisputed induction. But even then there will be much to fight about.

There has lately been a severe epidemic of yellow fever at Bermuda, and the newspapers have reproduced for us the old stories, which one reads in all similar histories. Ship-importation—this time by a blockade-runner, of course—then propagation by nurses and washerwomen, and so forth. Doubtless, the anti-contagionists have found abundance of "insanitary conditions," which have quite settled the matter to their minds. It is to be feared that we cannot hope to have much light thrown on the general question from this latest outbreak.

There was recently, in some ships of the Mediterranean squadron, a strange sort of epidemic lung disease, which at one time seemed almost to point towards a confirmation of the notion popular in some countries, of the contagion of phthisis. Subsequent reflections, however, and the unfortunate absence of positive and conclusive data, seem to detract in great measure from any value which might have been attached to this occurrence.

Dr Parkes dwells with natural emphasis on the great amount of inefficiency caused by the venereal group of diseases. When he says that "the soldier conceals that ailment as long as possible," we find ourselves at variance with him. Our belief is, that soldiers are most anxious and ready to seek relief in the proper way, when they find themselves affected with such diseases. Two charges are apt to be brought against the soldier, both of which we believe to be unfounded, and both of which cannot surely be true. One is, that he conceals venereal disease, because, as Dr Parkes says, "he will be sent to hospital, put under stoppages, etc." The other is, that for the purpose of being sent to or detained in hospital, and to save money under the stoppage system, he simulates diseases, aggravates symptoms, or endeavours to get undue importance attached to trifling ailments. These are opinions to which we do not assent, and with which we have no sympathy. A soldier is, as far as our experience goes, very much like other men. He may think lightly of ailments or he may exaggerate them, but that is from his individual habit of mind, not because he is a soldier. When the soldier element acts in the matter, it leads him to declare the ailment, because that is what his sense of duty prompts him to do. It seems to us, and we say it with every respect, that these essentially *low* notions about the soldier belong to the class of what Archdeacon Hare called "idle, purposeless, untruths,"—untruths which pass current from mouth to mouth, and which men repeat, simply because they have heard them, and without a thought as to whether they are true or not. They are relics of the bad old times, when the soldier was looked on as little better than something to be flogged and bullied. Dr Parkes must pardon us if we add, that he shows a sympathy with these sentiments which surprises us much, when, in connexion with venereal diseases, he speaks with approval of a form of "inspection" which, when he was in the service, was in some instances—let us hope they were rare—conducted with revolting brutality. To say, as he does, that some medical officers considered these inspections to be "derogatory," is to use language which is quite inappropriate. Nothing which is for the good of his charge, or for the general advantage of the service, and which falls under the category of professional duties, can be derogatory in the eyes of a man of sense. With any one who thinks otherwise than as we have put it, we have no sympathy. But we have some respect for the dignity of human nature. These inspections, as they used to be carried out, were outrages on what ought to be held most sacred and most respected. Besides all this, they were quite useless, excepting in so far as that, by establishing a relation of spy and culprit, an inducement was offered to the latter to furnish some excuse for the practice. Treat soldiers as they ought to be treated, with tenderness and justice, and there will be no occasion for exceptional and insulting manifestations. When Dr Parkes says that "a great outcry has been raised against the proposal to detect and cure the disease in prostitutes," is he not simply repeating a form of words, forgetful for the moment of the fact that Parliament has undertaken the duty; that the Contagious Diseases

Act of last session was introduced with the eager sanction of men of all parties; and that, through the active support of Mr Arthur Kinnaird, it may be said to have received the *imprimatur* of Exeter Hall? Dr Parkes and those who think with him have now got their Contagious Diseases Act. He seems to expect little good from it. On that point we are quite at one with him.

If legislation is not likely to help us, may we look for more assistance from a queer sort of Commission, which, the newspapers tell us, is now sitting, on the subject of syphilis, in London. A Commission is usually announced to the public in a public official way; but, for what is to be gathered about this one, we must look to fragmentary newspaper notices. A gentleman, whose standing in the profession is of such old date that he can hardly be abreast of the latest writings on the subject—who, for aught we know, may have studied syphilis with John Hunter at the siege of Belleisle—lately published a pamphlet, which his best friends must have wished to be entirely unnoticed, of which it would be difficult to say what was its worst feature, the grammar, the logic, or the taste, and the thesis of which seems to have been that there is no such disease as syphilis, and that no one knows how to treat this non-existent disease. This seems to have been seized on by some of our public men, with an eagerness which there may not be much lack of charity in ascribing, in some measure, to that strange dislike of medical men which they have lately exhibited, and which seems to be coincident with a dwarfing of the proportions of our public men. The result is a Commission on the treatment of syphilis. Now, as we all know, there is scarcely one subject, in the whole range of medicine, which has, within the last thirty years, been so fully or scientifically treated as this subject of syphilis. The Commission is likely to find that there is no royal road to the treatment of that disease. It is creditable to some leading members of the profession, that they should, as is reported, have declined to have anything to do with the Commission.

We have attempted no systematic analysis of this book, nor does it need one to recommend it to our readers. The general reader will find in it a great mass of facts, all ready to his hand, on important subjects, and such as he will find in no other volume. To the military medical officer it will be an indispensable handbook. For these purposes we do not wish it to be shorter by a single page; but for the general good of Her Majesty's Service, we should like very much to hear that Dr Parkes had been commissioned to condense it into the shape of regulations for the sanitary administration of the army. Such a code is much needed, and no one is better fitted—we should rather say, no one is so well fitted—for the duty.

Photographs (*Coloured from Life*) *of the Diseases of the Skin.* By ALEX. BALMANNO SQUIRE, M.B. Lond. London: Churchill: 1864.

THE applications of photography to medicine have as yet been limited, and we are glad to find that Mr Balmanno Squire has taken the initiative in employing it for the illustration of diseases of the skin. Although photography only ensures accuracy of outline, it is easy for the artist to add colour; so that at least half of the illustration is an absolute reproduction of the lesion as it exists in nature. We have received the first of Mr Squire's photographs, which consists of a remarkably good illustration of Psoriasis affecting the legs. The characters of the disease are clearly indicated, and the execution, both of the photographer and of the colourist, is unexceptionable. The plate is accompanied by an intelligent commentary, which contains the chief facts relative to Psoriasis, and indicates the points illustrated by the drawing.

Part Third.

PERISCOPE.

PRACTICE OF MEDICINE.

ON THE CONDITION OF THE HEART IN EMPHYSEMA OF THE LUNGS.

BY DR WATERS, LIVERPOOL.

AMONGST the more prominent symptoms of general or lobar emphysema of the lungs is an altered condition of the heart. The expansion of the lungs pushes the cardiac organ downwards, backwards, and towards the mesial line; so that in advanced states of the disease the heart is felt and seen to beat in the epigastrium beneath the ensiform cartilage; the cardiac region becomes resonant from the overlapping lung, and the lower site at which the sounds of the heart are best heard is altered. These are facts familiar to all who have given attention to pulmonary diseases, and it is not to them, but to certain changes which take place in the walls of the heart, that I wish to direct attention.

It has been the opinion of many pathologists that the right cavities of the heart alone become affected in emphysema; but the researches of Gairdner, Lebert, and others have satisfactorily proved that, in the majority of cases where emphysema is extensive and of long standing, the cardiac disease is not confined to one side. My own observations tend to confirm this opinion, and to show that the form of heart-disease most frequently associated with emphysema is a general hypertrophy and dilatation of the ventricles; for I have never seen a post-mortem examination of a case of extensive and long-standing lobar emphysema in which the left ventricle, as well as the right, was not affected.

That this diseased condition of the heart is a consequence of emphysema we can have no difficulty in believing, when we consider the nature of the lung-affection, and especially that the palpitation, and other cardiac symptoms, are always preceded by dyspnœa and other signs of the pulmonary malady.

But hypertrophy of the ventricles is not the only change which takes place in the heart, for valvular disease is frequently found. The deposits which occur about the valves are no doubt secondary to the changes which take place in the muscular walls, and must be attributed to the general mal-nutrition produced by the pulmonary disease.

Can we give any satisfactory explanation of the causes which lead to hypertrophy and dilatation of the cavities of the heart in emphysema?

When we consider the anatomical arrangement of the pulmonary tissue, and especially of the pulmonary bloodvessels in the disease, we can have no doubt as to the manner in which the right side of the heart becomes influenced. The impediment which exists to the circulation through the lungs, in consequence of the physical condition of the lung-tissue, and the imperfect aëration of the blood, together with the diminution in the number of the pulmonary bloodvessels, must necessarily give rise to an overloaded state of the right cardiac cavities, and to increased action on their part.

No such explanation as that just given will, however, apply to the hypertrophy, so commonly found, of the left ventricle; for there is a diminution in the quantity of blood which finds its way into that cavity, and consequently, on this account, rather less call for muscular action than in a state of health. We might therefore infer that we have in these circumstances an element of atrophy, rather than of hypertrophy. But morbid anatomy teaches us that the latter usually exists. Whence then does it arise?

It appears to me that we must look, in great measure, for an explanation of the fact to the effect produced on the heart by the displacement it undergoes in the disease. This displacement is always the greatest where the emphysema is most extensive, and it is in such cases that the left ventricle becomes most hypertrophied. As the lungs expand, the heart is pushed away from its normal position; and, consequently, the direction of the axis of its cavities is altered with reference to that of the vessels connected with them. The ventricles of the heart are so placed, in a state of health, with regard to the arteries which issue from them, that no impediment exists to the onward passage of the blood, and the circulation is effected with the smallest possible expenditure of muscular force; but displacement of the heart necessarily alters the relations between these several parts, and produces an embarrassment of the heart's action—an embarrassment that can only be overcome by more powerful contraction. We consequently find that hypertrophy follows.

This appears to me to be the chief reason that must be assigned for the occurrence of hypertrophy of the left ventricle in emphysema. It is true that, in consequence of the condition of the venous system, some increased force may be required on the part of the left ventricle, and thus another element for the production of hypertrophy may exist. The altered situation of the heart is, however, I believe, the main cause; and this must have an influence in producing the changes which take place in the right ventricle, but here another cause is also at work.

Amongst the symptoms of emphysema, resulting from the changes which I have just referred to, must be enumerated the powerful impulse of the heart, often felt in the epigastric region, together with the smallness and feeble character of the pulse, as felt, for instance, at the wrist. A knowledge of the condition of the heart and of the state of the pulmonary circulation serves to explain these phenomena. The powerful impulse of the heart is the result of its hypertrophy, and embarrassment from position; whilst the smallness of the pulse is due to the small quantity of blood which the left ventricle expels at each beat, and its feebleness to the fact that the force of the left ventricle is expended, in part, in overcoming the resistance which exists to the passage of the blood from the ventricle, in consequence of the altered position of the latter, and in part merely in distending the arterial tubes. The diminished circulation through the pulmonary tissue, and the accumulation of the blood in the venous portion of the circulating system, sufficiently account for the small quantity of that fluid which the left ventricle has to react on at each beat.—*Lancet*, 12th November 1864.

CASE OF STRANGULATION OF INTESTINE BY A DIVERTICULUM ILEI.
BY DR WILKS.

Of the various abnormities of the intestinal canal, perhaps that known as Meckel's diverticulum verum is one of the most important, from the peculiarities of its formation. The deformity is not an excess of development, but an arrest of formation, and is thought of such importance by Rokitansky that he devotes special consideration to it in his Pathological Anatomy. It is a dilatation of the small intestine, representing a hollow appendix, which consists of all the intestinal membranes, and is placed at from eighteen to twenty-four inches from the cæcal valve. Rokitansky does not assent to Meckel's view that it is a remnant of the umbilical canal; it evidently has its origin, he asserts, in the development of the intestine in the umbilical vesicle. The condition of the diverticulum in the following very interesting case favours this opinion, inasmuch as, in place of terminating in a free appendix, as is sometimes found, it was attached to the abdominal walls at the umbilicus, ending there in a blind tube. The diverticlum was pervious throughout, and would have admitted a lead-pencil. The risk of strangulation of the bowel is always greater under such circumstances than when the abnormal appendix is floating and free, in which case it seldom exceeds five or six inches in length.

E. M. W——, aged ten, was admitted into Esther ward 13th August, 1864. She never had any symptom (according to the mother) of obstruction of the bowels until 4th August, when, after eating some gooseberries, she began to vomit. At this time there was some slight evacuation from the bowels. Since then there had been all the symptoms of obstructed bowel; no passage, and frequent vomiting, which finally became fæcal. Collapse at last set in, with great heat of abdomen and peritoneal symptoms. She died two days after admission.

At an examination twenty-four hours after death, the body was found to be spare, and the abdomen tympanitic. Acute peritonitis was present. The membrane was vascular and covered with lymph. On lifting up the parietes, a band was seen passing from the umbilicus to the lower portion of the ileum; and on separating the intestines the point of constriction was seen at this spot, about a foot or a foot and a half above the termination of the ileum; the portion of intestine above being much distended, and that below contracted. The latter portion was coiled up, and lay hanging down towards the pelvis, so that the band produced a greater drag upon it. This band, on more careful examination, was found to be a diverticulum of ileum, which at its commencement was as large as the contracted portion of bowel, with which it was in connexion; and on examining it further, it was found pervious in its whole length—in fact up to the umbilicus, where it terminated in a blind end, and was attached to the walls. It would admit an ordinary lead-pencil along it. It was, then, evidently the remains of the vitelline duct or umbilical vesicle, which had continued open from an early period of fœtal life. The contraction seemed to be caused more by simple dragging on the part, although no doubt the portion below had fallen in such a manner as to cause the sudden complete constriction. It appeared, however, as though some constriction had long existed. The dragging of the diverticulum was so great that on removing the parts a laceration occurred at the point where it sprang from the ileum, and fæcal matter escaped. It is possible that this commenced during life, and set up the sudden and fatal peritonitis.—*Lancet*, 12th November 1864.

ON THE TREATMENT OF ECZEMA. BY R. H. MEADE, ESQ.

After perusing the learned paper on the Nature, the Varieties, and the Treatment of Eczema, by Mr Erasmus Wilson, which was read at Cambridge, and published in the *British Medical Journal* of 19th November, I felt that the last division of the subject was much less satisfactorily treated of than the earlier ones; for, while the description of the nature and varieties of the disease was very graphic, the directions regarding the treatment of this common and troublesome affection were too general and indecisive.

In the treatment of this as of most other complaints, it is doubtless necessary, as Mr Wilson says, to look to the constitution of our patient, and endeavour to correct any disordered function that may be present, or remove any predisposing causes; but is this sufficient to cure a case of eczema? May we not often succeed in strengthening or otherwise improving the general health, and yet find the disease as troublesome as ever? Are there no remedies which may be looked upon in some measure as specifics? I believe there are; but the only one recommended in this light by Mr E. Wilson is arsenic; and though the efficacy of this tonic is undoubtedly great in many forms of cutaneous disease (the true scaly affections, for instance), I have little or no faith in its curative powers in cases of eczema, and have almost always found it disappoint my expectations.

Though eczema (as Mr E. Wilson truly says) occurs in various forms and degrees, arises apparently from various causes, is met with in very different constitutions, and breaks out at all ages, yet I have mostly found it yield to the same remedies; and the most efficacious ones in my experience are bichloride of mercury and antimony. I quite agree with Mr E. Wilson, that eczema is generally a disease of debility; but though it mostly arises in weak states of constitution, it is in its own essence inflammatory; and therefore, while the strength is supported by generous diet, an alterative remedy, like bichloride of mercury, which is so generally efficacious in chronic inflammatory complaints, may be advantageously given, and even combined with tonics.

With regard to antimony, it is less generally applicable in eczema than corrosive sublimate, but it will sometimes be found very useful; and in some obstinate cases, I have seen these two remedies exceedingly efficacious in combination. The form of the disease in which antimony administered singly seems more particularly applicable, is that in which it is acute in its character, and occurs in persons of a full inflammatory habit, who are also, perhaps, subject to gout or rheumatism. Tartar emetic given here, in combination with aperient salines and magnesia, will often cure the complaint very quickly.

We frequently see very annoying cases of eczema in oldish people, in whom the skin round the anus and about the scrotum or labia is affected. I have found the combination of bichloride of mercury and tartar emetic in the same mixture particularly useful in these cases, and sometimes cure them in a week or two, when they had resisted other treatment for two or three years. The dose of bichloride which I generally find sufficient is one-sixteenth of a grain, given three times a-day, with one-eighth of a grain of antimony; in some demulcent mixture, as decoction of sarsaparillia or dulcamara.

Eczema very frequently occurs in infants, breaking out soon after birth, and becoming aggravated as teething commences. Most of these cases have, I believe, an hereditary origin—one or the other parent having a disposition to the complaint. These young subjects are mostly weak, and require strengthening treatment. Unless they are suckled by a strong mother or wet-nurse, they should have good beef-tea or broth given them, in addition to the milk; and have little or no farinaceous food. These are cases which will be especially benefited by corrosive sublimate. It must be given in very small doses, and continued a long time; it may be taken, with occasional intermissions, for months together with advantage. A sixtieth of a grain, or even less, in a very young child, taken twice a-day, with a little fluid extract of sarsaparilla and glycerine (which is a very good demulcent), will mostly check the complaint. Should the bowels be at all irritated by the mercury, a little opium may be combined with it.

In recommending bichloride of mercury so strongly as a remedy for eczema, I do not wish it to be inferred that I am bringing forward anything new; for I believe that this medicine has been long and largely used in this complaint by the medical officers of the London Hospital for the Diseases of the Skin; and, knowing this, I was the more surprised that it was not even mentioned by

Mr Wilson in his paper. Having long used it in my own practice, I am anxious to bear testimony to its efficacy, and can recommend those of my medical brethren who are not yet acquainted with its virtues in the treatment of eczema, to give it a fair trial, both in combination with antimony and without it.

In the few remarks which I have made upon the treatment of eczema, I have made no allusion to external applications. I now mention them for the purpose of saying that I by no means underrate their importance; but, as I am not professing to write a paper on the general treatment of this complaint, but only wishing to call attention to one or two particular remedies, I think it unnecessary to enter fully into the subject.

EPILEPSY, AND THE ADMINISTRATION OF BROMIDE OF POTASSIUM.

BY DR G. GODDARD ROGERS.

A VERY interesting paper on the above subject, from the pen of Dr M'Donnell, was published in the *Dublin Quarterly Journal* of February last. It fell under my notice in May, when I was about to give a record of considerable success in the treatment of a case where, all other remedies having failed, I had been for a considerable time trying the bromide of potassium. After reading Dr M'Donnell's observations, I determined to give the medicine a few months' longer trial before venturing to speak with any confidence as to its efficacy. To my own mind it is satisfactory that so long a time elapsed between the publication of Dr M'Donnell's paper and my perusal of it. I came to the treatment of the patient mentioned below with an unbiassed mind, and with a feeling that I had undertaken an almost hopeless task. I was, however, gratified by a measure of success, and strengthened in my belief that good results were to be obtained from bromide of potassium in certain forms of epilepsy, when my attention was directed to the above-mentioned able article. I may just observe that I have given the drug to other epileptic female patients at the hospital with good effect, but the case I now append is the only one I have been able to watch for a prolonged term. Doubtless we are treading on safe ground if, for the present, we adopt Dr M'Donnell's view, that "each case of epilepsy is in itself a study;" but I trust that erelong so many individual cases will have been observed of epilepsy connected with derangement of the female genital organs, as to enable us to form a group or groups of these affections amenable in a large per-centage to a special remedy, even though we may in the present state of our knowledge only rudely conjecture its mode of action. With all deference, I beg leave to suggest that during a certain period—say from January 1865 to March inclusive—the various hospital physicians in this metropolis use the bromide of potassium in all cases of epilepsy where there is any reason for regarding uterine disorder as the source of the complaint. If no good result follow, yet no great harm can accrue, and at all events we shall have cleared away an untenable theory. There is true wisdom in the remark of M. Pouchet: "Every idea *à priori*, every hypothesis, is only good if we accept it with a strong determination of abandoning it if the facts are no longer explicable by its means. Without this, its influence is disastrous."

CASE 1.—R. B., aged 34, married, with two children, the youngest eleven years old, first came under treatment at the West London Hospital in May 1861. About six months after marriage she miscarried, and had for three weeks frequent attacks of syncope, followed a few weeks later by a convulsive seizure, during which she lost her consciousness. She does not remember being told afterwards whether she foamed at the mouth or bit her tongue. These attacks were few in number for some years; but she often had "fainting fits" and "sighing fits," during which she "felt lost"—(*petit mal?*)

In the early part of 1861, the fits recurred more frequently; so that she was obliged to give up her employment as occasional nurse. Feeling low-spirited, and despairing of aiding her family, she came to the hospital. She was rather below the middle height, of florid complexion, with a wild staring expression, and very excitable manner. The heart's action was somewhat feeble, the breathing tranquil, and the bowels apt to be somewhat relaxed. Pressure of the crowd in the waiting-room, or the anxious struggle to be amongst the first served with medicine after seeing the physician, would sometimes bring on a seizure. One I particularly remember was most violent, and lasted a long time. Although the true fits, or, as she always termed them, the "struggling fits," were principally confined to the pre-menstrual or post-menstrual epochs, yet they occasionally followed coition, as I learned from the husband, who, by the way, has been a noted pedestrian in sporting circles during the past twelve years.

I first gave turpentine and caster-oil draughts; but no worms were dislodged. Bichloride of mercury with bark was next tried; and remembering that valerian had been mentioned by some authorities as putting in a claim for approval, I tried it in many forms—infusion, ammoniated tincture, valerianate of iron, of zinc, and of quinine. Dr Duncan Gibb, who was about that time my colleague, had been giving the bromide of ammonium to allay irritability and decrease sensibility of the larynx and pharynx prior to using the laryngoscope. Thinking the same drug might act beneficially on the mucous lining of the uterine organs, I administered it freely to my patient; but with no good effect. The same was the case with iodide of potassium; and I then accidentally refreshed my memory by reading Sir C. Locock's paper on the bromide.

From the 21st November 1862, to the 3d December, R. B. took bromide of potassium in four-grain doses three times a-day, with compound tincture of valerian. Between these dates she had two fits of a mild character. The medicine was intermitted until the 9th, when she came in great distress, saying that she was suffering from severe flooding, and had had several violent fits, on one occasion nearly falling into the fire. She was ordered to resume the medicine.

2*d Jan.* 1863.—To leave off the tincture of valerian, and take ten grains of the bromide in water three times a-day.

8*th.*—Two severe attacks.

16*th.*—No seizure since last entry.

She now took quinine and ammonia up to 6th March. The seizures were very frequent during a portion of this time, and she suffered severely from menorrhagia.

From the 13th March to 12th May she took ten grains of the bromide three times a-day, and much improved. In fact, she relied solely on the medicine, and declared that the fits would return directly she left it off. To test this statement, during the latter half of July, I merely prescribed a placebo of aromatic water; but so great was her distress that she implored me to allow her to resume the original medicine, and during August and September she took fifteen grains of the bromide three times a-day; I afterwards increased the dose to a scruple. Beyond this dose I never found occasion to go with the patient, and, from my experience in other cases with larger doses, I am compelled to differ from Dr M'Donnell when he says that ten grains three times a-day is "too small a dose to develop any good result."

In January 1864, I discharged my patient, she not having experienced a severe fit, or one of the *petit mal* description, for more than three months. She resumed her former work of nursing, and I saw nothing more of her until the 10th of June last, when she came to me complaining of vertigo and "fulness about the throat," the "choking aura," which was the sure precursor of a seizure at a menstrual period. Two days later the seizure occurred, but it was of a very mild type. I kept her on scruple doses of the bromide up to the 15th July, seeing her during this time on six ocasions. She remained quite free from attacks up to the time of her dismissal in July, and on the 3d November I heard that her good health had been uninterrupted. I do not mean to say that she is cured; but in 1861 and 1862 her life was a burden in consequence of frequent attacks, and from these she has enjoyed an immunity for three, four, or five months.

Systematic writers on materia medica have dwelt much on the anaphrodisiac influence of bromide of potassium. I do not think its beneficial influence is to be looked for in a simple lowering of the sexual power. When that power is unduly drawn upon, a degree of irritation is produced which the bromide may calm. But in the case above related, I had the fullest opportunities of learning from the husband that there never had been any diminution in his wife's *empressement.* And to arrive at greater certainty, the husband coming under my care for some trivial ailment during his wife's attendance at the hospital, I took the opportunity of administering the bromide of potassium to him in fifteen and twenty grain doses three times a-day. The information supplied after the lapse of a month satisfied me that here, at all events, the depressing property of the drug was *nil.* Where there is irritation and frequent priapism, the result of onanism or venereal excess, and where there is every reason to conjecture that such irritation is at the bottom of the epileptic seizures, then the bromide of potassium is, I think, of service. Dr M'Donnell has promised us some further observations which it is probable will throw still more light on this important subject.

As confirmatory of the conjecture hazarded above respecting epilepsy in the male subject, I add a few notes of an hospital case which will bring this paper to an end.

Case 2. E. B., aged sixteen, came to the West London Hospital in June last. His appearance is heavy, he complains of loss of memory, is partially deaf, and the pupils are much dilated. At twelve years of age, when working in a gun-factory, he contracted bad habits, which he has continued to indulge up to the present time. Sometimes he has practised self-abuse three and four times a-day. The bowels are costive, and there is a feeling of weight at the vertex. Before he was thirteen years of age he had several fits, which his friends describe to me as most severe, his struggles being extremely violent, and his tongue often bitten through. There was an intermission of eighteen months followed by a fresh series of fits; and he then went to Salisbury Infirmary. The change of air and the medical treatment effected but little good; and when he returned to London he had "twitchings of the face," and fits almost every day. In June and July he was under my care, and I gave him the bromide three times a-day in doses gradually increased to twenty-five grains. The attacks gradually became less frequent, and he was entirely free from them all through August. On the 2d of September, his friends brought him again, as he had suffered from a slight fit during the night. I resumed the bromide; and, after taking it a fortnight he felt better, and had no "twitchings" and no sign of a fit. Up to the present date (21st Nov.) this lad has had no further seizures; and I think it reasonable to regard his improvement as due to the influence of the drug, coupled with obedience to my enjoining a strict abstinence from vicious habits.—*Lancet*, 10th December 1864.

TREPHINING FOR EPILEPTIFORM CONVULSIONS FOLLOWING INJURY TO THE HEAD—DEATH—AUTOPSY. BY DR OWEN REES.

William G., aged 27, admitted 6th April 1864, for epilepsy. Eighteen months ago, when in perfectly good health, he fell from a ship, and struck his head. He became immediately insensible, and was so for some time. Three weeks after the injury he had his first fit. He had never been able to work since.

His speech is somewhat incoherent, and he takes a little time to collect his thoughts before answering questions. He had frequent, almost daily, attacks of epilepsy but they were short. He did not foam at the mouth, and did not bite his tongue. He had severe pain in his head, he seemed lost, and had difficulty in answering questions. There was a scar on the left side of the head, at the part where the head was struck. A blister was applied. Iodide of potassium (gr. iij.) with liq. potassæ (ʒss.) was given three times a-day. He subsequently had an abscess on the neck, but this got well again, and he was discharged relieved, 23d May. The pain in the head was gone, and the fits were somewhat less frequent.

On 1st June, the patient was readmitted, as the fits were again frequent—twice as frequent as before. He seemed to have great pain in the head, and yet could not answer any questions. He got worse after admission, taking little food, and had fits at very short intervals, and almost constantly. This being the case, Mr Hilton was consulted, and it was agreed on, after several consultations, to trephine. This was done, 22d June, at the seat of injury, where the bone was somewhat raised on the left parietal bone, near to the superior longitudinal sinus. The bone was thought to be a little more solid. The dura mater was not opened. Ice was applied to the head. For thirty-six hours the patient had no fits, but after that interval they were more frequent, occurring daily, until the patient's death, four days after the operation. Head symptoms came on, and he died comatose.

Post-mortem.—On removing the scalp the trephine hole was seen in the bone near to the top of the head on the left of the middle line; purulent matter was running through it. On removing the calvaria, a round patch of lymph was seen upon the dura mater, corresponding to the trephine hole. It was situated to the left side of the longitudinal sinus. A probe was used in order to ascertain if there was any laceration of dura mater, and on passing it round the circumference of the patch, it passed through a small ragged opening, but whether the opening was caused by a softening process, or caused by the operation, was not very evident. On turning back the dura mater, acute arachnitis was seen on the left side of the brain. The hemisphere and the arachnoid surface of the dura mater were both covered with a thick layer of purulent lymph, and some of it poured from the arachnoid cavity. The right side of the brain was quite healthy. A careful examination of the brain showed no disease, the only morbid condition being the recent and acute arachnitis. An examination of the calvaria failed to show any marked disease. Over the parietal bone, on the left side, there was a slight projection, and it was thought that in the neighbourhood of the trephine hole the bone was somewhat thick, but this appearance was so slight that probably it would not have been noticed unless especially looked for.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLIV.—MEETING I.

16*th November* 1864.—Dr Warburton Begbie, *Vice-President* of the Society, in the Chair.

I. CANCER OF THE ŒSOPHAGUS.

Dr Warburton Begbie showed a specimen of œsophageal cancer which had extended to the air passages. On looking at the preparation it would be at once seen that the upper part of the œsophagus was the seat of malignant disease, and that there was an opening into the air passage involving the cricoid cartilage and the upper rings of the trachea. The patient, a man, had been in the hospital for some weeks. He first complained of dysphagia, and applied to the Surgical Hospital, but was put under Dr Begbie's care. The disease was recognised as probably cancer of the œsophagus. The difficulty of swallowing increased, and the patient died of inanition, the œsophagus being completely obstructed.

II. FIBRINOUS CASTS OF THE BRONCHI.

Dr Haldane showed a specimen of fibrinous casts of the bronchi, for which he was indebted to his friend Dr Cairns. The subject from whom the preparation was obtained was a man about thirty years of age. He was a stone mason, and had generally been employed in hewing and preparing stone, but latterly had acted as foreman to his employer, and had not worked as formerly. For more than a year he had suffered from cough, worst in the morning, and for some months past had frequently expectorated such casts as were shown. The expectoration was preceded by a paroxysm of suffocative cough which was relieved as soon as the casts were discharged. The casts shown were about the thickness of a crow quill, and divided into branches corresponding to the divisions of the bronchi. They were of tough consistence, and of a greyish-white colour. Dr Haldane had seen the man, and had carefully examined him. He was stoutly made, and with the exception of slight dulness under one clavicle, with a little harshness of respiration, the condition of his chest was satisfactory. The patient had never spat blood, but stated that on one occasion a little blood adhered to the end of one of the casts.

III. ON THE PATHOLOGY OF RICKETS.

Dr Stephenson read a paper on this subject, which will be found at page 611 of the present number of this Journal.

Professor Spence had listened with much interest to Dr Stephenson's paper, but must protest against it being considered that surgeons viewed rickets as a local disease. Liston, long ago, had taught that rickets was a local manifestation of struma, and this was the view generally entertained by surgeons. The disease was essentially one of malassimilation, and the treatment, cod-liver oil, iron, etc., had been directed towards the condition of the system, not to the local lesion.

Dr Sanders alluded to Trousseau's views on the subject of rickets. The theory adduced by Dr Stephenson was ingenious, but it must be remembered it was only an hypothesis. To explain the pathology of rickets, it was essential to investigate minutely the condition of the bones.

IV. THE RELATIONS OF THE WEIGHT AND LENGTH OF THE NEWLY-BORN MATURE CHILD TO ITS MOTHER'S AGE.

Dr Matthews Duncan read a paper on this subject, which will be found at page 497 of last month's number of this Journal.

MEETING II.

Wednesday, *7th December* 1864.—Dr Douglas Maclagan, *President*, in the Chair.

I. ATROPHY OF CEREBELLUM.

Dr Tuke showed a specimen of atrophy of the cerebellum from a man who had been for several years a patient in the Royal Edinburgh Asylum. There had been no symptoms during life to indicate the lesion.

II. ABSCESS IN THE LIVER.

Dr Grainger Stewart exhibited a specimen of hepatic abscess, which had been removed from the body of a man who had been a patient under Dr Bennett in the Royal Infirmary. He had led an irregular life. Had obscure abdominal symptoms prior to his admission on 5th November. In October he had felt a severe pain in the right hypochondrium and epigastrium. About 8th Nov. signs of pleurisy were observed, and about the end of that month, after extensive effusion had taken place into the right pleural cavity, there was sudden, copious, purulent expectoration. Pneumothorax also became established, and the patient died on the 5th of December.

Autopsy.—The right pleural cavity contained some air, and about four pints of bloody pus. The *lung* was compressed and coated with lymph. Its base was adherent to the diaphragm. A communication existed between the lung and the pleural cavity, and a portion of the lower lobe above the adherent part was gangrenous. This communicated with the abscess in the liver by an ulcerated opening through the diaphragm. There was also an ulceration through the diaphragm posteriorly, at a point where the case of the lung was not adherent. The liver was greatly enlarged, and contained two abscesses with thickened walls, each capable of holding half a pint of fluid. They communicated with one another; had perforated the diaphragm, and one of them had partially discharged itself into the cellular tissue behind the peritoneum, and the abscess had extended downwards to the right kidney, and a considerable portion of the cortical substance of that organ had been destroyed.

Dr Stewart thought the succession of changes the following:—Chronic abscesses had formed in the liver. In October, one of these burst into the retro-peritoneal cellular tissue. About 8th October, perforation of the diaphragm, with pleurisy and empyema. Somewhat later, perforation into the lung from the liver and pleural cavity, and the establishment of the conditions found at death.

III. ON MALFORMATION OF THE ORGANS OF GENERATION.

Mr Turner read a paper on this subject, which will be found at page 575 of this number of the Journal.

IV. ON CLEFT PALATE.

Mr Annandale read a paper giving the details of the case in which he had operated for cleft palate, and which will be found at page 621 of the present number of the Journal.

V. ELECTION OF OFFICE-BEARERS.

The following gentlemen were elected office-bearers for the ensuing year:—*President*—Douglas Maclagan, M.D. *Vice-Presidents*—Peter D. Handyside, M.D.; J. Warburton Begbie, M.D.; Rutherford Haldane, M.D. *Councillors*—John Smith, junior, M.D.; Thomas Balfour, M.D.; Robert Bruce, M.D.; R. E. Scoresby-Jackson, M.D.; W. Sanders, M.D.; Alexander Simpson, M.D.; James Cappie, M.D.; T. Grainger Stewart, M.D. *Treasurer*—George W. Balfour, M.D. *Secretaries*—Patrick H. Watson, M.D.; Charles Dycer, M.D.

ROYAL COLLEGE OF PHYSICIANS.

At the Annual Election Meeting of the Royal College of Physicians of Edinburgh, held on Thursday, 1st December 1864, the following office-bearers were elected for the ensuing year:—*President*—Dr John Graham Macdonald Burt. *Vice-President*—Dr David Craigie. *Council*—Dr David Craigie, Dr William Seller, Dr John Moir, Dr Robert Bowes Malcolm, Dr Alexander Wood, Dr William Henry Lowe. *Examiners*—The President, Drs Craigie, Seller, Wood, Douglas, R. Paterson, Wright, Keiller, Pattison, Cumming, Duncan, J. W. Begbie, Haldane, Sanders. *Treasurer*—Dr Samuel Somerville. *Secretary*—Dr Rutherford Haldane. *Librarian*—Dr J. Matthews Duncan. *Curator of Museum*—Dr T. Strethill Wright. *Clerk*—Mr Christopher Douglas, W.S. *Auditor*—Mr Kenneth Mackenzie, C.A. *Under Librarian*—Mr John Small, M.A. *Officer*—Thomas Marshall.

THE MEDICAL WARRANT FOR INDIA.

The following important document has been despatched to the Governor-General of India.

India Office, London, 7th November 1864.

To his Excellency the Right Honourable the Governor-General of India in Council.

Sir,—Para. 1. In continuation of my despatch, No. 152, dated 16th May 1864, and with reference to your reply thereto, No. 242, of the 29th June 1864, I have now the honour to communicate to your Excellency the decision at which her Majesty's Government has arrived regarding the future establishment of medical officers for service in India.

2. In a despatch, No. 82, dated 12th March 1862, the late Governor-General in Council recommended the formation of an amalgamated medical service for the united army of Great Britain and India, and expressed his concurrence generally in a scheme for carrying out such a measure, embodied in a memorandum by Colonel Norman which accompanied that despatch.

3. A very important feature in the working of that scheme was the unavoidable removal of medical officers from employment in India after a limited number of years' service in that country, whatever their employment might be.

4. I was apprehensive that this might be attended with serious inconvenience; but, after much and careful consideration of the subject, I resolved to submit the proposal, with some slight modifications, for the concurrence of the Secretary of State for War.

5. I forward for your information the correspondence which has taken place upon the subject, by which your Excellency will perceive that Earl de Grey objects to the scheme mainly on the following grounds:—

1*st*, That the employment of British medical officers for limited periods in civil situations in India would put a stop to the continuous military training secured under the present system of the army medical department, and considered by him to be essential to the efficiency of the service; that officers so employed might acquire habits of independence inconsistent with a due performance of regimental duties, and would have to return to a regiment at an age when men in general do not easily bend themselves to the practice of subordination required from a regimental officer of inferior rank.

2*d*, His time, it is stated, would have been spent in treating diseases different to a great extent from those of soldiers', and he would have acquired no experience, and have had no occasion to practise that most important part of his duty as an army medical officer, the prevention of disease. He would, in short, not be the officer whom the existing organization of the medical department of the British army contemplated, and whom it is the object of the costly and elaborate system recently established to provide for the army.

3*d*, Earl de Grey further objects, that "regimental duties with native troops are at present of a very different nature from those required in British regiments, and would not afford medical officers the training and experience which the new organization has rendered essential;" and *lastly*, his Lordship observes, that if the amalgamation were once carried out on these principles, it would be extremely difficult, if not impossible, to abandon them hereafter, whatever might be the result.

6. In my reply, I explained to Earl de Grey and Ripon my reasons for not being able to concur in the objections offered by his Lordship; but as I felt satisfied that those objections had not been lightly entertained, I felt that it would be useless to press a reconsideration of the subject.

7. It was further obvious from the correspondence that any plan involving the withdrawal for a time from their regular duty of the officers of the army medical department, would be liable to objection on similar grounds, and her Majesty's Government directed their attention therefore to the possibility of providing for the future demands of the Government of India by the formation of medical staff corps for each Presidency.

8. In order to obtain for the medical service in India the advantage of having its officers drawn from the same source, and having passed the same examination, as those of the British army, and with a view of placing them on a footing of perfect equality, by obliterating as much as possible all distinction between the bodies of officers so employed and those of the British army, it was proposed to form the several staff corps by means of medical officers of her Majesty's service volunteering to join them.

9. With this view, however, it became necessary to obtain the sanction of Parliament to the repeal of that part of the Act of the 16 and 17 of Victoria, which prescribes the manner in which medical officers shall be provided for employment under the Government of India.

10. Her Majesty's Government having failed to obtain the consent of Parliament to a measure proposed with this view, it became necessary to give

up the scheme of a Medical Staff Corps, as proposed, and to revert again to the system of an entirely separate service, as heretofore.

11. The changes which have been recently made in the amount and composition of the military force in India, the transfer of the whole of the European troops to the British army, and the reduction of the native force, render necessary a corresponding reduction in the establishment of medical officers serving under the local Governments, as well as some change in the nature of their employment.

12. One of the objects aimed at in the contemplated union of the British and Indian medical service was the abolition of the double administrative staff in India. This advantage would have been unattainable on the formation of medical staff corps, and is equally so in the continuance of a separate service, as now proposed.

13. It is obvious that a single medical staff for all India is only compatible with one amalgamated medical service, and that the objections advanced by the Secretary of State for War, and which proved fatal to the scheme of amalgamation, must apply with even greater force to any plan that would involve the indiscriminate employment of officers from either service in the supervision of both.

14. The evils that have resulted up to the present time from the employment of a double staff will, it is believed, disappear when the duties of the officers of the two services shall have been entirely separated. The sole objection to such double staff will then consist in its expense. This, I apprehend, will, on a readjustment of the local department of inspection, with a view of meeting the altered condition of the service, be found susceptible of considerable reduction.

15. Her Majesty's Government have therefore determined that the duties of the medical officers of the British army serving in India shall in future be kept wholly distinct from those of the medical officers of the Indian establishment, and that there shall be a separate administrative establishment for each.

16. Under these circumstances, it will be necessary that the administrative establishments in the several Presidencies should be revised, and the duties of the several medical officers of the higher grades redistributed, and I request that you will take the subject into early consideration, with a view of determining the number of inspectors-general and deputy inspectors-general of each service that will be required to conduct the administrative duties of the respective services in each Presidency.

17. In my despatch, No. 152, I authorized your augmenting the number of officers above the rank of assistant-surgeon in Bengal to 152, Madras to 86, Bombay to 69, leaving the aggregate establishment of medical officers in each Presidency the same as before.

18. That establishment is at present as follows:—Bengal, 425; Madras, 243; Bombay, 193.

19. You were informed, in my despatch, No. 291, dated 8th August 1863, that the War Office would take measures for relieving, as soon as possible, the assistant-surgeons of the Indian army from the brigades of artillery and new line regiments, and the same course will hereafter be adopted as a necessary consequence of the transfer of those regiments to the British army, in regard to the surgeons. A reduction in the establishment of medical officers required for service in India will necessarily follow, and I request that the subject of the number likely to be required henceforward in the several Presidencies may receive your early consideration.

20. In making this calculation, you will, of course, take into account the several situations which may be properly filled up by uncovenanted members of the medical profession.

21. It is believed that, in consequence of all appointments to the local service having ceased during the last three years, the aggregate number of medical officers at present borne on the strength of the several Presidencies will be, if anything, below the prospective wants of the service, but I shall

make arrangements without delay for filling up vacancies as they may occur on your revised establishment.

22. The promotion of assistant-surgeons who may hereafter enter the Indian army will be regulated by length of service, and not, as heretofore, by succession to vacancies in a fixed establishment of officers of the higher grades.

23. Assistant-surgeons of twelve years' service from the date of first commission (of which two years shall have been passed in charge of a native regiment), who shall have passed the prescribed examination in professional subjects, will be promoted to the rank of surgeon.

24. The benefit of this rule is to be extended to all assistant-surgeons now in the Indian service, but officers now in the service who may be so promoted will be considered supernumeraries to the establishment of surgeons as recently laid down, so that no promotion by seniority will take place until such supernumeraries are absorbed.

25. You will make the promotions consequent upon this concession from the date of receipt of this despatch.

26. In the despatch above referred to, I informed you that you were authorized to introduce certain changes in the mode of payment of officers of the medical department serving in India, and you were informed that, pending a further communication upon the subject, officers of the British or Indian medical service, holding staff appointments, the salaries of which are consolidated, will continue to draw their salaries as at present; and all officers of the Indian medical department in the receipt of staff salaries, including such as are in medical charge of native regiments, will continue to draw the aggregate amount of pay and staff allowances they now receive, provided it be not less than the rate of pay laid down in the above table for officers of their rank and standing in the service.

27. I have now to inform you that it has been determined that in future all employment on the part of medical officers of the Indian service involving the receipt of special staff salary shall be considered as staff employment, the salaries being in all cases consolidated; and that all appointments, whether civil or military, held in future by officers of the present Indian medical service below the rank of deputy inspector-general, will be alike tenable by surgeon-majors, surgeons, and assistant-surgeons.

28. The salaries of the principal administrative and military appointments are fixed at the following consolidated sums:—Inspector-general, 2500 rs., deputy inspector-general, 1800 rs., whether of the British or Indian medical service; surgeon-major in charge of native regiments, 1000 rs., with 90 rs. for horse allowance in cavalry regiments; surgeon in charge of ditto, 800 rs., with 90 rs. ditto; assistant-surgeon above five years' full pay service ditto, 600 rs., with 60 rs. ditto; assistant-surgeon under five years' ditto, 450 rs., with 60 rs. ditto. But officers now in the Indian service will receive the pay due to their rank as laid down in para. 10 of my Despatch, No. 152, of 16th May 1864, when such pay is in excess of the consolidated salaries above mentioned.

29. Officers who may hereafter be appointed to the Indian medical service will receive pay when unemployed in India according to the following scale:—

Rank.	Years' Service.	Unemployed Pay.		
		R.	A.	P.
Surgeon-major	25	888	12	0
Ditto	20	852	3	7
Surgeon	15	677	6	11
Ditto	12	640	14	6
Assistant-surgeon	10	410	9	5
Ditto	6	392	5	2
Ditto	5	304	14	2
Ditto	under 5	286	10	0

30. The salaries of other medical appointments in the civil and military departments will be revised with reference to the above, and fixed at a consolidated sum; and I request that a report may be made to me upon the

subject with as little delay as possible. In the meantime, the aggregate sums at present received in each case will continue to be drawn.

31. With a view of promoting the efficiency of the service, it has been further determined that the tenure of office by a deputy inspector-general of the Indian service shall, as in the case of inspector-general, be limited to five years; officers being, however, if not disqualified by age, eligible either for employment for a second tour of duty in the same grade, or for employment in the higher grade of inspector-general by promotion thereto.

32. You will follow, in the cases of officers now holding the office of deputy inspector-general, the same course which your Government adopted in 1861, in regard to combatant officers holding certain staff appointments, and permit all such to hold their offices for two years longer, irrespective of the periods they may severally have served in them. Should any officer have served up to the present time less than five years, he will be allowed to complete a full period of seven years.

33. The rank of inspector-general and deputy inspector-general conferred upon officers of the Indian medical service under the Royal Warrant of the 13th January 1860, is to be considered as substantive rank. These officers, on vacating office at the expiration of the five years' tour of duty, will be permitted in future to draw respectively an unemployed salary of 1200 rupees per mensem in the former and 900 rupees in the latter case for a period of six months from the date of their vacating office; after which they will be placed, while unemployed, on the rate of pay laid down in my despatch of the 16th May 1864, for officers of corresponding rank in Europe. These sums, deducted from the consolidated salary, will regulate the moiety of staff salary to be drawn by officers of those grades during absence on sick certificate. The decision reported in the third paragraph of your letter No. 242 of 1864 will cease to operate from the date of publication of the present despatch.

34. With a view to improving the position and prospects of officers of the Indian medical service, it has been resolved to introduce prospectively the following revised pension rules.

35. Officers of the Indian medical service will be allowed to retire on the following scale of pension, on completion of the required periods of service:—

After	30	years' service	in India	£550
„	27	„	„	456
„	24	„	„	365
„	21	„	„	292
„	17	„	„	220

36. An inspector-general, after five years' active employment in India in that grade, will be entitled to retire upon a pension of £350 per annum, in addition to that to which he may be entitled under the above scale.

37. A deputy inspector-general will, after five years' active employment in India in that grade, be entitled to retire upon a pension of £250 per annum, in addition to the pension to which he may be entitled under the above scale.

38. In each of the above cases, six months' absence on medical certificate will be allowed to count towards actual service in those grades.

39. Officers now in the Indian medical service will, on retirement, have the option of pension according to the above rules, or according to those now in force.

40. My attention has been directed on this occasion to the rule regarding the qualification of an assistant-surgeon for promotion to the rank of surgeon required under Clause 3 of the Royal Warrant, dated 13th January 1860. There can be no doubt that this rule, which has been again brought to notice by a recent despatch from your Government, requiring a service of two years in or with a regiment, bears very hardly on many officers, the nature of whose employment precludes their showing the required qualification, and who on entering the service had no reason to suppose that such a regulation would be adopted.

41. These considerations have induced me to consent to exempt from the

operation of the clause of the Royal Warrant all the assistant-surgeons who entered the service prior to its date. It is to be understood, however, that the rule is to be strictly enforced in the case of all medical officers who entered the service after January 1860.

42. In my despatch, No. 152, of 16th May 1864, par. 7, I informed you that her Majesty's Government had determined to modify the instructions given in my despatches of the 31st October 1860, and 30th September 1861, and to permit the period of service qualifying surgeons for the rank of surgeon-major, under that Warrant, to be calculated from date of first commission, including all leave of absence of whatever kind.

43. I have now to inform you that the same principle is to be observed with respect to the grant of honorary rank on retirement, under Clause 14 of the Royal Warrant, and that the twenty-five years' service qualifying for a step of honorary rank, the requirements of the clause in other respects being fulfilled, shall be inclusive of all leave of absence.

44. I have further to inform you, that it has been determined, as in the case of combatant officers of the Indian army, to confer upon the medical officers of that service Royal commissions in substitution for those which they now hold, conferring rank in her Majesty's service in any part of the world. The medical officers of her Majesty's Indian service will not, however, be required to serve out of India, except with their own consent.

45. I have, lastly, to inform your Excellency that assistant-surgeons appointed to her Majesty's Indian service in future will not be called upon to become subscribers to any military or medical funds, and that her Majesty's Government have determined to guarantee to present incumbents on and subscribers to the several medical funds the annuities and pensions to which they are, or to which they, their widows and children, may become entitled from those funds according to the regulations now in force, and at the present rates of subscription. But this guarantee must be regarded as conditional on the absolute transfer of the assets of the fund to the Government, and must not be construed as conveying to any fund, or to any member of it, benefits which are not actually provided for in the regulations as at present in force.

46. The widows and children of medical officers hereafter appointed to her Majesty's Indian service will be granted pensions not less than those to which they would be entitled under the provisions of the Royal Warrant of 15th June 1855.

47. The above measures, which obviously tend greatly to improve the condition and prospects of the medical service in India, cannot be carried out, as your Government will at once perceive, without a heavy expense to the State. It is hoped, however, that the result will be at once to diffuse a spirit of satisfaction and contentment among the officers now in the service, and to secure for the future a certain supply of medical officers of good social position, liberal education, and professional ability, for her Majesty's service in India.

I have the honour to be, Sir,
Your most obedient humble servant,
(Signed) CHARLES WOOD.

QUESTIONS AT THE SECOND EXAMINATION FOR THE DEGREE OF BACHELOR OF MEDICINE,—OXFORD, *December* 2, 1864.

Dr Acland; Dr Chambers; Dr John W. Ogle, *Examiners.*

PATHOLOGY.

Medical, Surgical.

1. Describe the microscopical appearances of typical specimens of encephaloid carcinoma, and fibro-plastic growth.
2. Describe the condition called leucocythemia.
3. Distinguish between what are termed typhus and typhoid fever.
4. What causes may produce general anasarca?

5. Under what several conditions is the urine found to contain (1.) Albumen, (2.) Sugar, and (3.) Pus globules?

6. By what conditions is jaundice produced; and in which may it be considered remediable either by nature or by art?

7. Enumerate the characteristics of what is termed "general paralysis of the insane:" and state the diseases for which it may be mistaken.

8. By what symptoms would you form a diagnosis of hæmorrhage into the spinal cord?

9. Enumerate the varieties of hernia that occur in the abdomen; and enumerate the swellings which may occur in the inguinal and scrotal regions, besides inguino-scrotal hernia.

10. Describe the various modes of growth in fibrous tumours of the uterus.

MATERIA MEDICA, PHARMACY.

1. What officinal preparations of the British Pharmacopœia contain opium, and in what proportions?

2. Mention the chief diuretics used in England, and give the doses of their several preparations.

3. Enumerate the "mixtures" for which directions are given in the British Pharmacopœia and state their composition.

4. What is the composition of the pulvis jalapæ compositus, of the pilula scillæ composita, of the liquor morphiæ hydrochloratis, and of the liquor strychnæ of the British Pharmacopœia?

5. Name the medicines which impart colouring matter to the excretions.

6. What are the preparations of iron in the British Pharmacopœia? For what purpose is ferri peroxidum hydratum used? How is it ordered to be made?

7. What classes of medicines are fitted for preparation by means of decoction and of infusion respectively? How are liquid extracts made?

8. What is the temperature at which linseed, mustard, charcoal, and yeast poultices are to be made?

9. Explain the principle of volumetric analysis.

10. Give the equivalents of the weights and measures employed in the British Pharmacopœia, in French weights and measures.

THERAPEUTICS.

1. Give an outline of the mode of treatment you would employ in a case of acute rheumatic fever.

2. How would you treat a case of pneumonia supervening upon delirium tremens, and what would probably be the result of your treatment?

3. What steps are required by the law of England to be taken before a person of unsound mind can be put under bodily restraint?

4. What would be your line of treatment in a well marked case of pyæmia?

5. Bearing in mind the several causes of uterine hæmorrhage, what ought to be the treatment of the various forms of this affection?

6. What steps would you take if a woman, previously healthy, were seized with convulsions during labour?

7. What measures would you adopt in treating a case of spasmodic stricture of the urethra?

8. What are the indications for the use of, and contra-indications against the use of, elaterium, digitalis, aloes, and copaiba, severally in various diseases?

9. What are poisonous doses of the officinal preparations of opium respectively? How do you treat deep narcotism from that drug?

FORENSIC MEDICINE AND HYGIENE.

1. By what symptoms would you seek to form a diagnosis between intoxication from alcoholic drinks, and concussion of the brain?

2. How would you proceed for the purpose of testing the contents of the stomach of a person suspected of having been poisoned by arsenic?

3. What are the symptoms of a poisonous dose of belladonna and of hydrocyanic acid?

4. What are the symptoms of poisoning by oxalic acid, and what the post-mortem appearances?

5. What evidence would satisfy you that an infant found dead had not breathed?

6. What are the several ways by which a room, or a ward, may be ventilated?

7. What impurities have been detected in the air of sick-rooms?

8. How is typhoid fever said to be propagated?

9. Under what conditions is drinking water usually contaminated by lead?

10. Can you state any diseases which make butcher's meat wholly unfit for human food?

CLINICAL EXAMINATION.

(a) At the Radcliffe Infirmary.

1. Write a report on the cases of , Ward, and , Ward, giving with care the history, diagnosis, and prognosis of the cases, and also the treatment you would recommend. Add such general remarks as occur to you by way of clinical comment on such cases.

(b) At the Museum.

2. Examine (writing a methodical account of your plan of examination) the urine marked A.

3. Describe and sketch from microscopical examination the object marked B.

4. Describe the morbid product marked C.

(Two passages were also given for translation and commentary; one from the Latin of Morgagni, the other from the Greek of Aretaeus.)

LETTERS TO THE EDITOR.

ARMY MEDICAL DEPARTMENT.

SIR,—In the *Edinburgh Medical Journal* for this month, you have done me the honour of inserting the remarks which I forwarded to you, on the Army Medical Department. Thanking you for your courteous attention, I venture to trouble you again on the same subject.—Your obedient servant,

December 1864. CONSTANS.

In discussing the relative advantages and disadvantages of the Army Medical Department, this one point must ever be borne in mind,—As soon as the full-pay service of the medical officer ceases, from that moment all connexion with the State ends; his services are no longer required; and, beyond the mere fact that his name is on the half-pay list, the State is ignorant of the existence of its former servant. In the other branches of the army, the moment a man's name appears on the muster-roll of the field officers, his advancement is steadily progressive; and though age and infirmities may possibly drive him from the tented field to the soft Brussels of "the Senior" or "the Rag," still he is not thereby debarred from holding the colonelcy of a regiment with its £800 to £1000 a-year:—or if he still exhibits sufficient vigour, an appointment as aide-de-camp to Her Majesty, governor of some colony, inspector of cavalry, artillery, or infantry, or a host of other snug berths, all bringing in a considerable increase of daily pay, woo the veteran soldier; consequently, what first at sight might appear to be a very liberal scale of pay and retirement for the medical officer, dwindles down very considerably on a careful comparative examination. It has been urged that the medical officer has his profession to fall back upon after leaving the service. With equal justice might it be said, that a major or a colonel has his profession to fall back on—and so he has; but there is just this difference, that the medical officer is not indebted to the State for having taught him his profession, and the Government very wisely takes advantage of the experience and knowledge of those men whom it has educated, and employs them in a thousand-and-one ways; whereas, with the exception of Enfield, Pimlico, and the eighteen or twenty military prisons, no places under Government are open to retired military medical officers.

Again, we do not consider it altogether fair that the authorities should recognise the existence of a profession in the employé whose period of service has expired, unless it also at the same time acknowledges the fact, that for the practice of that profession the medical man must still be young and vigorous enough to undergo the fatigue, &c., attendant thereon, and consequently allow him to retire at such a period of his service when he may reasonably be supposed to have sufficient energy to overcome all the usual difficulties of establishing himself.

Again, a medical man, having invested a certain amount of capital in his professional education, has a right to look forward to a fair return for the money so laid out. Although the authorities have most wisely conceded to the medical officer the right to obtain such honours and distinctions as are the special insignia of military service—and it is with a proud feeling of satisfaction that we are enabled to point out those who have received such decorations, and to know that their courage and gallantry have well deserved these appropriate rewards: still the connexion of the medical officer with the State is more or less a pecuniary one. It appears to us, that to obtain something like a perfect medical department, the inducements to enter and remain in it should, after a time, be counterbalanced by the inducements to leave it. In this way alone can you ensure, amongst a comparatively young body of men, sufficient circulation to establish a fair current of promotion.

The new Indian Warrant, which will once more speedily raise the Indian medical service to that high standard of excellence for which formerly it was so deservedly renowned, will, by contrast, render the position of the Queen's officer more wretched and deplorable. It must be a subject of endless regret, however, to those who have sincerely at heart the welfare, not only of the profession, but of the troops and the service generally, that a distinct edict has been promulgated on the subject of the amalgamation of the two services.

There is not a doubt that, had the services been united, within a very few years, independent of all changes or improvements, promotion would have become more rapid for all ranks, and that prizes would have been open to the whole body of the army medical officers, which are now to be grasped and held by a select few. Some time would have been necessary for the absorption of a certain number of the seniors in the Indian branch; but the full tide of steps would have rushed in at last, and the position of all the medical officers of the royal army would have been infinitely better than it can ever be under the present circumstances; for it cannot be expected, nor perhaps is it desirable, that a warrant should emanate from the portals of the War Office on the same liberal scale as that which has so recently escaped from the pen of Sir Charles Wood.

The endless petty squabbles which those who have served in India can recall to mind, as constantly occurring between the Queen's and Indian men, will be revived with greater intensity; for what is more likely to kindle the embers of slumbering jealousy, than the fact that now an Indian assistant-surgeon of five years' service draws very nearly the same amount of rupees as a Queen's deputy inspector-general.

There is, however, one vivifying ray, which throws its cheering influence over us as we peruse this warrant, and that is, the convincing proof which it affords of the authorities being keenly alive to the vast importance of this subject; and we will not insult the War Office by supposing that it is altogether insensible to the results which must accrue from this act of the Secretary of State for India, and how the "niggers" will now have the élite of the profession, whilst English soldiers, English mothers, and English children must fain put up with the cheap and indifferent article scraped up from the Irish schools.

That the War Office must have been aware of the impending promulgation of this Indian warrant there can be no doubt, and the only reason that we can suppose for the non-production of a similar document for the Royal Army is, that the points at issue are infinitely more complicated, and involve not merely the subjects of pay and retirement, but the questions of relative rank, mess, position in army list, dress, *et multa alia.* And although delays are unpleasant and proverbially dangerous, still we should rather wait a fair time, and have a perfect warrant, embracing and settling every question. With regard to all the points of relative rank, etc., we know, unfortunately, from sad experience, that whatever determination may be arrived at, few will be perfectly satisfied; but, with reference to pay, etc., we have good grounds for supposing that the following scale would give satisfaction :—

Assistant-surgeons, on appointment, ...	10s.	per diem.
Do. after five years, ...	12s. 6d.	„
Do. after ten years, ...	15s.	„
Surgeons, on promotion, ...	18s.	„
Do. after fifteen years' service, ...	22s. 6d.	„
Surgeons-major, ...	25s.	„

No change to take place in the existing rates of pay of the administrative branches, but certainly some alteration might be made with regard to servants' allowance. It is altogether absurd that a deputy inspector-general of twenty-five years' service should only be allowed the same as an assistant-surgeon on appointment. We would recommend the following scale for this allowance :—

Assistant-surgeons, ...	1s. per diem.
Surgeons and surgeons-major, . . .	2s. „
Deputy-inspectors, ...	3s. „

Retirement optional, after twenty years' full-pay service, on £1 per diem.
Retirement after further service to stand as it does at present.

This rate of retirement, no doubt, at first sight appears very high, and it might be feared that so great would be the inducements for men of twenty years' service to leave, that promotion would be absurdly rapid, and the interests of the service thereby materially injured. By referring, however, to actual figures, such will be found to be by no means likely to occur; for, supposing the whole of the present surgeons-major (and we may put them down in round numbers as 100) were to go, that would just promote at once the assistants of 1854. Well now, take the full average of men arriving at the rank of surgeons-major after that at twenty per annum, and allowing the present average rate of casualties to be fifteen, this would give thirty-five steps annually. At this rate the men now at the bottom of the list of assistant-surgeons might reasonably look forward to promotion in fifteen years, but not a day before.

So convinced are we that, even with the above inducements to retire, promotion for the junior branches would still be very slow, that we should most strongly recommend the necessity of strictly limiting the age of deputy-inspectors to fifty-five, and that of inspectors to sixty years.

There is one point more which we should wish to speak about, and that is, the strong objections which have been raised to the term "assistant-surgeon." It has been urged that, as the duties of surgeons-major, surgeons, and assistants, are identical, the term assistant might with propriety be discarded. We do not quite coincide with this view of the case. In civil life, assistants are to be found all over Great Britain, so that professionally the term is not a strange one. In the army, every little difference or distinction of address or title carries with it more weight and importance than in civil life, and the principle of seniority is the alpha and omega of the service; consequently, we would most decidedly approve of retaining the present designation for the very junior medical officer. After six years, when he attains the relative rank of captain, then let him drop the "assistant," and, by way of distinction, the present surgeons might be styled senior surgeons. All these changes should be recognised as distinct commissions, and be notified as such in the Gazette.

As above stated, we should rather wait a while and give ample time for the effective solution of all the difficulties which surround this subject; but it would be a wise and politic step on the part of the War Office, if they could imitate the example of Sir Charles Wood before the commencement of the next session; or at least, if they could, in a semi-official manner, notify that an improved warrant was about to be issued. Such a concession would, especially at this moment, be doubly grateful and well-timed, and would utterly preclude from future discussions all those unpleasant inuendoes about compulsion which are necessarily so distasteful to the military authorities.

THE ARSENIC-EATERS OF STYRIA.

SIR,—The following extract from a letter of date 12th December, addressed to me by Dr Knapp, of Liegist, will probably interest such of your readers as took the trouble to read my paper on the Arsenic-Eaters of Styria in the September Number of the Journal.—Yours truly, R. CRAIG MACLAGAN.

"Especially I give you my heartiest thanks for having sent me your paper on the Arsenic-Eaters, and to this expression of thanks add a few further particulars bearing on your observations, as I believe they will be agreeable to you from your interest in the subject. I naturally observed, during several days, the two men who took the arsenic before our eyes. Mat. Scholer continued perfectly well, there being not the slightest change to be remarked either in his sensations or his appearance. Flecker, during the same day, continued quite well, but he applied the drink-money (as our expression is) too literally, so that he during two days was *unwell*, but the appearances were only those of an immoderate enjoyment of alcohol, not those of arsenical poisoning, as our medicinal-rath, Von Vest, was convinced when he visited me some days after your departure. Since that time I have often had these arsenico-phagi with me, who have sometimes taken the sulphuret, sometimes the arsenious acid.

"Scholer took before me, on the 16th April at midday, seven and a-half grains of arsenious acid, without the slightest visible influence on his feelings, and a portion of his urine of this and the next day I sent to the professor of chemistry at Gratz, Professor Folwarzny, who, as I myself saw, demonstrated with Marsh's apparatus, evident traces of arsenic. At my suggestion, the medical clinik in Gratz will, during eight days, observe such an arsenic-eater, and chemically examine all his secretions and excretions. I have also since then become acquainted with several more arsenic-eaters, and still hope one day to make an anatomical investigation on one who has indulged in the habit during years, and to have the mucous membrane of his stomach examined."

28 Heriot Row, 19*th December* 1864.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*The Modern Practice of Medicine: A Lecture delivered before the Royal College of Surgeons.* By D. RUTHERFORD HALDANE, M.D., F.R.C.P., Physician to the Royal Infirmary.

MR PRESIDENT AND GENTLEMEN,—To endeavour, within the limits of a single lecture, to give a view of the present condition of the Practice of Medicine, might well be considered a presumptuous undertaking; and had I been called upon to address an assembly consisting exclusively of medical men, I should have hesitated to make the attempt. But as the audience which I have the honour of addressing embraces representatives of the other learned professions, I feel that I may be permitted to omit numerous details, and to confine myself to the consideration of those broader results which can scarcely fail to be interesting to every educated man.

The Practice of Medicine may be conveniently divided into three departments; there is, in the first place, that which inquires into the nature of disease, or Pathology; in the second, that which treats of the recognition of existing disease, or Diagnosis; and in the third, there is the division which comprehends the treatment of disease, or Therapeutics. Let us, then, inquire what is the present position of Pathology, Diagnosis, and Therapeutics, respectively.

Pathology has made extraordinary advances during the last half-century; this is to be ascribed principally to the systematic study of morbid anatomy; to the prosecution of microscopic researches; and to the cultivation of experimental physiology. The result has been that a new pathology has been elaborated, or is in course of elaboration. Accurate observation has modified and given precision to our views regarding inflammation and its results. A series of disorders has been traced to the impaction of minute particles of fibrine in distant parts of the vascular system. Much light has been thrown upon various obscure nervous disorders. Fevers, formerly confounded together, have been shown to be different in their causation, their symptoms, and their results. A whole group of diseases dependent upon what are called the degenerations, has been established. An accurate knowledge of the diseases of various special organs, such as the lungs, the liver, and the kidneys, is being obtained. With such results as these, and I have only alluded to some of the most striking, it cannot be denied that the advance of pathology has been remarkable, and that its future prospects are most encouraging. Much still remains to do, but we seem to have obtained a solid foundation on which we may hope that a complete and permanent superstructure will be reared.

The progress of Diagnosis has been not less satisfactory. The great feature of the last forty years has been the introduction of what have been called methods of physical examination. By means of percussion and auscultation, we can distinguish with wonderful accuracy the conditions of the thoracic organs. The application of the microscope to the examination of the blood, of urinary sediments, and of various morbid products, has proved a source of much valuable information. Different forms of paralysis and other kinds of nervous disease can be distinguished from one another. There is, in fact, scarcely a disorder into the recognition and differentiation of which a considerable degree of precision has not been introduced.

But, important, nay, essential, as is a knowledge of Pathology and Diagnosis, it must be borne in mind that these departments are but means to an end. To cure disease and alleviate suffering is the object of the physician, and it is therefore to a consideration of the present condition and future prospects of Therapeutics that I propose to devote this lecture.

If we look back upon the progress of Therapeutics during the last fifty years, we shall find that its advances have been comparatively inconsiderable. Important improvements have no doubt been made, but these have consisted rather in the abandonment of time-honoured, but vicious methods, than in the introduction of new remedial measures. There can be no doubt that the system of bleeding and dosing was formerly carried to excess, and that the *nimia diligentia* of the physician was not unfrequently the most serious complication of the disease. For centuries the treatment of disease was almost purely empirical; and before a rational system of treatment could be established, it was necessary to prepare a pathological basis on which it could be founded. It has often been disputed whether Medicine is an art or a science. The fact appears to be, that like every pursuit which rises above the level of a handicraft, Medicine has a scientific and a practical side; the former embracing the theoretical principles on which its practical application is founded. In the absence of a scientific foundation, a sound practice is therefore impossible. The influence of pathology is only now beginning to make itself felt in the treatment of disease, and that influence has hitherto been manifested rather in indicating the uselessness or dangers of old practices, than in providing us with new methods of treatment.

A fundamental and most important principle of pathology has only lately been clearly recognised; it is this, that diseases are not new and independent entities, but that they are perversions of normal or physiological processes. Formerly, when an inflammation manifested itself, it was regarded as something superimposed upon the organism; as an enemy attacking the fortress of life, which required to be repulsed by the most energetic measures. Its supplies must be cut off by the enforcement of a rigorous diet, and it must be attacked with the heavy artillery of bleeding, mercury, and blisters. But it was not kept in mind that by these measures the garrison was weakened in an equal degree with the enemy, or rather in a greater degree, so that even if the adversary were overcome or retired from the contest, the patient often succumbed owing rather to the severity of the treatment than to the malignancy of the disease. Now, however, we look upon inflammation and other diseases in a different light; we view them as perversions of normal states induced by the operation of some external or internal agency. We endeavour, therefore, to put the part or the system in the most favourable circumstances for the resumption of its physiological condition.

The importance of the *vis medicatrix naturæ* has long been recognised, but it is only lately that the physician has ventured to repose much confidence in it. By the expression *vis medicatrix* we mean that when disease exists, there is generally a tendency to spontaneous recovery. Thus, when symptoms of a specific fever manifest themselves, we believe that they are due to the introduction into the system, and the subsequent multiplication of a morbid poison. The result is a disturbance of the organism, which endeavours to eliminate the poison, the presence of which is incompatible with a healthy condition of the body. If the poison prove too strong, death ensues; but if, as generally happens, the powers of the system are sufficient, the poison is eliminated, and recovery takes place. The object of the physician in the treatment of fever should, it is now recognised, be exactly the same as in the case of poisoning by an ordinary toxic agent. His first endeavour should be to get rid of the poison before the system has been brought under its influence; this, however, he cannot do. He rarely knows that the poison has been introduced until its effects upon the system have declared themselves. Besides, he is ignorant of the channel by which it has entered, and he therefore does not know in what manner to apply his remedies. Failing this, he should endeavour to neutralize the poison, to render it inert. But, as the nature of the fever poison is still unknown, he cannot give antidotes, which he can often do successfully in the case of the ordinary poisons. It only remains for him, therefore, to preserve the system as far as he can from the effects of the noxious agent; to favour nature in her endeavours to eliminate the poison; and to help her to get rid of excrementitious matters, the retention of which within the organism would be a source of additional danger. This, no doubt, may seem a comparatively humble office, yet, it is of no mean importance.

The old maxim used to be "starve a fever." Excellent treatment if it could be directed against the fever only! but, unfortunately, the patient was too often starved in the process. Dr Graves, to whom Medicine is much indebted for the introduction of a rational dietary in the treatment of fever, when going round his convalescent ward one morning, and expatiating on the healthy appearance of some who had recovered from severe typhus, remarked to the students who accompanied him, that it was all the effect of good feeding; "and lest," said he, "when I am gone, you may be at a loss for an epitaph for me, let me give you one in three words:—

"HE FED FEVERS."

This feeding of fevers, and indeed of other diseases, is one of the greatest improvements in modern therapeutics; and now our wonder is that the practice should ever have been different. In health, a constant waste of tissue is going on, which must be made up by the introduction of new material, that is, by food. In fever, this waste goes on far more energetically than in health, as is evidenced by the rapidly advancing emaciation. In addition, a poison is circulating in the blood, disordering every function, and tending to cause death by its depressing effects upon the nervous system. We are now satisfied that it is irrational to combat such a condition by the withdrawal of food, which, if necessary in health, must be doubly so when the body is exhausted by disease.

At present, therefore, it would appear that the physician rather endeavours to put his patient in the most favourable condition for recovery, than that he attempts actually to *cure* disease. The instances in which the physician attempts to cure are few; and in these cases, even when his treatment is successful, he cannot tell *how* it has acted, because he has prescribed it empirically. The best example we have of the cure of disease, is the success which generally follows the use of quinine in intermittent fever, but of the mode of operation of the remedy we are ignorant, and the use of the drug in continued fever, founded upon a presumed analogy between the two diseases, has proved unsuccessful. The physician has, however, a twofold duty to perform. His first endeavour should be to arrest death, failing this, he must do his best to relieve suffering. With the latter object before him, he is often called upon to treat symptoms, although he may be ignorant of the essential condition of the abnormal state on which they depend. Pain, sleeplessness, cough, and various other symptoms he is often enabled to relieve, and in doing so, he sometimes makes an impression upon the actual disease.

To sum up, therefore, I would say that the present state of the Practice of Medicine is this:—The physician seldom attempts to *cure* disease; he endeavours to place and maintain his patient in the most favourable condition for recovery; and he treats symptoms, although he often does not know on what they essentially depend. "Restorative medicine" is a favourite expression in the present day; and no doubt it is a great improvement upon the systems of practice which have preceded it. But I shall presently endeavour to show that restorative medicine is not enough, and that the physician must hold out to himself a loftier aim, which there are good grounds for believing he may be enabled to attain.

The question now arises: how has this great change in the Practice of Medicine been brought about? The answers which have been returned have given rise to much controversy, and opinion is still divided in regard to it. Some maintain that the change in practice has resulted from a change in the character of disease; they assert that diseases are of a less sthenic type than formerly, and that the severe depleting treatment previously enforced was judicious at the time, although in the present day it could not be borne. Others are of opinion that disease is the same now as it ever was, and that the change in practice has been the result of the advances of pathology and diagnosis. I do not think, however, that either of these answers furnishes a satisfactory solution of the question.

That a certain change in the type of disease has taken place, I believe to be probable, although I do not think that this explains entirely, or even to any great extent, the change which has occurred in practice. Even at the time when the antiphlogistic regimen was most strictly enforced, there were practitioners who maintained that such treatment was injudicious, and that it was more likely to weaken the patient than to cut short the disease. On the other hand, Bouillaud, one of the few living representatives of the old style of practice, laughs at the idea of a change of type, and goes on bleeding *coup sur coup*, with the same heroism as formerly. Various causes have doubtless concurred to bring about a certain change in the human constitution, at least in this and other civilized countries. Towns have increased disproportionately to the general increase of the population, and the tendency is still to the crowding together into vast centres of masses of inhabitants. Employments are probably on the whole more unhealthy than formerly, at least many persons have been taken from rustic pursuits, and out-of-door labour to work in manufactories and warehouses, which are often unhealthy in consequence of deficient ventilation, floating particles of dust, or the disengagement of noxious fumes. A change has taken place in the food of the people; tea and coffee have to a great extent taken the place of milk, meal, and other highly nitrogenized articles, and although the food of the population has on the whole decidedly improved, this amelioration has not been universal, and has been accompanied by certain disadvantages. The use of tobacco has increased to such a degree, that an influence upon the health of the community cannot but have been exerted. Finally, the world is far busier than formerly; competition was never so keen as it is at present; the minds of whole classes of the community are kept in a state of continual tension;

wealth has increased, and luxurious habits have augmented in at least a corresponding ratio.[1]

These various influences have, on the whole, had a debilitating effect, and it seems fair to conclude that a certain change has been produced upon the character of disease. But, I do not think that the change has been by any means so considerable as to account for the revolution which has taken place in practice. Poets have always been prone to sing of the comparative degeneracy of their own day; but the most enthusiastic of the *laudatores temporis acti* never dreamt of such a deterioration in constitutional power as some would have us to believe has taken place within the last generation. A complete change in the type of disease without adequate causes to account for it, seems *à priori* improbable. The study of medicine would become a really hopeless pursuit if we could suppose that diseases could so change their character, that treatment which yesterday was judicious, should to-day be useless or injurious. It is chiefly in regard to bleeding that there is said to be an intolerance in the present day; but the same might as well be asserted of blistering. Blisters are now applied far less frequently than formerly, but I doubt whether the warmest supporter of the change-of-type theory would seriously maintain that their use has been comparatively discontinued because they do not produce the same good effects as formerly, or because they exert too powerfully depressing an influence upon the constitution.

That the recent advances in Pathology and Diagnosis have had something to do with the change in practice, I fully believe; although, I consider, that these alone are insufficient to account for it. Indeed, I think that the change had begun independent of Pathology, and though Pathology has done much to foster, it cannot be said to have initiated it. I think we must in great part refer the change which has taken place in treatment to a more general cause than has hitherto been assigned for it.

During the present century, a great change has come over the current of ideas and the course of public opinion. A spirit of independent inquiry has gradually been aroused. Doctrines, which had maintained their ground for ages, have been rudely assailed, and have in many cases been cast aside. There is no department of knowledge which has not been affected by this spirit. The History of Greece and Rome, accepted without question for centuries, has been subjected to a scientific criticism, and many of its most romantic portions have been shown to be little more than fables. The dogmas of Theology no longer obtain that unhesitating assent which they formerly commanded, and we see the minister of religion compelled to give serious consideration to the objections of the man of science. Even the most uncompromising maintainers of the inspiration of the Scriptures now admit a latitude in the interpretation of passages, which not long ago would have been sternly refused. In Art, we have seen a school of painting arise which, rejecting all authority, has refused to be guided by the example of the great masters, and has declined any teaching but that of nature. It would have been strange had Medicine escaped the influence of this tendency; in fact, there were special reasons why this could not have been the case. An incorrect Pathology, handed down in various forms from generation to generation, had impressed on medical practice a needless severity. Treatment was too lowering; medicines were given in much too great quantity, and often in very unpalatable forms; while the physiological effects of large doses of active drugs were anything but agreeable. It was therefore not surprising that, in the existing state of public feeling, a reaction should have taken place. Everything was ripe for a change, and, as usually happens, the wave of public feeling seemed about to pass over to the other side. A new system of treatment, based upon a principle having a certain degree of plausibility, and professing to cure diseases by the administration of infinitesimal doses of medicine, was announced, and soon attracted a large body of adherents. Now, although the results of this system were enormously and often dishonestly exaggerated, enough remained to satisfy both the public and the medical profession that, under the influence of diet and regimen, recoveries took place from maladies which had hitherto been supposed to require energetic treatment. On neither side was the lesson thrown away. The public declined to be dosed with the same assiduity as formerly. The reflecting physician saw an important experiment performed before his eyes, which taught him that nature when placed in favourable circumstances was equal to the cure of various diseases. Another circumstance which tended to shake the practitioner's confidence in drugs, was the increasing attention paid to morbid anatomy. It was found that organic disease was far more frequent than had been supposed, and the physician could scarcely hope to find remedies which should exert any influence upon parts which had undergone important material changes. Not unnaturally, this produced in his mind a feeling of despondency, for he did not bear sufficiently in mind, that on the dissecting-table he did not see disease itself,—he only witnessed its results; that he found the organs in a condition which had proved incompatible with the life of the individual,—a condition over which medicine could be expected to exercise but little control.

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[1] See *Health and Disease*, by Dr Edward Smith, p. 373, *et seq.*

The result of these and other causes has been, that in many minds a scepticism regarding the value of all medicine has been engendered, and that the treatment of disease has in some quarters degenerated into pure expectancy. Those who have faith in the efficacy of remedies must allow that the Practice of Medicine is at present in a transition state; it has been going through a phase through which it was necessary that it should pass; but we have a right to expect that it shall vindicate for itself a position more exalted than that which it at present holds. Accordingly, I now proceed to inquire whether it is not reasonable to expect that Medicine may ultimately acquire the power of *curing* many diseases.

I may at once state my belief, that if this power is ever to be attained it will be by the discovery of agents which shall act in a specific manner upon the different tissues and organs of the body. Here, however, I must remark, that I do not use the term *specific* in the sense in which in Therapeutics it is generally employed. When we say that quinine is a specific in ague, or that lemon-juice acts specifically in scurvy, we mean that these agents act upon the system in some unknown manner and overcome the disease. We may indeed theorize as to their *modus operandi*, but what it is we do not know, and our only warrant for employing them is, that experience has shown that in certain morbid conditions their curative powers are to be depended upon. It is not with this meaning that I here use the term specific,—which, indeed, in the instances I have alluded to, is much the same as empirical,—but I employ it in a physiological and strictly scientific sense. When I say that I hope to see the introduction of specific modes of treatment, I do not mean that we should spend our time, most probably fruitlessly, in a search after drugs which shall exert a mysterious curative influence on disease, but that we should seek for and discover remedies which shall act in a specific manner upon the organism, and which shall modify the nutrition and the functions of particular parts. One of the greatest improvements in Pathology is, as already stated, the recognition of the principle that diseases are not distinct entities, but that they consist in the perversion of normal vital processes. If, then, we can modify in a definite manner the nutrition of the part or organ which is the seat of the abnormal process, it is evident that we shall have made an important step in our efforts to relieve it.

But to prove that such a scheme is not Utopian, and that we may fairly anticipate the introduction of such a method of treating disease, I must now endeavour to show that many facts in physiology clearly indicate that specific affinities exist between certain organs and certain substances.

My first illustration of the truth of this doctrine may be derived from what is known of the history of nutrition. Everything leads us to believe that specific affinities exist between the tissues and the circulating fluids. The capillary circulation is carried on, or at least is materially assisted, by the existence of these affinities or attractions. Unless the blood be in a healthy condition relatively to the tissues, and unless the tissues be in a normal state relatively to the blood, retardation or stagnation takes place. The much-contested cause of the stagnation of the blood in an inflamed part appears to be that the tissues have been put in an abnormal condition in consequence of the operation of an irritant, and the healthy relation which should exist between them and the blood has been disturbed. On the other hand, in asphyxia, the blood stagnates in the capillaries of the lungs because, from the deficiency of oxygen, it has not been freed from carbonic acid, and therefore no longer bears its normal relation to the pulmonary textures.

Again, all the tissues of the body are nourished, directly or indirectly, by the blood. The composition of the blood is essentially the same in all parts of the body; yet, tissues having the most various chemical composition, derive their nutriment from it. The brain requires a regular supply of phosphorus and fats; the muscles require albuminous materials and salts of potash; the bones require phosphate of lime; the hairs require a small quantity of iron. Every organ is constantly undergoing disintegration, and unless the loss be as constantly supplied, the integrity of the part would suffer. To provide for this continuous waste of material is the purpose of healthy nutrition; and as all must be derived from the blood, we can only suppose that it is because certain organs have a peculiar affinity for substances which enter into their composition, that these are deposited in particular situations. We can explain this selection in no other way than by saying, that the power of extracting from the blood what is necessary for its support, is an inherent property of living tissue.

The same principle is illustrated in the case of secretion. The purpose of secretion is either to purify the blood by the removal of materials which have served their purpose, and have become useless and deleterious, or to provide substances which shall have an active part to perform in the processes of nutrition. The secretions of glands, the structure of which is apparently much the same, are very different. The renal and hepatic cells do not differ very widely; yet, how different is the urine from the bile. The peculiarities in the secretions of the various glands cannot be explained by any difference in the arrangement of the bloodvessels; we are therefore led to look to the epithelium as the essential element upon which the activity of the gland depends. And we can only conclude, that the healthy epithelium of the different glandular structures exerts upon certain materials contained in the blood an elective affinity or attraction in virtue of which they are withdrawn from the circulating fluid. This doctrine of the affinity of the glands for particular elements holds good, not only when all is going on regularly, but is even more strikingly illustrated when certain foreign substances are taken into the body. Salts, when introduced into the stomach are, if soluble, absorbed and eliminated by different channels. The special channels by which they are eliminated vary according as there is a special affinity between the substance introduced and particular glands. Some substances appear capable of being eliminated by all the glands of the body; thus, if iodide of potassium be swallowed, it may be detected in all the secretions, in the saliva, the urine, the pancreatic juice, and the bile. Yet, even here, all the glands do not stand in precisely the same relation to it; iodine being more readily given off by the salivary glands than by any other secreting apparatus in the body.[1] The channels for the elimination of other substances are often more limited. Yellow prussiate of potash, for instance, is not given off by all the secreting organs. It is eliminated freely by the urine, in less quantity by the gastric juice, while it cannot be detected at all in the saliva. Iron, again, is seldom found in the secretions. When administered internally it is absorbed, though in extremely small quantity; sufficient, however, is taken up to modify, either directly or indirectly, the constitution of the blood. The whole, however, of what is absorbed is not retained in the blood, it cannot become accumulated beyond very narrow limits, and it has been a question by what channel it is eliminated. Recent observations have shown that it is given off by the bile, which appears to stand in a close relation to the colouring matter of the blood.[2] In these and similar cases we must suppose that specific affinities exist between substances introduced into the body and certain secreting organs, in virtue of which affinities the channels for the elimination of the foreign material are determined.

The same principle is illustrated, if possible in a more striking manner, in the mode of action of various poisons. Poisons may act in one or other of two ways. Those which are very prompt and energetic in their action appear to produce their effects upon the nervous system; those, again, which operate more slowly seem to act by modifying the nutrition, and thereby producing abnormal conditions of other parts of the body. In either case, however, (except in the case of those poisons which act chemically), it is necessary that the toxic agent be absorbed into the blood. Those poisons which, when taken into the blood, act most energetically upon the nervous system, produce no effect when directly applied to it. Prussic acid when swallowed, or when placed upon the conjunctiva, kills almost instantaneously; it has, however, been applied with impunity to the denuded brain of the horse. Not only must poisons be absorbed into the blood, they must, to produce their effects, pass into the arteries in order that, through the medium of the capillary circulation, they may be brought in contact with the elements of the tissues. The poisonous properties of sulphuretted hydrogen are well known; it is said to prove fatal when respired in the proportion of one eight-hundredth part. The gas is absorbed into the blood from the air-cells of the lungs, passes into the arteries, and produces its toxic effects upon the brain. When introduced by another channel, so that it has an opportunity of being eliminated before reaching the arteries, it may, however, prove innocuous. Water containing a large proportion of it may be drank with impunity; and it is related of a chemist who had a predilection for this substance, that he was in the habit of drinking a saturated solution of it. In this case the gas is absorbed by the veins of the stomach and intestines, passes into the vena porta, then into the inferior vena cava, thence into the lungs, by which it is so completely eliminated that none remains to pass into the arteries and produce injurious effects upon the brain.[1]

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[1] See Claude Bernard, Leçons sur les Propriétés Physiologiques, etc., de Liquides de l'Organisme, tome i. p. 447.

[2] Claude Bernard, *loc. cit.*, p. 448.

Seeing, then, that poisons do not in general act locally, but that they must, to produce their effects, be absorbed into the blood, let us now endeavour to discover whether their mode of action is not regulated by specific affinities which exist between them and certain parts of the body. Beginning with the poisons which act most rapidly, let us take woorara, strychnia, sulphocyanide of potassium, and nicotine, and inquire whether we cannot, by the aid of experimental physiology, discover what are the special parts of the system which they affect.

The *Woorara* or *Woorali poison* is employed by certain native tribes in South America for poisoning the arrows which they use, either in war or in the chase. It is harmless when taken into the intestinal canal, but is a most deadly poison when introduced into a wound. The following are the phenomena which characterize its action:—Shortly after the introduction of the poison the animal falls down, apparently without suffering, and without uttering any cry; occasionally, though not generally, slight convulsions show themselves. The conjunctiva is at first sensitive, and when it is touched the eyelids close; the animal, however, soon loses the power of closing the eyes, although the sensibility does not appear to be abolished. The pupils dilate, the sphincters become relaxed, and the respiration ceases. The animal is now apparently dead, but the heart goes on beating and continues to do so for two or three minutes, when the contractions become weaker and gradually cease. During the period while the heart goes on beating, it is impossible to produce any reflex movement. When the body is opened, the blood is found black from imperfect oxygenation. The poison has proved fatal by abolishing the respiratory movements. When galvanism is applied to the nerves, no movements of the corresponding muscles are elicited; reflex actions we have seen are abolished; but the muscles retain their properties, for, when directly galvanized, they contract. Now, the action of this poison is absolutely confined to the nervous system; but we can go farther, and say that it acts exclusively upon the motor nerves, and that it kills by paralyzing the muscles of respiration. The heart goes on beating after motion in the other muscles has been abolished, because its movements do not stand in the same relation to the nervous system; finally, it stops, in consequence of failure of the respiration; but if artificial respiration be kept up for a sufficient time, the effects of the poison go off, and the animal recovers.[2]

[1] Claude Bernard, Sur les Effets des Substances Toxiques, p. 57.

[2] Ibid., p. 305, *et seq.*

Strychnia, the chief active principle of nux vomica, is well known as a violent poison. When introduced by any channel into the body, the animal speedily becomes rigid, or is thrown into violent convulsions. Death occurs either during a tetanic spasm, or is due to asthenia, and in the former case appears to be the result of asphyxia, occasioned by rigidity of the muscles of respiration, and their consequent inability of movement. The mode of action of strychnia is peculiar; it does not act as a direct excitant of the nervous system, but it exaggerates excessively the reflex functions of the spinal cord, so that the slightest irritation produces violent tetanic convulsions. And it appears to produce the augmentation of the vitality of the spinal cord in two ways,—*1st*, By increasing the amount of blood in the spinal cord, by paralyzing the muscular coats of the vessels which supply it; and, *2d*, By acting in a special manner on the tissue of the cord.[1]

When *Sulphocyanide of Potassium* is injected into the cellular tissue of an animal it speedily becomes paralyzed, the action of the heart ceases, and if the dose of the poison have been small, voluntary motions may be observed, even after cessation of the contractions of the heart. Artificial respiration has no effect, because paralysis of the heart has been occasioned by an influence exerted directly upon itself. This is the reverse of what we saw to be true in the case of woorara, and experiment has proved that sulphocyanide of potassium destroys by contact muscular irritability, and that it does not affect, at least directly, the nervous system.[2]

The last of the poisons which I mentioned was *Nicotine*. This alkaloid, derived from tobacco, is one of the most deadly poisons known; a few drops placed in the eye of an animal kill it almost instantaneously. When the foot of a frog is examined under the microscope, the circulation is seen going on in the well-known manner; if now the animal be poisoned by nicotine, the arterial system becomes immediately emptied, in consequence of powerful contraction of the coats of the arteries. The heart, however, continues to beat, and the stoppage of the circulation seems the result of an effect produced upon the bloodvessels themselves. The explanation of this phenomenon is to be found in the result of experiments performed upon the sympathetic nerve. When the sympathetic is cut in the neck, the parts supplied with branches from it become the seat of a more vigorous circulation, in consequence of the coats of the bloodvessels which had been paralyzed by the section of the nerve becoming dilated and allowing more blood to pass through them. If, on the contrary, the cut end of the sympathetic be galvanized, an opposite effect is produced,—the circulation in the part is diminished, because the coats have contracted under the application of the stimulus, and the calibre of the vessels has consequently been diminished. This second effect is precisely what is produced by nicotine; and, accordingly, it seems highly probable that the fatal effects of that poison are due to an action on the great sympathetic, and, through it, on the vascular system.[1]

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[1] See Brown-Sequard, Lectures on the Principal Forms of Paralysis of the Lower Extremities, p. 50.

[2] Claude Bernard, Sur les Effets des Substances Toxiques, p. 350.

Here, then, we have four of the most deadly poisons which prove fatal by acting on the nervous system, but in which the phenomena and the mechanism of death are essentially different. Are we not, then, justified in believing that each of them has acted upon a different part of the nervous system?

We see the same tendency to localization in the case of those poisons which do not act so much through the nervous system as on the structure of the organs themselves. We know, for instance, that arsenic acts upon the stomach, through whatever channel it is introduced. Mercury, whether swallowed, inhaled, or introduced by inunction, exerts a peculiar effect upon the mucous membrane of the mouth, and on the salivary glands. Lead produces a peculiar form of paralysis, by injuriously modifying the nutrition of certain muscles.

The last source of evidence to which I shall refer is derived from what we see in the case of the storing up within the body of certain metallic substances. It is well known that some poisonous substances may remain in the body for a length of time, without giving rise to any markedly injurious effects. We must suppose that they have formed insoluble compounds, and that, as they can no longer find their way into the arterial circulation, their poisonous properties cannot be exerted. And accordingly, if they again become soluble, poisonous effects again manifest themselves. But such substances are not stored up at random within the body. Certain organs have a peculiar affinity for certain substances, and in virtue of this affinity the particular substance finds its way to a definite locality. Thus, arsenic has a peculiar affinity for the liver; lead, for the liver and muscles; mercury, for the bones. When nitrate of silver has been taken for a length of time, it occasionally produces a peculiar form of discoloration of the skin, in consequence of the metal having been deposited in its tissue; in rarer cases a similar deposit is found to have taken place in the kidneys, in the form of a blackish-blue coloration of the membrane of the coils of vessels in the Malpighian tufts, and in the intertubular stroma of the medullary substance.[2]

I have thus endeavoured to show, from a variety of considerations, that specific affinities exist between particular substances and certain parts of the organism, and that these affinities may be the means of exerting an important influence upon the nutrition of the part. We are, therefore, justified in believing that the same principle holds good with regard to medicines generally; and a necessary corollary from this is, that medicines, to manifest their full energy, must be so administered as to exert their influence upon particular organs. I believe that the carrying out of this principle will exert a most important effect on the progress of Therapeutics. Instead of acting upon the system as a whole, we wish to concentrate our treatment upon the diseased locality. We all know how much more efficacious is topical than general treatment, when the former is possible. At present, in treating pneumonia, we combat diseased action in the lungs by means which only affect these organs in common with the whole system; but if hereafter we discover an agency which acts specifically upon the lungs, it is evident that we shall not only act more powerfully upon the seat of the disease, but that we shall save the general system from unnecessary interference. Suppose we were endeavouring to put out a fire in an isolated chamber, whether would it be better to fit up fire-engines on the outside, and play upon the building until we had inundated it completely, or to enter the apartment, and with a single well-directed bucket of water, succeed in extinguishing the still limited conflagration?

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[1] Claude Bernard, *loc. cit.*, p. 399.

[2] Virchow, Cellular Pathology, p. 213.

I am aware that it may be by many regarded as Utopian to anticipate a time when we may expect to cure disease by modifying directly the nutrition of the diseased part; but I must say that I do not regard this prospect as by any means hopeless. I may, indeed, give one example, in actual practice, of the application of the principle.

Paraplegia, or loss of power in the lower half of the body, may depend upon various causes. Shortly after the peculiar effects of nux vomica and strychnia in stimulating muscular action had become known, they were introduced as therapeutic agents in various forms of paralysis, and among others, in paraplegia. The mode of action of the drugs was not known, and it was soon found that while some cases improved under their use, others were not beneficially affected, or became worse. Experiment has, however, shown that strychnia acts by producing, through the vaso-moter nerves, paralysis of the walls of the bloodvessels, which consequently yield to the pressure of the blood, become dilated, convey more blood to the spinal cord, with the effect of exaggerating its nutrition, and thereby exalting its reflex property. Accordingly, in cases where paraplegia depends upon an inflamed or congested condition of the cord—that is, in conditions where there is already an excess of blood—it is found that strychnia aggravates the existing paralysis. On the other hand, we have in ergot of rye a substance which produces the opposite effect upon the bloodvessels. We know that ergot acts upon the uterus, causing powerful contraction of its non-striated muscular tissue. In like manner, it produces an exciting or stimulating effect upon the muscular walls of the bloodvessels, causing them to contract, thereby diminishing their calibre, with the effect of limiting the supply of blood to the cord. And we find in practice that in cases where paraplegia depends upon the presence of an excess of blood in the nervous centre, ergot of rye may be given with the best effects, while strychnia would only have acted in aggravating the malady. These, Gentlemen, are not theoretical statements,—they are well-recognised therapeutic results. This example also illustrates the absolute necessity for a sound pathology and an accurate diagnosis to guide us in our choice of therapeutic agents. Unless we are thoroughly acquainted with the various morbid changes which may affect the different organs of the body, and unless we are able by an analysis of symptoms to determine on what special morbid action the symptoms depend, our treatment must often be worse than useless. This is especially true regarding the specific treatment of which I have been speaking; for just in proportion to its advantages when judiciously applied must be its dangers when abused.

I think it is probable that if the system which I have been endeavouring to describe be ever carried out, our most valuable remedies will be found to be those which act through the nervous system. All the most recent observations and experiments go to show, that the nervous system exercises a most important influence on nutrition both in health and disease; and as our object should be to attack disease in its very earliest manifestations, it must be through the nervous system that we should endeavour to influence the nutrition of the part, before any serious organic lesion has been established.

It will naturally be asked, how we are to attain to a knowledge of the specific properties of medicines, and of the circumstances in which they will be useful. My answer is, by *experiment.* And it must be borne in mind that it is not sufficient that we are acquainted with the physiological effect of a medicine, that is to say, with its effect on the healthy, it is essential that we become familiar with its action in disease. Strange as it may seem, some violent poisons act less readily on sick than on healthy animals. If equal quantities of cyanide of mercury be introduced into the stomach of a healthy dog, and of a dog weakened by disease, the healthy animal will be killed almost immediately, while the other will live for a considerable time. The reason is, that in the healthy animal the metallic cyanide has been decomposed by the acid of the gastric juice, and prussic acid has been set free; while in the stomach of the enfeebled animal there was such a deficiency of gastric juice that the decomposition could not take place, and death did not occur until the cyanide itself had been absorbed.[1] All medical men know how, in particular diseases, there is a marked tolerance or the reverse for certain substances; how, in cases of abdominal inflammation, what would ordinarily be poisonous doses of opium may be taken with impunity; while, in diseases of the kidney, a few grains of calomel may produce dangerous symptoms. Therefore it is that the real therapeutical experiments must be conducted by the clinical physician. The experimental physiologist may have demonstrated the action of a substance upon the healthy, but it is only at the bedside of the sick that those researches can be made which will teach us the *curative* powers of medicines. In this point of view, Hospitals have a peculiar value. It is chiefly from large institutions, where disease is seen on a great scale, and where the subjects of observation are placed under similar external circumstances, that important therapeutical results can be expected. The establishment of Hospitals has done much for the science and practice of Medicine, but it is perhaps in regard to Surgery that their value has been principally appreciated. Medical Pathology and Diagnosis have doubtless profited largely, but the therapeutical results have hitherto been comparatively inconsiderable. The value of well-conducted Hospitals is generally pretty well acknowledged by the public, and I feel sure that, even in a selfish point of view, their supporters will be amply repaid for their liberality. This form of charity is in an especial manner "twice blest;" not only is good done to those for whose benefit the institutions have been primarily established, but opportunities are given for the advancement of medical knowledge, the benefits resulting from which cannot fail to be experienced by the donors. In connexion with this subject, I gladly allude to the Royal Infirmary of Edinburgh, which has done so much to relieve suffering in this city, and indeed through all Scotland, and which has proved itself so invaluable an adjunct to our Medical School.

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[1] Bernard, *loc. cit.*, p. 103.

This institution has peculiar claims upon the public; though not, like many other Hospitals, richly endowed, and though to a great degree dependent on its annual subscriptions, it shows a liberality in the admission of patients unrivalled, I believe, in the kingdom. To obtain admission within its walls no Governor's order is required; disease and suffering constitute the only claim, and one which, so far as its accommodation permits, is never refused. The building, which has existed for a hundred and thirty years, is in some respects in arrear of modern requirements; but I am glad to know that there is a prospect of a new one being constructed; and I feel sure that the Managers may proceed boldly in the matter, confidently relying on the liberality of their fellow-citizens.

In bringing these observations to a conclusion, I have only a single remark to make. Therapeutics, in its future progress, may or may not follow the direction I have ventured to indicate, or it may advance by some other path; but of one thing I feel certain. Medicine is in itself too noble a pursuit, and its cultivators are a too devoted body, to allow us to entertain any doubt as to the ultimate result. It cannot be expected that Medicine shall ever take rank with the exact sciences, for where we have to deal with the phenomena of life, vitality introduces a disturbing element which we can never expect to overcome; but we may safely look forward to a period when we shall be enabled to undertake a more equal struggle with disease, — a period when the success of the physician shall be more nearly than at present on a level with his good intentions.

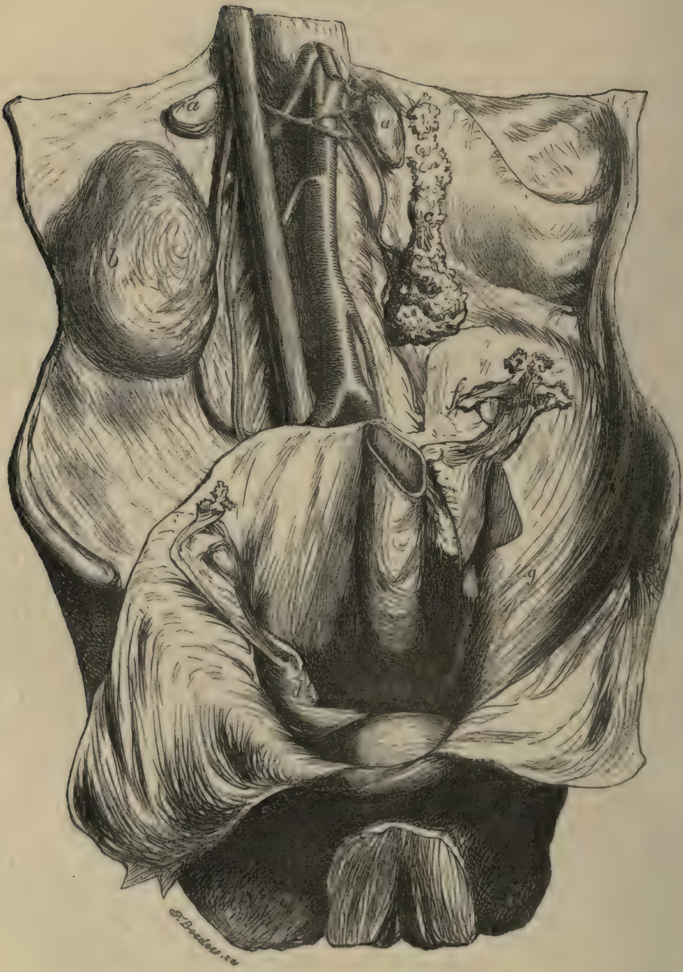
ARTICLE II.—*On some Malformations of the Organs of Generation.* By WM. TURNER, M.B.Lond., F.R.C.S.E., Senior Demonstrator of Anatomy, University of Edinburgh.

(*Continued from page* 581.)

CASE 2.—*Uterus Unicornis.*

THIS example of a very rare malformation of the uterus was met with in the body of a woman, past the middle period of life, which was admitted into the dissecting-room early in the present session. When the coils of the small intestines were drawn on one side, it was noticed that the uterus did not possess its usual form and position; and on a careful examination, the following arrangement of parts was observed:—The uterus was situated in close relation to the inner wall of the right side of the pelvis, its upper end being three-fourths of an inch below the pelvic brim. It was smaller than usual, its length being one inch and a-half, its greatest breadth one inch and a-quarter. Its shape differed from that of the normal uterus, the left margin being convex, and the upper margin almost pointed. From the latter the ovary, with its ligament, the Fallopian tube, and the round ligament of the right side only sprang. The organ was invested by peritoneum, which was reflected from its anterior and posterior surfaces to the pelvic wall, without the intermediation of a broad ligament. From its form, position, and connexions, it was evident that the uterus in this person consisted solely of the right uterine cornu. The right ovary was situated partly at the pelvic brim and partly in the right iliac fossa. It looked shrivelled, and on its surfaces were faint traces of scars. The Fallopian tube also reached into the same fossa; the peritoneum surrounding it and the ovary was continuous with the peritoneal lining of the fossa. The fimbriated end was connected to the ovary by a slight fibrous band. The tube was pervious, and could be readily inflated. Between the layers of its peritoneum were faint traces of the organ of Rosenmüller. Attached to the left margin of the uterus, close to the junction of the body and cervix, was a flattened band, not more than three-eighths of an inch in diameter at its widest part, which passed across the back of the bladder six-eighths of an inch above the line of reflection of the peritoneum, from the rectum to the bladder. From the bladder it extended to the inner surface of the left pelvic wall, up which it proceeded as far as the pelvic brim, where it terminated in a free stump-like extremity. This band, although readily recognisable at its uterine and free extremities, could with difficulty be traced along the bladder and pelvic wall, for it appeared to be little more than a mere thickening of the peritoneum in those localities. From the position and connexions of this band there can be no doubt that it represented the left uterine

Fig. 2.



Note of Explanation to Fig. 2.—Uterus Unicornis. *a*, *a*. Supra-renal capsules. *b*. Right kidney. *c*. Mass of fat in left lumbar region. *d*. Uterus unicornis. *e*. Left ovary; the rounded structure *g*, immediately below, is the stump-like free end of the left uterine cornu. *f*. The Fallopian tube. This figure is engraved from a careful drawing of the specimen by my pupil Mr Richard Caton.

cornu, atrophied in this case almost to complete disappearance. The left ovary was connected by a short ligament, proceeding from its lower end, to the stump-like end of the flattened band, and it possessed a meso-arium one inch in length, which was continuous over the line of the external iliac artery, on the one hand with the peritoneal lining of the pelvis, on the other with the serous covering of the left iliac fossa. The ovary was thin, its surfaces perfectly flat, and with no trace of scars. Between the folds of its meso-arium a brownish mass, probably the organ of Rosenmüller, was situated. The left Fallopian tube was two inches long, and completely detached from the atrophied left uterine cornu. It was connected to the upper end of the left ovary by the pointed extremity of a triangular-shaped fold of peritoneum, one and a-half inches long, which permitted the tube to be readily moved about, and even drawn as far upwards as the iliac crest. Its fimbriated end was well marked, the mouth open, the tube pervious. The opposite end was closed, and formed a rounded cul-de-sac. The left round ligament was connected to the stump-like end of the atrophied left uterine cornu.

The hymen was absent. The vagina, three inches long, readily admitting three fingers, and directed obliquely to the right. Into its upper end the os and cervix projected. There was a slight bulging of the vagina, to the left of the cervix. The pelvic surface of the posterior wall of the vagina was covered by peritoneum. Lips of the os uteri smooth. The cavity of the right uterine cornu and cervix measured together two inches in length.

The rectum was in its normal position. The left colon descended over the surface of the psoas. The superficial surface of the left quadratus and iliacus was invested by peritoneum, which had therefore an extensive relation to the abdominal wall on this side. When the colon was removed, the left kidney was found altogether absent, the position of that organ being in part occupied by the gut, and in part by a mass of adipose tissue. The left supra-renal capsule was of ordinary size. From the absence of the left kidney there was no renal artery or vein on this side, but a vein formed by the junction of the left capsular and ovarian veins with one of the lumbar veins crossed the front of the aorta in the usual position of the left renal vein to join the inferior vena cava. The right kidney and capsule were normal, and it could not be said that this kidney was hypertrophied. There was no appearance of inflammatory affection of the peritoneum either in the pelvis or abdomen proper. The pelvis was symmetrical, and possessed the usual dimensions met with in the female.

CASES 3 AND 4. *Uterus Bicornis Unicollis.*

Two specimens of this malformation of the uterus have now come under my notice in the dissecting-room,—one three years ago, the other early in the present session. The women in whom they were found were advanced in years. In both specimens the pelvic peritoneum exhibited evidence of an old inflammatory attack. In Case 3, a number of fibrous bands passed in the antero-posterior direction across the pelvic cavity from the bladder to the rectum, and in their course were attached to the uterus, and more especially to a small calcified fibrous tumour connected with its right cornu. In Case 4, the ovaries and Fallopian tubes were situated in the iliac fossæ, the right ovary being attached to the under surface of the meso-cœcum by adhesions, the left to the under surface of the sigmoid meso-colon. The peritoneal investment of the uterine cornua and of the rectum was rough and flocculent.

The uterine characters in both specimens corresponded in so many particulars that they may in a great measure be embraced by a single description. Vagina, os externum, and cervix uteri undivided. Body of uterus separated into two distinct cornua, each about two inches long, which, diverging widely from each other, stretched almost transversely across the lateral diameter of the pelvis, so that the angle formed by the junction of their inner margins was very obtuse. Each cornu was somewhat ovoid in form, the extremity representing the larger end of the egg being the furthest removed from the cervix. Each cornu had a special opening

Fig. 3.

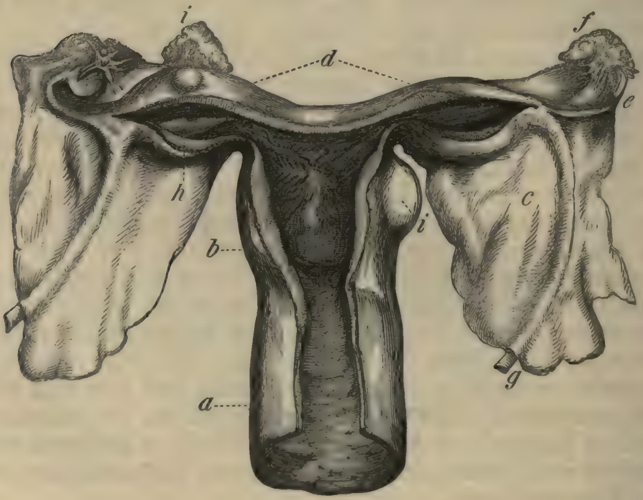


FIG. 3 represents Case 3. — Uterus Bicornis Unicollis. *a.* Vagina. *b.* Cervix uteri. *c.* Broad ligament. *d.* Uterine cornua. *e.* Fallopian tube. *f.* Ovary. *g.* Round ligament. *h.* Uterine polypus. *i, i.* Fibrous tumours.

The four specimens described in this paper are now deposited in the Anatomical Museum of the University of Edinburgh.

into the upper and outer angle of the canal of the cervix on its own side; the cavities of the horns extended throughout almost the entire length of their respective cornua. The hymen was absent, and the mucous membrane of the vagina comparatively smooth. The lip of the external os smooth and without fissures. The ovarian ligaments were connected to the outer end of each uterine cornu posterior to the place of origin of the Fallopian tubes. The round ligaments arose from the outer ends of their respective cornua, and then passed outwards between the folds of the broad ligaments.

Case 3 is the only one of the two specimens which has been opened, and in it the canal of the cervix was seen to be one inch in length, and its folds or plicæ sharp and well defined. Projecting into the cavity of its right horn was an elongated polypus, connected to the uterine wall by a very short stalk proceeding from its upper part. When examined microscopically with a power of 200 diameters, this polypus was seen to be composed partly of connective tissue, and partly of tubular glands, the latter resembling in form and anatomical characters those glands which constitute so important an element in the uterine mucous membrane.[1] The Fallopian tubes attached to the outer end of their respective cornua curved upwards, outwards, and then inwards; their fimbriated ends being in close contact with, and united by adhesions to the ovaries. The ovaries were deeply and frequently scarred. The meso-arium was much thickened by adhesions, more especially on the right side, so that the condition of the organ of Rosenmüller could not very satisfactorily be made out; traces of it on the left side were, however, noticed. The orifice of each tuba amidst the fimbriæ was dilated, and the tubes were widely pervious for about two-thirds of their extent; but at their uterine ends they seemed to be quite closed, for I was unable to pass either a probe or fine bristle from them into the uterine cavity. Embedded in the substance of the left side of the cervix was a small fibrous tumour, and similar tumours about the size of peas were situated in the anterior and posterior walls of the right cornu. Projecting from the same horn was a calcified fibrous tumour about the size of a hazel nut, which was firmly connected to the rectum by strong adhesions.

In Case 4, the outer end of the right Fallopian tube was so closely united to the ovary by adhesions that the fimbriæ were no longer recognisable. On the left side, a cyst about the size of an ordinary playing marble was developed at the outer end of the ovary, and to its wall the fimbriated end of the tuba was so firmly connected that its mouth was occluded. The thickened condition of the peritoneum around the ovaries, and the adhesions between them and the meso-cœcum, and sigmoid meso-colon obscured the form and external characters of those bodies, but they were apparently atrophied. The bladder had a remarkable pyriform shape; its apex, formed by a portion of the unobliterated urachus, reached for nearly 3 inches above the os pubis. The pelvis was smaller than is usual in woman, the diameters of the inlet being as follows: Conjugate, 4 inches; transverse, 4½ inches; oblique, 4¾ inches.

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[1] For an excellent description of the gland structures met with in some forms of uterine polypi, I may refer to an essay by Rokitansky: Ueber, Uterus-drüsen-neubildung in uterus und ovarial sarcomen: Zeitsch. der Gesellschaft der Aerzte zu Wien, 10th September 1860.

I have not been able to obtain any information respecting the history of the three women in whom the above specimens of malformed uteri were found, so that I cannot state if they were ever pregnant. From the absence of cicatrices in the lips of the os, and the sharpness of the folds of the mucous membrane of the cervix in Case 3, it is probable that she had never given birth to children. That such malformations are not necessarily incompatible with pregnancy, if the ovaries are properly developed and the Fallopian tubes pervious, is, however, sufficiently proved by numerous recorded cases. Dance, Robert Lee, Thilo, and others, have related cases of impregnation of the right horn; Rokitansky, Cruveilhier, and others, cases of impregnation of the left horn; and Hohl and Generali, cases of impregnation of both horns, and consequent production of twins in the uterus bicornis. The case of Generali is one of much interest, for the presence of twins in the uterus was diagnosed before the labour commenced, and a month's interval elapsed between the birth of the two children. The case was regarded at the time as one of superfœtation dependent on the peculiarly malformed condition of the uterus. On the death of the woman, thirty years after, in the year 1847, Generali found the body of the uterus divided into two lateral halves, to each of which a Fallopian tube was connected.

Again, the uterus unicornis, in which the second horn was completely wanting, has been met with in the impregnated state by Chaussier and Chiari, and in Chaussier's case (related by Dr Granville in the Philosophical Transactions for 1818), the woman had on one occasion given birth to twins. Amongst the examples of uterus unicornis in which a rudimentary second horn existed, Stoltz has related a case in which the woman had had both male and female children in the developed horn; and Dionis, Canestrini, Ingleby, Heyfelder, Rokitansky, Scanzoni, Luschka, and some others, have related cases in which impregnation had taken place in the cavity of the rudimentary horn itself.[1] Several of those last named were at first supposed to be pregnancies of the Fallopian tube, and in them rupture of the rudimentary cornu and extension of the fœtus into the abdominal cavity took place before the sixth month of intra-uterine life.

The various forms of uterine malformation which have now been recorded by different pathologists has rendered necessary some system of classification under which the different varieties may be arranged. Two of the most convenient of these systems are those employed by Dr Arthur Farre in his excellent article "On the Uterus and Ovaries,"[1] and by Kussmaul in his exhaustive treatise "On Deficiencies and Malformations of the Uterus."[2] Case 2 belongs to Dr Farre's second group, and Cases 3 and 4 to his third group; whilst their position in Kussmaul's system of classification is sufficiently indicated by the names I have employed in the course of my description. Indeed, the figure 23[3] which Kussmaul gives, from a specimen sent to the anatomical museum at Heidelberg by F. C. Naegele, may be considered to afford a fair representation of my specimens of uterus bicornis unicollis. The term uterus bicornis is indeed much to be preferred to that of double uterus, so often applied to this particular kind of malformation; for the latter term does not, like the former, accurately express the nature of the mal-development, and conveys, moreover, an inexact conception of its morphology. Both specimens completely bear out the remark which Rokitansky makes when describing the different forms of uteri possessing two cornua, that the closer the junction of the two horns takes place to the cervix or os externum, the more obtuse is the angle at which they unite, and the wider is the fissure which exists between them.

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[1] Abstracts of all the cases mentioned above, excepting the cases of Stoltz and Luschka, are given by Kussmaul in his Treatise on Malformations of the Uterus. Stoltz and Luschka's cases are recorded in the Monatschrift für Geburtskunde, vol. xvi., 1860, and July 1863.

The number of specimens of uterus bicornis, more or less modified in different instances, which have now been recorded in the adult human female, is so considerable, that one may almost regard it as one of the most common forms of uterine mal-development. It does not appear to affect the reproductive functions of the organ, and it frequently exists without any malformation of other parts of the body. In both my specimens, the body and the other viscera were perfectly well developed, excepting that, in Case 4, the pelvis was somewhat below the average size, and the urachus was not completely obliterated. And as in part offering an explanation of this, it must be remembered that the uterus bicornis represents not only a stage which the human uterus goes through in its progress to its perfect condition, but one also which is the persistent form in many of the mammalia. At the third month of intra-uterine life of the human fœtus, the uterus presents the two-horned condition. The two Müllerian ducts from which it is developed have at that period only so far blended together at their lower ends as to form an undivided vagina and cervical canal. After this period, if the process of development advances, a gradual blending of the two horns takes place, so as to form the undivided body of the uterus, and the only portions of the ducts of Müller which remain separate from each other, are their upper ends, which constitute the

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[1] Todd's Cyclopædia,—Supplement.

[2] Von dem Mangel, &c., der Gebänmutter. Würzburg, 1859.

[3] A drawing of the same uterus is also given by Martin in his Hand Atlas der Gynakologie und Geburtshülfe. Taf. xxxi., fig. 5. Berlin, 1862.

Fallopian tubes. Hence these cases of uterus bicornis, instead of exhibiting a doubling of the uterus, really present a separation of the single organ into its two primary halves.

The uterus unicornis is not only a more unusual and more serious form of mal-development of the organ than the uterus bicornis, but it appears to be very frequently associated with an imperfect condition of other parts of the body though in it: other things being favourable, all the processes of gestation may be carried on. Thus, it has been found conjoined with deficiencies in the lower limbs and one half of the pelvis, as in cases of monopodia and sympodia. But it may also, as in Case 2, exist in persons well-formed externally, but with a remarkable irregularity or defect in some of the internal organs, more especially in the urinary apparatus. In cases related by Stoltz and Rokitansky, the urinary bladder as well as the uterus was one-sided. In a preparation in the University of Freiburg, described by Kussmaul in his treatise, p. 112, the left kidney was situated in the left posterior part of the cavity of the pelvis, and the pelvis itself was contracted and small in all its diameters. In cases recorded by Chaussier, Rokitansky, Stoltz, and others, as in the specimen I have described this evening, the kidney on the affected side was altogether wanting; in most of these cases it was noted that the kidney on the opposite side was hypertrophied, but I cannot say that any hypertrophy of the right kidney existed in mine.

It very seldom happens that the atrophy of one of the uterine cornua occurs so completely as in Case 2. Not only has the canal of the left Müllerian duct been in a great measure obliterated in that case, but the duct has almost entirely disappeared in some parts of its extent. It is probable, however, that both of the ducts of Müller had assisted in the production of the vagina; the width of that tube and the existence of the cul de sac at its upper end to the left of the cervix uteri tend to that conclusion. Whether or how far the left duct had assisted in the production of the cervix it is more difficult to determine. On the back of the bladder and inner side of the left pelvic wall, the position of the Müllerian duct was indicated merely by a thickening of the peritoneum. At the left pelvic brim the remains of the duct were represented by a short, stump-like, free process, which evidently represented the outer end of the uterine cornu, for both the short ovarian ligament and the round ligament of the uterus were connected to it. The upper or Fallopian end of Müller's duct was, however, pervious, but quite detached from the uterine portion of the structure, the atrophy having been so great at their original line of junction as to effect a complete separation between them. Its position, almost on a line with the posterior part of the iliac crest, was an approximation to the primary seat of the duct.

We must look for the mode of production of this serious defect in uterine and renal development to some cause operating at a very early period of embryonic life in the immediate neighbourhood of the Wolffian bodies; for though neither the uterus nor the kidneys take their rise from these structures, yet the Müllerian ducts from which the former springs, and the latter organs at their first appearance, are in such close proximity to those bodies, that circumstances tending to produce atrophy of the one, are not unlikely to affect the other, and that this is the case is shown by the frequent conjunction of renal and uterine atrophy in the same individual. And, further, in the male, as in the case I first described this evening, a partial atrophy of the kidney may exist in conjunction with a complete atrophy of the structure at the head of the Wolffian body from which the globus major of the epididymis is produced. The perfect condition of the supra-renal capsule, both in my case, where the kidney was absent, and in the one described by Stoltz, bears out in a striking manner the statements made by embryologists of the complete independence of the kidney and the supra-renal capsule in their mode of development.

ARTICLE III.—*Case of Acute Strumous Laryngitis.* By ROBERT TURNER, M.D., Keith.

(*Read before the Medico-Chirurgical Society, 7th December* 1864.)

IN the *Association Medical Journal* for the 28th of July 1854, an instance is related of (what I venture to term) *Acute Strumous Laryngitis*, as one of a series of cases illustrative of certain views on the pathology and treatment of laryngo-tracheal inflammation advanced by me in the three preceding numbers of the same journal. The account of the after-progress of this case which I am now enabled to give will, I trust, be found sufficiently interesting to justify me in again bringing it under the notice of the profession.

To render the narative complete, I premise a condensed statement of the case from its commencement down to the period at which the published report was taken.

The patient, then a young woman of twenty, is one of a family in which the scrofulous diathesis is strongly marked: three of its members having died of pulmonary consumption within the last twelve years. She first came under my professional care in March 1849, for a deep and rather extensive ulcer of the right leg, situated about its middle, and occupying the space between the bones. The accompanying constitutional disturbance was considerable, being evinced by profuse night-sweats of regular recurrence, small, rapid pulse, and loss of appetite, flesh, and strength. There was no appreciable irregularity of the form or movements of the thorax, but a slight degree of dulness was perceptible on percussion of the left clavicular and infra-clavicular regions; the breath and voice sounds also being here somewhat harsh; and expiration was audible and rather prolonged in the last-named situation. The presence of tuberculous deposit, of limited amount, in the upper lobe of the left lung was, therefore, probable; but there was neither cough nor expectoration, nor pain in any part of the chest; and the respiration, although quickened, was not more so than seemed a necessary accompaniment of the accelerated action of the heart, and of so much emaciation and debility. The pulmonary lesion, then, whatever its nature or extent, appeared to be quiescent.

Under the use of cod-liver oil and iodide of iron, with a nutritious diet, and the application of stimulating lotions, strapping, etc., to the sore, a gradual cure was accomplished.

About the begining of autumn 1850, the girl again applied to me,—the malady on this occasion being an affection of the right ankle-joint. There was effusion into the cavity of the articulation, as well as considerable enlargement of the surrounding tissues, and inversion of the foot. The same constitutional treatment was again employed: a small issue was established under the external malleolus, a splint was applied to the inner aspect of the leg and foot, and the limb was bandaged. A tedious recovery from the affection of the joint resulted, but without a corresponding amendment of the general health, which continued in a very unsatisfactory state, passing from bad to worse throughout the greater part of 1851. The symptoms, as noted about the begining of winter of that year, were:—"Short, dry cough; shifting pains of the chest, most complained of under the clavicle, and in the upper scapular region of the left side; feeble and frequent pulse, and hurried breathing. There is a marked falling off in flesh; two or three periods have passed without the return of the catamenia; and the patient is now confined to bed."

The physical signs, observed at the same time were:—"Percussion-sound dull over and under both clavicles, but dulness more intense and extended at left side; bronchial respiration and bronchophony distinct in the upper part of the chest on both sides, but loudest in left infra-clavicular region."

Intercurrent bronchitic attacks succeeded, with profuse but difficult expectoration, at which times the cough was almost incessant, and she frequently vomited her food. The employment of cod-liver oil, with syrup of iodide of iron,—which, contrary to my advice, had been discontinued when the patient regained the use of her limb,—was resumed and persevered in, except when an urgent attack of bronchitis required the suspension of these remedies and the substitution during its continuance of expectorants and sedatives. Naphtha was administered for the irritability of stomach, and with apparent benefit. Steady counter-irritation to the upper part of the chest at both sides, with a carefully regulated, rather generous diet, formed the rest of the treatment. For several months, the unpromising state of matters described remained unchanged; but at length tokens of improvement began to appear. The bronchitic invasions became less frequent and severe,—the general health also undergoing a gradual amelioration. About the end of July (1852), the patient was able to be out of bed the greater part of the day, and even to take an occasional short airing in mild weather. Her cough was then decidedly less troublesome than it had been since the commencement of her illness; the "stitches" were no longer complained of; all hectic symptoms had left her; the appetite was much improved; there was already a palpable increase of flesh; and the menstrual discharge, which had again appeared, was recurring with considerable regularity. The physical signs did not seem now of graver import than in 1849, and, in short, an evident arrest of the pulmonary disease had taken place.

In October, November, and December of 1852, the girl was again under my care, along with three other members of the family, for fever of a mild typhoid type, which ran its course even more favourably with her than with any of the others. Abeyance of the phthisical symptoms continued,—indeed, throughout this illness I very rarely heard her cough. Early in December, she was convalescent, and at my last visit (on the 7th), I found her better, in all respects, than I had ever before seen her. Her pulse was little above 80, and of tolerable strength.

From that date I saw or heard nothing of my patient until the 13th of the month following (January 1853), when I received an urgent summons to visit her without delay. This, my engagements at the time did not permit me to comply with, and other aid was procured; but, on the 15th, I was again sent for, and on arriving, early in the afternoon, found her in a most precarious state,—affected with acute laryngitis in an advanced stage. The practitioner who had been applied to, on the 13th, not being now in attendance, an imperfect detail of the previous history of the attack was supplied by the patient's relatives, to the effect that, after unusual exposure to the night air about the 10th or 11th, she had been seized with catarrhal symptoms, with some huskiness of the voice, and short, dry cough; but that the laryngeal affection did not set in, or excited no apprehension, till the morning of the 13th. In the afternoon she had been bled from the arm, and the upper part of the chest had been blistered, with partial but very transient relief to the breathing. Sometime after, a draught of infusion of senna had been given, and almost immediately rejected. The vomiting and nausea thus induced had procured another slight and temporary remission of the dyspnœa, but afterwards the case had proceeded uninterruptedly, and with progressive severity, up to the time of my visit. The patient's distress was now very great. The prolonged, stridulous inspirations, so loud as to catch my ear as I entered the house; the small, rapid, and irregular pulse; the livid, tumid face; the fixed, prominent eye-balls; and the cough, occurring in paroxysms threatening asphyxia; bore ample testimony to the extremity of her danger, and at once satisfied me that so scanty an access of air to the lungs was incompatible with the continuance of life beyond, at most, a few hours. Bronchotomy had therefore, I believe, become an indispensable preliminary to all further treatment; although, taking into account the antecedents of the case, as well as its severity and duration, the expedient appeared to be one of but slender promise. It was proposed, and consented to with the utmost alacrity. A delay of more than an hour took place in consequence of my having to send home for my instruments, and even within this brief space the affection underwent an appreciable aggravation. Respiration became alarmingly irregular; the pulse intermitting, and exceedingly depressed and rapid; the face, neck, and chest were bedewed with a cold, clammy perspiration; and sight, hearing, and consciousness were fast failing. The trachea was now opened, the incision dividing its two or three upper rings, and a tube of medium size inserted. After some rather severe fits of coughing, with ejection of bloody mucus through the tube, respiration became comparatively free. A bit of gauze was bound over the tube, and a temperature of 60° Fahr. was directed to be maintained in the apartment day and night; an opiate was given, and a light, farinaceous diet enjoined. At leaving, I had the satisfaction of seeing my patient easy, tranquil, and even cheerful, the placid expression of her countenance then, contrasting very agreeably with the agony depicted in it but two hours before. It is unnecessary to repeat the details of after-treatment from day to day. It will suffice to state that the tube was removed from the trachea, and the opening allowed to close on the 20th of January, the fifth day after the operation; on the 23d, the wound was granulating, and respiration going on entirely through the natural channel; on the 31st, the patient had been able to be out of bed for more than an hour, the pulse having now fallen to 82, and the respirations to 26; and under the date February 7, the report is as follows:—"Convalescence advancing very satisfactorily. Voice and breathing natural, and the cough nearly gone. Percussion and auscultation give almost the same results as in 1850, and the patient is sensible of an increase of strength. She is now little in bed during the day."

For the succeeding ten months, the corroborant measures already specified and careful avoidance of exposure to cold secured for my client a condition of health to which she had long been a stranger. During this period I saw her only incidentally, except once or twice for a threatening of bronchitis, which an expectorant mixture and a mild counter-irritant to the chest served to avert; but on the 29th of November 1854, the laryngitis suddenly recurred with all its former intensity, setting at nought every effort for its removal without the aid of tracheotomy; and I therefore, on the afternoon of the 30th, again had recourse to this operation, cutting into the same part of the trachea as before. In its subsequent course, this second so closely resembled the first attack that it does not seem requisite to enter into details. Life was again preserved, but otherwise the result was not so completely successful as on the former occasion. Writing in July of 1854, I state:—"The use of the tube cannot yet be dispensed with, although within the last month the glottis has again become in some degree pervious to air; and on withdrawing the tube at my last visit (on the 7th inst.), and stopping the orifice in the trachea, I found that forced respiration could be maintained, with little inconvenience for about two minutes, but no longer. The pulse was then 96, and the respiration (through the canula) 40. The signs of emphysema of the upper portions of both lungs have of late been superadded to the auscultatory phenomena formerly observed. The general health, however, is better, the patient being able to move about within doors, eating with relish, and sleeping without the aid of an opiate. The cod-liver oil is continued, in half-ounce doses, three times a-day."

Throughout the next ten years from this time, the case pursued a steady, if somewhat uneventful course of improvement, the history of which, with the exception of one or two incidents, or rather episodes, may be given in few words. The patient's health and strength slowly, but perceptibly, returned, the bronchitic attacks becoming less and less frequent and severe until they ceased altogether; and, eventually, she was often able to go abroad in fine weather, wearing, as a precaution on such occasions, a modification of Jeffreys' respirator over the mouth of the tube in the trachea.

In the spring of 1856, the girl's constitutional taint again manifested itself locally in the form of an ulcer on the dorsum of the left wrist. A plan of treatment was adopted for this similar to that employed for the deeper and more extensive sore of the same character on her leg, and within a few months it had completely cicatrized.

Early in the following year (1857), *hooping-cough* prevailed in the district where my patient resides. She had escaped this affection in childhood, but now took it, along with two younger sisters and a boy living in the house. The familiar designation of the malady, however, as applied to her case, is a misnomer; there was, in fact there could have been, no *hoop*, the larynx and its belongings having been (literally) cut off from all participation in the semeiological concern. When I first saw her, on this occasion, the distinctive phenomena of the affection were but imperfectly developed. They soon became completely so: the attacks assuming a character as purely paroxysmal, and a rise, progress, and decline of the seizure being as distinguishable, as in ordinary cases. The fit set in with that indescribable feeling of discomfort so often the herald of the approaching tumult in this disease: then came a succession of forcible expiratory efforts, marked by a rapidly *remittent* snoring sound issuing from the canula, which appliance the patient often required to hold with her finger and thumb, at such times, to prevent its ejection. This spasmodic action continued, with the usual accompaniments of livid, swollen face, and shaken frame, until a quantity of viscid mucus was expelled through the tube, and a full, prolonged inspiration, the chest expanding with comparative freedom, announced the termination of the paroxysm. Occasionally, when this was of great severity, and the attack might be said to have reached its acmé, vomiting occurred simultaneously with the mucous expectoration; but I never witnessed when present during a fit, nor could I learn that there occurred in the course of the illness, a reduplication of these symptoms, such as may very generally be observed in *common* pertussis before the paroxysm is at an end. The transition from spastic constriction (for this may surely be inferred) to free permeability of the trachea and bronchial tubes was always strikingly abrupt.

Once or twice, at the commencement of a paroxysm, I exhibited the vapour of chloroform through the artificial opening, and with such effect in lessening the severity as well as the duration of the fit, that my patient pleaded to be allowed to "take this sweet nepenthe" at her own hand in my absence. To this I consented, but limited her to the use of twenty-five or thirty drops of the anæsthetic on each occasion, stipulating, besides, that her brother, the nature of whose employment enabled him to be always at hand, must superintend the inhalations, and at once remove the handkerchief on the slightest warning being given of the approach of unconsciousness.

The girl has often since spoken in the warmest terms of the relief thus afforded her.

The attack ran its course favourably in about ten weeks.

Repeated trials of the patient's ability to breathe through the glottis, made at intervals for two or three years after the second operation, yielded no more promising result than the first attempt before referred to; and she never could raise her voice above a hoarse whisper; so that, in common with several professional friends who saw the case from time to time along with me, I had ceased to indulge any hope of its farther improvement. On meeting the girl by chance, however, in August last, my first interview with her for eighteen months, I was equally surprised and gratified to find that her voice had returned. Although of uniform pitch only, it was astonishingly clear and articulate; and this, notwithstanding the presence in the trachea of so formidable an impediment to vocalization as the bronchotomy tube. She stated that this fortunate change had begun to appear, and the general state of her health to improve, in the course of a residence at the seaside during the summer of last year; and that the amendment, in both respects, had advanced gradually since, having, she believed, been greatly promoted by a second visit to the coast of like duration, and at the same season of the present year. On now withdrawing the canula and stopping the orifice in the trachea, I found that respiration could be carried on in the natural way without the slightest inconvenience, and that my patient could speak aloud distinctly, without apparent effort, and with varying intonation. In these circumstances, a renewed endeavour to restore normal respiration seemed justifiable, and my proposal to this effect was readily acceded to. To make sure of the propriety of the measure, however, a probationary occlusion was decided on, to be followed, should it prove quite satisfactory, by the permanent closure of the artificial opening. A piece of oiled silk about two inches square, its centre covering the aperture in the trachea, was laid over the neck, and coated thickly, as well as a wide margin of the skin around, with a solution of gutta-percha in chloroform. This covering was further supported by strips of adhesive plaster and an elastic band surrounding the neck. The patient was directed to return in a week, and on her presenting herself at its expiry, she was found to have experienced no inconvenience from the trial, the *besoin de respirer* continuing to be fully satisfied by the ingress of air through the natural channel. But as it was evident, on the removal of the dressings, that the opening in the trachea was contracting, it was resolved to delay the ulterior proceeding, and allow this contraction to go on to its *maximum*, in order that the closure might be effected with the least possible extent of wound.

On this and a previous occasion, a laryngoscopic examination was made in the case by my friends Dr J. W. Black, of Edinburgh, Dr A. Macdonald, now House-Physician in the Royal Infirmary there, and by myself, but neither structural change nor imperfection of function in the organ of voice was detected.

27th September 1864.—The opening in the trachea was closed to-day by paring its edges and bringing them together with four interrupted wire sutures, which were supported with strips of adhesive plaster, a compress of lint, and an elastic band round the neck.

3d October.—Sutures removed, and adhesive plaster re-applied. Lips of the wound in perfect apposition, and no suppuration.

5th October.—The wound has healed throughout by the first intention. On examining to-day with the laryngoscope under more favourable circumstances than before, for I was able to reflect on the throat-mirror the rays of a bright noon-day sun, a very distinct view of the interior of the larynx was obtained. I could nowhere perceive any enlargement or loss of tissue, and the vocal cords closed, separated, and vibrated with the natural freedom and rapidity. Their colour, however, forcibly attracted my attention, for they resembled nothing so much as two bits of dingy white paper inserted in the tube, and contrasted strikingly with the hue of the parts around and beyond. How far this blanched appearance may be a departure from the normal aspect of these parts, I must confess myself too little practised in the inspection of the living vocal apparatus to be qualified to decide.

24th October.—The patient, visited by my son to-day, continues well, breathing and speaking with ease. No cough.

Appended to my first report of this case (*Association Medical*

Journal for 1854, p. 667), are the following remarks:—" The great proclivity to laryngeal complication in pulmonary consumption is well known; the researches of Louis having long since established the fact that ulceration of this portion of the respiratory tube occurs once in every four instances. I am disposed to regard the laryngitis in the preceding case as an unusual manifestation of this morbid tendency in its early stage, tuberculous exudation into the tissues which surround the glottis forming the *predisposing cause* of both attacks. The gradual increase of the deposit between the first and last seizures is, I believe, denoted by the circumstance that, although the acute affection has been a second time recovered from, a structural impediment to the entrance of air in sufficient volume by the glottis remains. From a state of complete occlusion, however, this orifice has of late been restored to some degree of patency; and, having seen the adventitious deposit in 'scrofulous finger,' and other localizations of the same diathesis, melt away under the influence of appropriate treatment, I do not despair of yet greater improvement being effected in the present case."

It has already been seen that this favourable prognosis, hazarded in 1854, is now more than realized. The pathological view on which it was based has not, I am aware, the weight of authority in its favour, but is, on the contrary, opposed to the general opinion; although a hesitating assent has been accorded to it in one or two highly respectable quarters. " It seems credible," says Dr Handfield Jones (*Jones and Sieveking's Pathological Anatomy*, p. 147), " that tubercle should liquefy and undergo absorption; but it has been very generally doubted whether this ever actually occurs. Dr Walshe, whose authority is great on this point, believes that absorption, under favourable circumstances, may take place, but acknowledges it to be a rare event." The facts of the case under consideration will, I believe, be found difficult of explanation (to say the least), unless the agency of the mode of repair in question be admitted. For, even keeping out of view the obviously hereditary and well-pronounced dyscrasia in the case, it is, I think, inconceivable that the product of ordinary inflammation could have followed a similar course of deposition, persistence for more than eight years, and subsequent entire removal; although the acute inflammatory attacks, especially the last of the two, may have induced—nay, most probably did induce—increased activity in the process of tuberculous exudation. I look upon the case, in short, as an example of incipient laryngeal phthisis in which acute laryngitis twice arose incidentally, and which has been, in the strictest sense of the term, *cured*. The opinion maintained by some that the ulceration constituting laryngeal phthisis takes place without previous infiltration and softening of tubercle in the tissues involved, and is due simply to contact with the acrid sputa of the accompanying pulmonary disease, I have always considered untenable. If it were otherwise, Why, it may be asked, does such ulceration never attend the non-tuberculous affections of the lungs; where, as in chronic bronchitis, the muco-purulent expectoration is, as a rule, even more abundant and prolonged; or where, as in pulmonary abscess, the fetor, often perceptible in every part of the sick-room, tells unequivocally that the quality of "acridity" is certainly not wanting in the sputa?

On account of the novel circumstances under which it occurred, I watched with interest and attention, and have described with some minuteness, the attack of *pertussis* of which my patient was the subject. It supplied, I conceive, a *demonstration* of what was before only matter of opinion, resting on theory;—viz., that, in the regular affection, spasmodic closure of the glottis is an essential element in the production of the characteristic *hoop*.

In the anomalous circumstances of this case, the production of a paroxysm of the duration and severity described, signifying, as it necessarily did, spasmodic contraction of the trachea and bronchial tubes, is a circumstance not, perhaps, sufficient of itself "to make or shake a creed," but one appearing to lend some support to the pathological doctrine (plausible on other grounds) which assigns *the pneumogastric nerves*, as its sphere of action, to the *materies morbi* of hooping-cough.

KEITH, 2*d November* 1864.

ARTICLE IV.—*Climacteric Insanity.* By FRANCIS SKAE, M.D., formerly one of the Assistant Physicians to the Royal Edinburgh Asylum.

INSANITY occurring in women at the climacteric period, connected with, and dependent upon the physiological changes then taking place, has been noticed by most writers on female diseases as one of the gravest and most important of the morbid conditions which are incident to that time of life. Various psychological authors, as Esquirol, Conolly, Sir H. Halford, Laycock, and others, have also observed the frequency of insanity in women at that period, and have referred to the "change of life" as the exciting cause of a large proportion of those cases of insanity which are met with between forty and fifty years of age. I am not aware, however, that any special monograph has been devoted to the description of this class of cases; and as it forms a large and important natural group, I have thought that the history of a number of cases of climacteric insanity, with their symptoms, treatment, and results, might be of some interest and practical value as a contribution to psychological medicine. With this view I have carefully examined the case books, for eighteen years back, of the Royal Edinburgh Asylum, and having succeeded in collecting two hundred cases of climacteric insanity, I now proceed to a description of the results arrived at from a consideration of these cases.

Age.—Most writers are agreed in stating that the grand climacteric of female life takes place between 40 and 50 years of age. They also admit that there are many exceptions to this; that menstruation not unfrequently ceases before 40; and again, that the function may continue unimpaired up to 55 or 56 years of age. I have accordingly confined myself to cases of insanity occurring between 40 and 50 years of age, with the exception of a certain number of cases in which the insanity could be distinctly referred to climacteric changes taking place either before 40 or after 50.

The following table shows the age at which the insanity commenced in 200 cases:—

TABLE I.

Age.	Number of Cases.
38	2
39	4
40	17
41	6
42	6
43	10
44	7
45	22
46	26
47	14
Carry over	114
Brought over	114
48	18
49	9
50	19
51	3
52	15
53	5
54	5
55	5
56	7
	200

It is seen from this, that the largest number of cases occurred between 44 and 51 years of age.

Symptoms.—Insanity occurring at the climacteric period generally manifests itself at first in the form of depression of spirits, sleeplessness, restlessness, and inattention to ordinary domestic affairs, followed by fear of undefined evil, with suspicion of the patient's most intimate friends and relations. From this the patients gradually pass into a state of profound melancholia, accompanied by various delusions, such as—that they have committed the unpardonable sin; that they are doomed to eternal punishment; that evil spirits constantly surround and haunt them; that their food can no longer nourish them; that they are unworthy to live any longer: they constantly express themselves as weary of life, and this, together with the conviction of their own unworthiness and wretchedness, leads to the development of one of the most dangerous, and at the same time one of the most prevalent of the symptoms of climacteric insanity, namely, suicidal tendency. In a great number of cases paroxysms of excitement alternate with the deep depression and gloomy despondency which are the general characteristics of this form of insanity. In several cases hallucinations of the senses occur, and, in a few, delusions of an exalted character also exist. Many are dangerous to others during the transient paroxysms of excitement, or in consequence of hallucinations of the senses; but distinct homicidal impulse is rarely developed. The bodily condition is commonly weak, although Dr Tilt and others describe an aggravated form of hysteria, amounting to insanity, as occurring at this period of life in connexion with a plethoric habit of body, and as dependent on an overplus of blood in the system. All those cases of insanity at the critical period which have come under my own observation, as well as those with whose history I have made myself acquainted, presented a marked asthenic type of disease; the patients were for the most part anæmic and emaciated. Apart from the delusion that their food could not nourish them, there was loss of appetite, frequently amounting to nausea at the appearance of food. The bowels were almost invariably constipated, the tongue dark and furred, the pulse thin and easily compressible, the countenance pale, bloodless, and haggard. With regard to the condition of the catamenia, the records of the asylum were not such as to afford any information which could be thrown into a tabulated form. It appears, however, that in the great majority of cases the catamenia were extremely irregular at the time the insanity supervened, while in others the insanity developed itself a short time (two or three months) after their total cessation.

The following table shows the relative frequency of the mental symptoms above described:—

TABLE II.—SYMPTOMS.

Melancholia,	107
Suspicions,	63
Fear of undefined evil,	68
Fear of soul's loss,	46
Excited,	84
Suicidal,	68
Refusing food,	39
Dangerous to others,	28
Homicidal,	3
Exalted delusions,	4
Demonomania,	9
Hallucinations of hearing,	14
...... of vision,	3
...... of both these senses,	6
...... of smell,	2

I may here remark of the symptoms above enumerated, that although they are met with individually in other forms of insanity, their existence as *a group* is so peculiar and characteristic of the insanity I am here describing, that any one accustomed to observe them could, I think, almost with certainty pronounce any case to be one of climacteric mania, from the symptoms alone, irrespective of a knowledge of the patient's age and history.

Melancholia.—It is seen from Table II. that melancholia is the prevailing characteristic of this class of cases, and was present in

107 of the 200. Indeed, melancholia existed in almost all the cases at some time, either previous or subsequent to their admission, although those only who manifested this symptom on admission are placed in the table. The melancholia of climacteric insanity is somewhat peculiar, and differs materially from that which is met with in insanity occurring earlier in life, as at puberty, or in puerperal mania, and it has less of the demented type than that which is seen in phthisical mania. It is distinct also from that idiopathic melancholia which is met with in persons of the true melancholic temperament at various ages. In climacteric insanity this characteristic symptom generally first shows itself in the form of transient fits of depression of spirits, accompanied by a desire for solitude, a disinclination to perform the most ordinary and trifling domestic duties, and a morbid sensitiveness and irritability quite at variance with the patient's natural disposition. It is somewhat curious to remark, that these "fits of low spirits," as the patient's friends call them, generally occur in the morning, and gradually pass off towards evening. The morbid sensitiveness and irritability shows itself in the way in which the patient magnifies the most trifling annoyances into grievous domestic afflictions, and regards slight disappointments as special visitations of Providence. They look upon all their friends who are not continually pitying and sympathizing with them, as treating them slightingly and with neglect, and yet the pity and sympathy of their friends almost invariably aggravate their disease, by ministering to a morbid craving. The patient begins to suffer much from sleeplessness, frequently passing the night lying awake, and brooding over fancied wrongs, sins, and misfortunes, and rises unrefreshed in the morning, to regard her domestic duties as a grievous burden, and to view all things through an atmosphere of gloom and despondency, which gradually becomes more and more permanent. At the same time, the patient's general health becomes affected, the appetite, at first capricious, becomes gradually more so, and is often entirely lost, and there is frequently nausea at the appearance, or even the idea of food; the patient, in consequence loses flesh, and has a worn, haggard look; the bowels are irregular, generally constipated; there is frequent headache and dizziness; the tongue is dark and furred; the breath offensive. In a great number of cases the patients have a great desire for, and are constantly taking, purgative medicine, which affords them no relief and almost invariably increases the general derangement of their health.

The above description applies to the commencement of the disease. As it progresses, the melancholia becomes either fixed and permanent, or alternates with periods of restlessness and excitement. At this stage a craving for stimulants frequently manifests itself, which, if not checked, is apt to lead to confirmed dipsomania, which latter disease is very often developed at the critical period in women. As the melancholia becomes more permanent, it is very frequently associated with the next symptom I notice, namely, suspicion.

Suspicion of Others.—It is seen by referring to Table II., that this symptom was present in 63, or nearly one-third of the 200 cases. This suspicion is generally first shown in the patient's regarding the want of extravagant expressions of sympathy in her friends not merely as indifference, but as an intentional slight and designed neglect; in fact, the first symptoms of suspicion are merely an aggravation of that morbid sensitiveness and irritability before noticed; but as this impression gains strength upon the mind, the patient becomes watchful of the words and looks of those around her, investing them with a meaning of grave import which they are far from possessing, and attributing artful and evil motives to the most innocent and harmless actions. She fancies she is regarded as an encumbrance, that her relations are tired of her and anxious to get rid of her; and, as this suspicion deepens, she imagines they mix drugs and poison with her food, watch her at night and when she is alone for an opportunity to rid themselves of her for ever. This suspicion of poison is very common, and extends not only to her relatives, but to those under whose care she may be placed, and it is a cause, though by no means the most common one, of refusal of food. Suspicion of others, while it tends to deepen the melancholic condition, very often gives rise to paroxysms of excitement, during which the patient is dangerous to, and frequently threatens the lives of those whom she suspects of having designs upon her own. And not only does it strengthen any suicidal tendency which may already exist, but it is apt of itself to induce it, by giving rise to the impression on the mind of the patient that her life is an encumbrance, and that her death would be a relief to all. Suspicion is principally directed to the patient's nearest relatives, very frequently her children, and their efforts to remove it, generally clumsy and exaggerated, tend rather to confirm it; and I have frequently observed that patients continue to suspect their relatives long after they have ceased to entertain any suspicion of their medical attendants in an asylum.

Fear of Undefined Evil.—A very common and characteristic symptom of this form of insanity closely associated with the melancholia and suspicion above described, is fear of undefined and impending evil. This symptom was present in 67 of the 200 cases, or one-third. Its presence gives rise to a very peculiar expression of vague terror in the patient's face, accompanied by a shrinking, startled manner, sleeplessness, and restlessness. This terror sometimes takes such complete hold upon the patient, that she shrieks out and calls loudly for help, although unable to tell what she is afraid of, except that "she feels that something awful is going to happen." When interrogated as to the nature of the fear, the patient generally describes it as a "something hanging over her," "an impending doom,"—a catastrophe which may happen at any moment, the fear of which is ever before her. Fear of undefined evil is frequently associated with hallucinations of the senses, more particularly that of hearing. It is also often seen in connexion with, though perfectly distinct from, the next symptom I notice, viz.:—

Fear of the Soul's Loss.—This was present in 46, or nearly one-fourth of the 200. The most common way in which this delusion is expressed is in the patient's maintaining that "she has committed so many sins," and is so "utterly unworthy," that there is no hope for her either in this world or in the next. But besides entertaining this general opinion of their irremediable unworthiness, more than one-half of the above number expressed the distinct delusion that they had committed "the unpardonable sin," and on that account were eternally lost. It is in those patients who exhibit this symptom, and who are constantly expressing the delusion of their eternal loss and consequent extreme misery, that the melancholia is most profound, and in whom the suicidal tendency is most common and most determined. This fear of the soul's loss is undoubtedly more commonly met with in this form of insanity than in any other, and marks well, I think, the distinction between the melancholia of climacteric insanity and that which is seen in phthisical mania, which, as described by Dr Clouston, consists in a general depression without any fixed delusions.[1]

Excitement.—Of the 200 cases, 84 are noted as labouring under excitement, which varied in degree,—in most cases being merely a temporary aggravation of the sleeplessness and restlessness, and in a very few amounting to acute mania. Excitement, although a very common, is by no means a very characteristic symptom of this form of insanity. It almost invariably, as before mentioned, shows itself in the form of brief and transient paroxysms, alternating with periods of deep melancholy and gloom. During these paroxysms the patient may be violent, threatening, and dangerous to others, or she may be moaning and raving incoherently about her fears, wringing her hands, and crying loudly for help. The first or dangerous form of excitement is the result of deeply-rooted suspicion that others are conspiring against her life and property, and is rarely accompanied by suicidal impulse. The last and most frequent form is caused by the intense fear of terrible evil, or the conviction of eternal ruin, and is very often accompanied by suicidal impulse; both forms are transient, lasting only a few hours, rarely more than a few days. In some cases the excitement recurs regularly at the menstrual period, and may do so for some time after the catamenia have ceased to appear; in others, the reappearance of the catamenia after these have been some time absent, is ushered in and accompanied by an attack of excitement. I have said that the excitement may amount to acute mania, and this, occurring in patients of weak and emaciated condition, not unfrequently causes death by exhaustion. Attacks of excitement sometimes occur in connexion with hallucinations of the senses.

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[1] See Journal of Mental Science, No. 45, April 1863.

Suicidal Tendency.—With the exception only of homicidal impulse, this is undoubtedly the gravest and most important symptom of any form of insanity, and it is manifested more frequently in climacteric insanity than in any other form, with the sole exception of puerperal mania. Of the 200 cases, 68 were suicidal, or one-third of the entire number. All of those 68 had meditated and threatened suicide: more than one-half of them had attempted it. Suicidal tendency is almost invariably associated with melancholia, fear of the soul's loss, and monomania of suspicion; and it is most determined and persistent in those who express the delusion of the soul's loss. But it may undoubtedly exist without any of those symptoms, and it is sometimes seen thus in connexion with and apparently dependent upon demonomania, or it may be seen with hallucinations of hearing alone. In not a few cases the manifestation of suicidal tendency was the cause of the patients being placed in an asylum, that being looked upon by their relatives as the first unmistakable proof of insanity. Suicidal tendency has two specially marked and well-known characteristics, which demand the most careful watching,—the one is the fact, that the accidental presence of the means of committing suicide, as a knife, a razor, or a rope, will at once give rise to the impulse in patients who would not take deliberate measures to procure those means, which impulse may arise instantaneously; the other is the no less fully ascertained fact, that suicidal tendency may continue to exist, carefully masked and concealed, long after the other symptoms of insanity which accompanied it seem to have disappeared. By this I mean that the delusions of the soul's loss, suspicion of relatives, etc., still remain, and nourish, as it were, the suicidal tendency, but that the patient ceases to express them, because she perceives that so long as she continues to do so, she is carefully watched and prevented from effecting her purpose. These delusions, although the patient no longer talks of them, are very frequently expressed in her *letters*. Concealed suicidal tendency, with apparent abandonment of the delusions with which it is associated, sometimes has the effect of inducing the patient's friends to remove her from the asylum, under the idea that she is recovered, and contrary to the remonstrances of the physician; and not unfrequently the consequence is that she takes an early opportunity of carrying out her concealed but fixed purpose, to their very great surprise and distress.

It would be quite out of place here to describe the various modes by which suicidal patients have attempted to destroy themselves, although some of them are curious enough, as showing how persistent and determined the impulse may be. Starvation, hanging, drowning, cut-throat, precipitation, are the most common; but the patient will sometimes employ the most *outré* methods of putting an end to herself, as swallowing her garters, or pieces of blankets, or she will attempt to strangle herself with her hands, dash her head against the walls, or plunge it in water, and keep it submerged with a determined persistency hardly conceivable. In one case transferred to Morningside from the Crichton Institution, the patient, as described by Dr Browne, had shown peculiar ingenuity in attempting to effect her purpose. Having accidentally discovered that she was receiving calomel, and believing that it was dangerous under such circumstances to be exposed to cold, she managed to fill her shoes with water, and sit with them on, in the hope of inducing fatal illness. Very frequently suicidal patients persistently refuse all food; but as this is also often the case with those who cannot exactly be called suicidal, inasmuch as they would not take direct means to destroy themselves, and as it is often seen in connexion with delusions and suspicions in those who are not suicidal at all, I notice it separately.

Refusal of Food was met with in 39, or nearly one-fifth of the 200. In those who refuse food as an indirect means of committing suicide, the patient suddenly expresses her determination not to eat anything more, and it is rarely that any amount of persuasion can induce her to alter that determination. Hunger in these cases seems not to be felt at all, or if felt, its cravings are disregarded or subdued with incredible perseverance, as food may be left for days untouched beside the patient. But refusal of food may arise from suspicion; in which case the patient at first shows much caprice in her appetite, taking a violent dislike to certain articles of food, saying they produce extraordinary effects, as burning pains, giddiness, faintness, and the like. She will then say her food and drink are tampered with, and finally refuse it entirely, declaring it is poisoned. Sometimes delusions are the sole cause of refusal of food, the patient saying her "inside is gone, and that food cannot nourish her;" or that food "makes her swell up enormously;" or that "noxious vapours arise from her food and ascend to her brain." In almost all cases refusal of food is only to be overcome by feeding with the stomach-pump; but even when this is used, the patient, if determined and persistent, will contract the habit of vomiting everything that is given her unless carefully watched. Refusal of food is undoubtedly one of the most troublesome and perplexing of the symptoms of this disease; for, although it frequently happens that a patient only requires to be fed once or twice with the stomach-pump to induce her to eat voluntarily, it is sometimes the case that a patient will rather submit to all the disagreeable annoyance of being so fed, than forego her resolution and take food of her own accord. And when this obstinate refusal of food is long continued, the patient gradually becomes weaker and more emaciated, and must eventually die of inanition, notwithstanding that the most concentrated forms of nourishment, together with stimulants, may be given her by the stomach-pump; as it is well known that food taken involuntarily, although it may support life for a time, is not properly assimilated, and fails to afford sufficient nourishment to maintain the vital powers for any great length of time. This remark does not apply to those rare cases of catalepsy, where patients may be fed for many months, and the body continue to be well nourished, for in these last there is no direct effort of the will exerted against the reception of food.

Dangerous to Others.—Of the 200 cases, 28 were either stated in the description of their symptoms previous to admission to be dangerous to others, or were subsequently found to be so. Those patients who threaten or attack others generally do so during the transient paroxysms of excitement produced by monomania of suspicion, and as the suspicion is, as we have seen, principally directed against the patient's relatives, such cases rarely prove dangerous in an asylum. But there are cases in which patients are dangerous, not as the result of suspicion, but in consequence of their being subject to hallucinations of hearing, or demonomania. When violence is the result of these last, it is a far more serious symptom than in the former case, inasmuch as being dependent on hallucinations or delusions at all times present, the impulse to commit violence may arise at any moment, without any general excitement to act as a warning; and a patient not described or supposed to be at all dangerous, may thus violently attack those near her, from a fancied supernatural instigation or command. Of the 28 who were dangerous to others, 3 only manifested *homicidal impulse.* In two of these the patients signified their deliberate intention to destroy their children. Both of these patients became demented. The other was the case of a woman who had frequently threatened and once attempted to kill her sister. This patient died in the asylum from exhaustion, after prolonged maniacal excitement.

Exalted Delusions.—I have before stated that delusions of an exalted kind are very rare in this form of insanity. Of the 200 cases 4 only had any such delusions; in these 4 instances the patients expressed the delusion that they were possessed of vast wealth, power, or occupied a very distinguished position in society; the delusions in these cases resembled those of general paralysis with this exception, that in none of the above cases did the patients manifest that propensity to hoard up rubbish, as stones, pieces of glass, iron, etc., regarding them as articles of immense value, as is so commonly seen in general paralytics.

Demonomania.—In 9 of the 200 cases the patients were demonomaniacs, or believed that they were possessed by an evil spirit or spirits, and were thus instigated to commit various acts against their will. Such patients I have said are frequently dangerous, but more frequently still the idea that they are possessed seems to add intensity to that vague terror before described, and is often the cause of a most determined suicidal tendency. I may here remark that nearly all the cases of demonomania recorded by Esquirol were those of women at the change of life.[1]

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[1] De Maladies Mentales, tome i. p. 490.

Hallucinations of Hearing.—It has been stated that persistent hallucinations of hearing generally indicate deep seated disease of the brain, that the insanity associated with such hallucinations is incurable or nearly so, and they are consequently apt to be regarded as a very grave and serious symptom. There can be no doubt that hallucinations often exist with hopeless and incurable forms of insanity; but that they do not of themselves give any evidence of serious brain disease is apparent, I think, from the fact that of the 14 mentioned in Table II., who had persistent hallucinations of hearing, 7 recovered, and only one died, the recoveries being in the ratio of 50 per cent., which is very nearly equal to the percentage of recoveries in those who had no such hallucinations. In cases of hallucinations of hearing, the patient generally declares that she hears a voice or voices speaking to her at certain times, most frequently at night: sometimes she will stand for an hour or more at the door of her room, with her ear at the keyhole, answering imaginary questions. In other cases the patient, while working or at meals, will suddenly pause in an attude of attention, and listen to fancied remarks or injunctions.

Hallucinations of both Senses (*Hearing and Vision*) were present in 6 cases, of whom 4 recovered, 1 died, and 1 was removed unimproved.

Hallucinations of Vision alone existed in 3 cases, of whom 2 recovered and 1 died. And hallucinations of the sense of *smell alone* were present in 2 cases, of whom 1 recovered and 1 died. Two of the patients described their hallucinations of vision as a vapour which rose up suddenly before them, taking no very definite form, but appearing all at once on their opening a door, a box, or a drawer, remaining stationary before them for a few seconds, and suddenly vanishing. In the other cases, the patients declared that the deity in bodily shape appeared to them, as also the apostles. In the two cases in which there were hallucinations of the sense of smell, one of the patients said that her food, clothes, and the air of certain rooms smelt strongly of brimstone; the other declared that every one who came near her had an overpoweringly disagreeable smell, and she constantly held her nose when approached or spoken to.

Results.—I now come to consider the results of the 200 cases, which are as follows:—

TABLE III.

Of 200, . .	104 recovered.
" . .	44 became demented.
" . .	24 removed improved.
" . .	22 died.
" . .	3 removed unimproved.
" . .	3 convalescent.
	200

From this it is seen that the recoveries took place in the ratio of 52 per cent.; or 53·5 per cent. if we include the three convalescent cases. This, I may remark, is higher than the percentage of recoveries from melancholia in general, which is stated to be only 27 per cent. by Haslam, although Dr Tuke found it as high as 54·88 at the Retreat. But it is the most aggravated cases only which reach an asylum; many are treated successfully at home; and I have no doubt that if such cases could be included here, the average would be found to be nearly as high as it is in puerperal mania, the most curable of all forms of insanity.

The following table shows the duration of the disease in those who recovered:—

TABLE IV.

	Number of Cases.
Under 6 weeks,	9
... 2 months,	10
... 4 ...	28
... 6 ...	20
... 8 ...	15
... 10 ...	8
... 12 ...	2
... 18 ...	8
... 2 years,	4
Total,	104

Duration.—It is seen from this that the duration of the disease was only four months in the largest number of recoveries, which is a most important point in favour of the curability of this form of insanity, it being undoubtedly true, as pointed out by Brierre de Boismont, that the more quickly any form of insanity can be cured, the less danger is there of any impairment of the mental faculties remaining. 44 of the 200 cases, after their disease had continued in a chronic form for a year or more, during which there were occasional attacks of excitement more or less maniacal, gradually passed into a state of hopeless dementia. 24 of the 200 were removed by their friends improved. Of these, as some of them were brought back, it is reasonable to conclude that at least the majority ultimately recovered, which would raise the percentage of recoveries very considerably.

Deaths.—Of the 200 cases 22 died in the asylum during the period of eighteen years, over which this investigation extends, thus giving a mortality of 1·22 per annum, or ·61 per cent. per annum. This rate of mortality is so very low as to demonstrate, that, apart from suicidal tendency, organic disease, or other morbid complications, this form of insanity has but little tendency to a fatal termination. The causes of death were as follows:—

TABLE V.

Exhaustion from prolonged excitement, or obstinate refusal of food,	5
Phthisis pulmonalis,	5
Cardiac disease,	3
Diarrhœa,	3
Dysentery,	2
Pleurisy,	2
Bursting of bronchial abscess,	1
Cancer of uterus,	1
Total,	22

In those 5 cases only in which death took place in consequence of exhaustion from prolonged excitement or obstinate persistence in refusal of food, can insanity be regarded as the direct cause of death.

The 5 who died from phthisis all manifested the symptoms of that disease on admission, and there was every reason to believe that the phthisis had existed before the insanity, which, in its general character, symptoms, and duration, differed markedly from phthisical mania, properly so called.

It may be said generally of the other diseases which were the immediate cause of death, that they all presented the asthenic type which is characteristic of this form of insanity.

Treatment.—The means of treatment which are indicated by the symptoms and general character of this form of insanity, and which appears to me most likely to be successful, are as follow:—

1*st*, The immediate removal of the patient from home, and from the scenes and associations in connexion with which the disease was first developed, and the separation from the society of relations and friends who, in their mistaken kindness, minister to a morbid craving for sympathy, or endeavour to combat delusions and remove suspicions in a manner which, however well-intentioned, rarely, if ever, does any good, and very frequently tends to aggravate the disease. I do not wish it to be inferred from this that the patient should forthwith be placed in an asylum. On the contrary, as I have before stated, many cases may be, and undoubtedly are, treated out of asylums successfully; and there can be no doubt whatever that in this form of insanity, more perhaps than in any other, there are a large number of cases where there is no suicidal tendency, refusal of food, or monomania of suspicion, and where the excitement is merely an aggravated form of restlessness that, if placed early under proper treatment would recover without the necessity for seclusion in an asylum at all; but I repeat, that in all cases, change of scene and removal from the influence of misdirected sympathy is absolutely necessary.

2*d*, One of the most important means of treatment consists in placing the patient under a regulated system of moral discipline and control, by providing occupation, amusement, and exercise at regular hours.

3d, Good nourishing diet, together, in many cases, with a regulated amount of stimulants, is of the utmost importance as a means of treatment, the more so as in the majority of cases there is great caprice in the appetite, and in many a desire to abstain from food entirely. In regard to the use of stimulants in such cases, I think they can only be prescribed with propriety when the patient's regimen is entirely within the control of the physician and her attendants, as the temporary relief which they produce is very apt to lead to undue indulgence on the part of the patient if left to her own discretion. As before stated, the obstinate and persistent refusal of food is only to be met with by using the stomach-pump, and in most cases it only requires to be used two or three times to convince the patient of the necessity of submitting to control.

4th, We have seen that the general health is commonly much deranged, the bowels frequently obstinately constipated, and the patient very often in the habit of taking large doses of purgative medicine, whereby the natural action of the bowels is destroyed. This requires tonics, gentle laxatives, and exercise.

5th, Narcotics form one of the most efficient means of alleviating and curing this form of insanity. It is somewhat difficult to lay down general rules for their administration. In some cases they obviously aggravate the restlessness and excitement, in others they act apparently like a perfect charm in procuring sleep, and allaying nervous irritation and despondency without interfering with the general health. The cases most suitable for this method of treatment appear to me to be those chiefly which are characterized by a general feebleness of the circulation, and a well-marked anæmic state of the system. In some such cases I have seen large doses, such as two drachms of the solution of the muriate of morphia, given nightly for a lengthened period, attended with the most beneficial effects and without any disturbance of the general health. The exhibition of such powerful doses must always be continued under the strict supervision of the medical attendant, and they must be carefully diminished and gradually withdrawn by him before the patient is left to her own control. In many cases where morphia seemed to disagree with the patient, other narcotics were used with advantage, such, for example, as the tincture of hyoscyamus and tincture of cannabis indica. These medicines require to be used in larger doses than in ordinary practice to produce the desired results, but the requisite dose can only be arrived at by a gradual increase from the ordinary medicinal dose up to that by means of which the desired object of natural repose is attained.

6th, The other indications of treatment consist simply in the removal of all local causes of irritation by the cure of local disease, and the re-establishment of the general health in accordance with the recognised code of therapeutics.

The principal conclusions to be drawn from the foregoing description of this disease are:—

1st, That the symptoms of insanity occurring at the climacteric period in women are so uniform, characteristic, and peculiar as to render it easily recognisable, and entitle it to be referred to a distinct natural group or family, which may be distinguished as climacteric mania or insanity.

2d, That this is one of the most curable forms of insanity accompanied by melancholia.

3d, That the duration of the insanity in curable cases rarely exceeds from three to six months.

4th, That this form of insanity, apart from suicide and organic disease, rarely tends to a fatal termination.

5th, That the most important indications of treatment are, the early removal from associations and friends; careful watching; nutritious diet; and the judicious administration of narcotics.

In conclusion, it has been pointed out by Sir Henry Halford and also by Dr Conolly, that a climacteric period occurs in the male between the ages of 50 and 60, during which insanity is frequently developed. I will only here add that I have seen many such cases, and I am satisfied that it presents in the male the same characteristic group of symptoms as it does in the female; but I shall reserve the consideration of this subject for some future opportunity.

ARTICLE V.—*A Case of Traumatic Tetanus treated by Opium Smoking and Internal Administration of Chloroform and Hemp.* By J. FAYRER, M.D., Professor of Surgery, Surgeon Medical College Hospital, Calcutta.

LUKHAM DASS, a Hindoo labourer, aged 24 years, of small but muscular frame, received an incised wound from the sharp edge of a split bamboo, on the outer aspect of the end of the right forefinger. The wound being slight, and the hæmorrhage trivial, he took no notice of it. Twelve days after the infliction of the wound, tetanic symptoms made their appearance, a feeling of constriction in the throat, rigidity of the jaws and muscles of the neck. The wound was dressed by a native, with some irritating application, which added to the mischief. Six days after the first appearance of trismus, he was admitted into Dr Fayrer's ward on the 29th August at 7 A.M. Tetanic spasms and the risus sardonicus were then well marked. The mouth could with difficulty be slightly opened, and he was unable to protrude his tongue. The muscles of the extremities were in a state of permanent spasmodic contraction, as were those of the abdomen and back, the trunk being in a state of opisthotonos. The skin was of the natural temperature, and covered with sweat. The pulse quick and compressible. The spasms increased with great violence, lasting for a few seconds, and returning at intervals of five or six minutes. The fits were more violent when the wound on the finger was touched, but a touch anywhere, a breath of air, or the fanning of the punkah, brought on violent spasms. The bowels had not been moved for four days. Urine voided freely.

29th August, 8½ A.M.—The two distal phalanges of the right forefinger were amputated. The wound had extended, exposed, and caused necrosis of the bones. The nerve trunks of the part removed were examined under the microscope and found healthy in appearance. After the amputation, which was done under chloroform, the patient became restless, and the spasms recurred with greater violence and frequency than before. Notwithstanding all this he took, or attempted to take, some sago and milk. An enema of castor oil, turpentine, and assafœtida was administered, and ordered to be repeated every sixth hour. The enema not acting satisfactorily, two drops of croton oil were given; after this the bowels were freely relieved before the evening. He had also been brought under the influence of opium smoke: four grains of that drug having been smoked in a chillum (pipe) every third or fourth hour. With the opium he had been ordered chloroform, min. x., tinct. cannabis indicæ, min. xx., every second hour, with continual applications of ice in the intestine of an animal, to the entire length of the spine.

30th August, 8 A.M.—He slept at intervals during the night; pulse quick and compressible; pupils not contracted; he seems quieted, but not narcotized by the opium and hemp; bowels have been moved again. The state of the mouth, neck, and rest of the body much as yesterday; spasmodic fits recur every five or six minutes; body bathed in sweat. The treatment to be continued as yesterday, but opium to be smoked every second hour so as to bring him fully under its influence.

31st August.—The patient feels easier this morning; abdomen comparatively soft; bowels moved last night; pulse not improved, quick, and weak. He can protrude the tongue for about a quarter of an inch. He is made to take beef-tea and port-wine, with sago, and milk, and eggs beaten into custard, with wine as frequently as possible, and these are supplemented with beef-tea. Enemata given after the cathartic enema have cleared the bowels out. The muscles of the extremities are rigid, and the whole body is covered with sweat. The chloroform has been increased to min. xviii. each dose, and gr. ii. of extract of hemp instead of tincture; the enemata and opium smoking to be continued.

1st September, 8 A.M.—Is quite under the influence of opium; pulse quick and compressible; on touching the ligature on the artery in the wound, the patient is thrown into violent spasmodic fits, which last for a few seconds only; the pupils are contracted, and the body covered with sweat.

5 P.M.—Bowels opened thoroughly; body in a state of opisthotonos. The medicines are to be continued, and instead of solid

opium, he is now to smoke a bazaar preparation of opium made for opium smokers.[1]

2d September, 8 A.M.—The patient had some sleep during the night. The extreme rigidity of the abdominal muscles is diminished, and the fits recur at longer intervals, of half-an-hour or more. The pupils are contracted; bowels moved twice in the night. The muscles of the extremities are as rigid as ever; but the trismus is disappearing gradually. He can speak tolerably. He smoked about 34 grains of opium in the form alluded to in the note, during the last twelve hours. The hemp and chloroform as usual. The wound was dressed with the opium lotion.

3d September, 8 A.M.—Body still in a state of opisthotonos; pupils contracted; body bathed with sweat. Mouth can be opened to the extent of one-fourth of an inch; but the general condition is one of spasm. The remedies, opium smoking, enemata, and ice to the spine, are continued as before.

4th September.—Had disturbed sleep last night; pupils natural; pulse slow and compressible. Bowels moved twice this morning; the rigidity of the muscles is diminished; the wound of the amputated finger is cicatrizing; the ligatures have come away. The remedies are all continued as usual.

5th and 7th September.—He has improved; the spasms have been less frequent, and less severe; the abdominal muscles much less tense, and he is able to protrude his tongue to the extent of half-an-inch. The quantity of opium smoked is about 70 grains daily.

8th September.—The chloroform and hemp have been slightly increased, as they are beginning apparently to lose their effect. Opium smoked during this day about 50 grains; towards the evening he had a pain in the chest, with cough.

9th September, 8 A.M.—The pupils much contracted; he is fully under the influence of opium; he is better as to the tetanic condition. The chloroform and hemp reduced to min. xviii. and gr. 1½ every four hours.

10th September.—Bowels moved freely three times during the night; spasms diminishing; chloroform and hemp discontinued for a time, and tinctura ferri muriatis, min. x., ordered to be given every four hours. It was found desirable to give the hemp and chloroform again in the evening. The ice applied to the spine; enemata and opium smoking as before.

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[1] The natives of Bengal generally smoke opium in this form, which they call "goolie." It is prepared in the following manner:—The guava leaf is cut into very small pieces, thoroughly dried in the sun or at the fire; an infusion of opium strained, and evaporated over the fire to the consistence of syrup; with this the dried guava leaf is mixed and reduced into a kind of paste. The mass is then divided into small balls or goolies, and each goolie weighs from eight to ten grains, and contains equal parts of opium and guava leaf. When smoked it is placed on a piece of burning charcoal in the ordinary hubble-bubble or hookah.

11th September.—Still pain in the chest; bowels moved three times during the night; medicines continued.

12th September.—The chest is still painful; ordered turpentine stupes. In the evening, as the pain increased, he was ordered opium gr. i., ipecac. gr. ii., every four hours.

13th September.—Pulse 108, very soft and compressible; body bathed with sweat. The opium and ipecacuanha discontinued; returned to the chloroform and hemp, and rub the chest, abdomen, and limbs with a liniment of camphor, opium, and chloroform. He smoked about 35 grains of opium since last report.

14th September.—Had sound sleep last night. He is gradually improving; the body is still in a state of chronic spasm, but it is much less rigid than formerly, and he is able to flex his limbs, open his mouth, speak, and take food easily. The least irritation brings on spasms still. The pains in the chest, no doubt the result of the spasm, are somewhat better. He takes his food fairly; upon an average he disposes of about 80 grains of the opium preparation daily.

19th September.—He continues to improve; is able to raise himself into a sitting posture, but still the slightest irritation brings back the spasms, though with less violence. The muscular pains are much reduced by friction with anodyne liniment. He was placed on his feet and supported; attempted to walk, but the effort brought on spasm, and the rigidity remained more or less throughout the day.

20th September.—A gentle current of electric magnetism tried, but it brought on the spasm more violently and was immediately discontinued. The improvement is now steadily progressing and he sits up in bed, though with some difficulty. The medicines, ice to the spine, and opium smoking, are still continued; he has became fond of the opium, and says he can't do without it.

30th September.—Doing well. The ice to the spine was discontinued, as it caused a burning pain. The other remedies and diet, which is of a nutritious character, are continued.

After this the improvement was gradual but sure; but for some time he complained of pain and stiffness in the muscles generally. He gradually regained the power of walking without aid. The medicines were diminished by degrees, and the quantity of opium reduced to half.

We found on examining him, after he was able to stand erect, and walk, that the spinal column has been curved laterally to a considerable extent at the second, third, and fourth dorsal vertebræ. He had been perfectly straight, and well formed; but now the shoulder is prominent, and his height considerably lessened. The result of the continued muscular spasm has been to distort the spine, and though he has recovered, it is with this deformity.

He left the hospital on the 11th October, and walked home. He was much emaciated and aged in appearance, as well as altered by the lateral curvature.

The quantity of opium goolie consumed from the 1st to the 30th September was nearly ʒ xlii., which gives an average of 84 grains per diem; of course much of this was lost in smoking, but still the quantity actually inhaled was large.

Remarks.—The above notes of the progress and treatment of a case of traumatic tetanus are very interesting, as they appear to indicate that opium smoking, if freely and persistently continued, has a controlling power over this mysterious disorder, enabling the patient to live through the terrible exhaustion that it produces.[1]

The results of the treatment of traumatic tetanus, when well developed, have hitherto, in my experience, been most unfortunate, and I cannot but regard this case as a very satisfactory one; as it enables me to feel that I have a more potent remedy at my command than any I have hitherto employed. I believe I have left none of the recognised methods of treatment untried; and in traumatic tetanus, I may say they have all, with the exception of division of the nerve trunk of the wounded part, disappointed me. Some time ago, I treated a case of severe traumatic tetanus, caused by a wound of the ball of the thumb, by section of the median nerve, about the annular ligament, and the result was most satisfactory; but in that case the tetanic symptoms had only recently set in, and no permanently ill effect had been set up in the spinal cord. I believe that though this treatment would frequently be of service if practised early, it is useless if resorted to after the disease has been thoroughly developed for even two or three days. In the case here detailed, however, it had been of much longer duration when the patient came under treatment, and yet the result was satisfactory.

It may be said that the large and repeated doses of hemp and chloroform, with the ice to the spine, may have been the causes of the curvature; no doubt they were to a certain extent, but I have treated many less severe cases with them, with little other effect than that of promoting euthanasia.

I am indebted to a very intelligent dresser, Baboo Gunga Pushad Mookeyea, B.A., for the notes of the case.

ARTICLE VI.—*Death from Rupture of Varicose Veins in the Lower Extremities.* With Illustrative Cases. By Dr ALEXANDER R. SIMPSON, Secretary to the Edinburgh Obstetrical Society.

(*Read before the Medico-Chirurgical Society*, 18*th January* 1865.)

DELPECH,[2] in treating of varices, describes how, under certain conditions, rupture of the distended vessel may take place and give rise to hæmorrhage. "In such a case," he adds, "the effusion of blood has sometimes been very considerable; it is an unheard of thing, however, that it should prove dangerous (cependant il est inouï qu'elle ait été dangereuse): the resulting syncope, or the slightest compression, has sufficed to check it."

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[1] I am indebted to Dr Gordon, C.B., for the suggestion of treating this case with opium smoking.

[2] Précis élémentaire des Maladies réputées Chirurgicales, section 8, chap. i. art. iii.

It is true that when a varicose vein happens to burst, it requires but slight compression to arrest the hæmorrhage; and in some instances the bleeding has ceased spontaneously, where the patient has fainted, but has afterwards recovered. Two cases illustrative of the former of these have come under my own observation, and may be briefly stated:—

NON-FATAL CASE 1.—Mrs W., when near the full term of pregnacy, ruptured a varicose vein. She applied her finger to the bleeding point, and thus restrained the hæmorrhage until, in the absence of her medical attendant Dr Wilson, I went and secured it more effectually by means of a compress and bandage.

NON-FATAL CASE 2.—E. L., æt. 50, unmarried, has had varix of left leg for five years. On 5th April 1862, on pulling off her stocking, she was alarmed by a sudden rush of blood from beside the inner ankle, which greatly astonished her, as she had never paid special heed to the enlargement of the vessels along the inner and anterior surface of the foot and leg. She tried at first to stem the flow by wrapping some cloths round the limb, but seeing the blood soaking through them, and having some ordinary sticking-plaster in the house, she cut a bit the size of a half-crown, warmed it at the gas, and applied it over the bleeding orifice, keeping it compressed with her hand till it had become fully adherent. She remained at rest for some days, and when she came to see me about a week afterwards there was only a slight ulcer left, which healed kindly under the use of some simple applications. I have seen her lately on account of the recurrence of ulceration on the site of the old scar, but there has never been any repetition of the hæmorrhage.

For notes of a third case, which is a good example of the rarer mode of arrest by syncope and subsequent recovery of the patient, I am indebted to Dr Alexander Wood.

NON-FATAL CASE 3.—"A man was found at the old stepping-stones at Malta Green, Edinburgh, lying apparently dead among a quantity of human excrement, with his trousers hanging about his legs and saturated with blood. From this he was conveyed to the Stockbridge station-house, where I saw him. He had fainted from hæmorrhage arising from a ruptured vein. I made a compress of a copper coin and lint, and bound up the leg. Stimulants were administered, and he recovered. He then told us that he had been obeying a call of nature at the place where he was found, and that while straining to expel a costive stool a mist came over his eyes, and he remembered no more until he found himself recovering in the station-house."

While cases of this kind, however, are thus not unfrequently occurring, where the bleeding is checked and the patient revives,[1] it is strange that a man of such extensive experience as a surgeon, and such wide renown as a teacher, as Delpech, should never have heard of a case where this accident was attended with more disastrous consequences. For not a few cases have been put on record,[2] both before his day and since, where rupture of a varicose vein has been followed by an immediately fatal issue. But the very fact of such a statement, coming from such a source, emboldens me to relate a case which I had recently occasion to witness, and to offer some general observations on the subject.

FATAL CASE I.—On the evening of Tuesday, 30th August, I was hurriedly summoned to see a woman in the "Servants' Home," who had "broken a bloodvessel." The messenger had heard some one say that she had a sore leg; but he had not the least idea what organ was the seat of the hæmorrhage. Putting a piece of lint and a bandage in my pocket, I went with him, and found the poor woman lying, somewhat on the left side, on a steep staircase, and with the left leg bent up under her, while the right was hanging downwards. There was a group of women on the landing below, busily wiping into pails the blood that was streaming down the steps; but none of them had any information of moment to give, and the patient was speechless, pulseless, and all but lifeless. When the dependent limb was raised, some blood ran out of the stocking. This was pulled down, and a large varicose ulcer exposed, with the patulous orifice of the ruptured vessel from which a little blood still seemed to flow. A compress was hastily applied; but when the attempt was made to administer a stimulant and move the patient to the horizontal position, she expired.

She had only been a few days in the Institution, and being a woman of a very reserved disposition, had not made any communication to her neighbours as to her ailments. She had told one of them, indeed, that she had been to a dispensary to get something for a sore leg; but when they found her bleeding to death, the only conjecture that was offered among them was that she might be flooding. She had been doing a day's work outside, and the diseased vessel must have given way before she reached her home at nine o'clock, for some blood was afterwards found on the doorstep in the lobby. At that hour the doors of the bedrooms, which occupy the fourth or highest story of the house, are all locked while the inmates are assembled for worship in a large room on the second story. The poor woman must have passed the door of this room on her way to her bedroom, two floors higher up. Finding the bedroom closed, she had descended a flight of stairs, and sat down for a while in "the parlour," where a great pool of blood was afterwards found soaking the carpet and staining the floor. Feeling herself growing faint, she must have been making her way to where she might get some aid, when she fell on the stairs with just sufficient noise to make the girl who sat next the door look out to see who could be moving about there. A messenger set off at once to fetch my friend Dr Moir, who is the medical adviser for the Institution; and as he was at the time from home, I was asked to go instead. It might be about 10.35 when I reached the "Home," and found the patient, as I have already stated, moribund. The hæmorrhage had begun, as precisely as could be determined, about forty minutes or a little more before life became extinct.

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[1] Puchelt, Das Venensystem in seinen krankhaften Verhältnissen, i. thl., p. 100, relates a case, and refers to others. A case is recorded by Hagendorn, in his Observationes et Historiæ Medico-practicæ, cent. iii. obs. 65: and the accident is, of course, noticed in almost all the systematic works on surgery, without usually being illustrated with special cases.

[2] Crisp, in his "Diseases of the Bloodvessels," p. 333, mentions two cases; and individual instances are related by Storch, in the Commercium literarium Norimbergæ for 1734, p. 116; Gendrin, in the Journal Gen. de Médecine for Jan. 1828, p. 61; Hiller, in the Medicinische Zeit. v. d. Ver. f. Heilk. in Pr. for 1842, No. 48; Carswell, in his Illustrations of the Elementary Forms of Disease, fasc. on Hæmorrhage, etc.; besides those presenting features of special interest, referred to in the course of this communication.

This is not by any means the first case of the kind that has happened in this city, although I have not met with a record of any of them in our Journals. For I have heard of the case of a woman in whom a varicose vein had burst, who fell down in crossing Leith Street, and speedily expired; and, through the kindness of Dr Alexander Wood, I am enabled to cite another instance which occurred many years ago, and regarding which he has furnished us with the following notes from memory:—

FATAL CASE II.—"I was sent for hurriedly one morning to see a washerwoman in Jamaica Street, who was reported to have ruptured a bloodvessel. On reaching the house a few minutes after, I found her lying on the floor in a state of syncope; under her was a pool of blood, which the women who were standing about believed to have issued from the vagina. Having satisfied myself that this was not the case, I next examined the legs, and found that the hæmorrhage had issued from an enlarged vein in the right leg. It was suspended by the syncope for the time, a bandage and compress were applied, and stimulants freely given, but the poor woman never rallied, and died in less than a quarter of an hour from my first seeing her."

The danger attendant on the rupture of a varicose vein becomes easily appreciable by a little study of the condition of matters such as we observe in the preparation before us. It was taken from the woman whose history I have detailed, and consists of the skin from the inner and anterior surface of the right leg, with the subjacent veins from around which the cellular tissue has been dissected away. It shows on the cutaneous surface two large irregular ulcers, and in the bottom of the upper one it is easy to see the opening into the ruptured vein, with ragged edges, and of sufficient size easily to admit of a crowquill. On the deeper surface it shows the internal saphenous vein, from which the hæmorrhage occurred, in a very varicose condition, and communicating freely with a number of collateral branches, all more or less similarly affected. Besides presenting the tortuous and dilated appearance peculiar to the affection, they are all seen to have undergone the change which has been pointed out by various writers,[1] as occurring in some forms of varix, where the walls of the vessels have become thickened and indurated, so as to feel like a hard cord, and resulting in the production of such a degree of rigidity in the tube as to prevent its spontaneous collapse and occlusion. This condition, as is well-known, is one of the circumstances that render injuries of such morbidly affected vessels unusually dangerous.

But the preparation shows, further, another pathological change which has received less notice, but is of equally great moment in thwarting the usual effort of nature to arrest the hæmorrhage. For whilst all the tissues in the leg have become to some degree congested and consolidated as a result of the impaired circulation, it will be seen that in the neighbourhood of the ulcers the skin has become specially firm and brawny, and the underlying tissues have become the seat of an inflammatory action which has led to the encasing of the affected vein in a dense fibrous tissue, to which its walls are intimately adherent, and which is so unyielding in its texture as to keep the canal almost permanently patent. The result of all this change is, that when from any injury the wall of the vein gives way at some point, the orifice is as certainly kept open as if a metallic tube were passed into the interior of the vessel; so that, however small the aperture may be,—and Abraham Colles[2] has specially pointed out how minute in such cases it sometimes is,[3] —the bleeding inevitably goes on until the heart's action fails, or until pressure be artificially applied.

I do not know why Sir Charles Bell[4] and others have been led into the error of stating that varix was more frequent in women than in men, seeing that Briquet[5] has given statistics to show that of 258 men, 71 were found to be the subject of the disease; while in 423 women, there were only 42 varicose; and Rokitansky[6] has observed it to occur with equal frequency in the two sexes. It may be owing to the circumstance, however, that the larger proportion of fatal cases of ruptured veins have occurred in women. The great preponderance of mortality in the female sex from this cause is very striking,—for out of the sixteen fatal cases in which I have been able to discover the sex of the patient, thirteen were women and only three men. The explanation of this remarkable difference is to be found, I take it, in the fact, that in a large proportion of the women the advance of the disease and the occurrence of all its attendant dangers were promoted by the changes consequent on conception.[1] For of the only eight of these women regarding whom we have any hint as to the condition of the uterus, we find that six were in a more or less advanced stage of pregnancy.

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[1] Briquet, Thése sur la Phlebectasie, Paris, 1825; or in Archives Générales de Médecine, tom. vii. p. 205, 1825. Hasse, Diseases of the Organs of Circulation and Respiration, Sydenham Society, p. 40.

[2] Dublin Quarterly Journal of Medicine, vol. xviii. p. 29.

[3] Petit, in his Œuvres posthumes de Chirurgie, tom. ii. p. 50, relates a case (non-fatal) where the opening into the vein was so small, that it was with difficulty it could be discovered.

[4] Sir Charles Bell, a System of Operative Surgery, vol. i. p. 91.

[5] Briquet, Arch. Générales de Médecine, p. 410.

[6] Rokitansky's Pathological Anatomy, Sydenham Society, vol. iv. p. 307.

Men, however, as we have seen, are not altogether free from danger from the bursting of distended veins. For the case related by Amussat[2] to the Parisian Academy of Medicine in 1827, which gave rise to a discussion there, was that of a man; and both Vidal[3] and Velpeau[4] state, though I know not on what authority, that the death of one illustrious member of the male sex has been attributed to this cause, that, viz., of the great astronomer Copernicus.[5]

One might suppose that the diagnosis would always be so easy that there could be no mistake as to the nature of the case. But Briquet[6] and Puchelt[7] have observed that the blood in dilated veins is sometimes strikingly red and arterial in colour, and when the vessel is cut or torn across the blood may spring from it with some force, and occasionally with intermissions. There is one case on record where this mistake was actually committed. I quote it from the graphic page of John Bell:[8]—

"I once saw a woman walk on foot from the north of Scotland, 300 miles, to have some operation performed. A vein which had been opened, bled so violently that even the tourniquet could not command it, and she left home under the impression that the artery was wounded; her own surgeon said so, and, from the violence of the bleeding, every surgeon who had seen her on her way up to town had acknowledged that it could be nothing but the artery. At the end of every day's journey she had lost blood, so that you might almost have traced her; she was several times obliged to stop a day or two from mere weakness. It was, after all, nothing but a vein that had ulcerated, and would not heal; but it was easily obliterated by a firm pellet of paper, kept very firm for a few days."

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[1] Murat, Revue Médicale, tom. iii. p. 348, 1827; Garmann, Ephem. Nat. Cur., Dec. i. Ann. iii. Obs. 204; Storch, Krankheiten der Weiber, i. Thl. p. 225.

[2] Archives Générales de Médecine, t. xiv. p. 608.

[3] Vidal, Traité de Pathologie externe, t. i. p. 416.

[4] Velpeau, Operative Surgery, by Townsend, vol. ii. p. 387.

[5] Gassendi, "le plus fidéle de ses biographes," simply tells that not long before the first printed copy of his great work, de Revolutionibus Orbium Cœlestium, was brought to him, he began to suffer from loss of blood, followed by sudden paralysis of the right side; and his enthusiastic fellow-countryman and admirer, Jean Czynski, gives no more definite information regarding the seat and source of this hæmorrhage in his work on "Kopernik et ses Travaux."

[6] Briquet, Archives Générales de Médecine, tom. vii., 1825, p. 400.

[7] Puchelt, Das Venensystem in seinen krankhaften Verhältnissen, ii. thl. p. 430.

[8] John Bell, Principles of Surgery, vol. i. p. 307.

It is worthy of note how slow the patient is in many instances to take alarm. It is true that they are sometimes in no position to call for aid. Thus, one of the cases related by Lombard[1] is that of a cook who was found dead in his bed from rupture of a varicose vein which he had refused to have operated on.

FATAL CASE III.—My friend, Dr. Arthur Mitchell, has told me of a case which was related to him by people well acquainted with the circumstance, of a woman in Aberdeenshire, who had been driving a cart many miles from her own dwelling, and was found in it quite dead when it drew up at her own door. She had been suffering from a varicose ulcer, and whether it was from some injury, or simply from the effort made in her getting into her cart no one could tell; but the varix had burst, and she had there bled to death while the horse made its own way home.

But even where the patients have been in a more favourable condition to seek for help, it would appear as if the bleeding had gone on so insidiously, and given rise to such slight disturbance, that they have not thought it worth while to complain, or have been making attempts to restrain the hæmorrhage by their own unaided efforts till they were overcome by faintness and death. Moreau[2] relates a fatal case of a woman, who served as kitchen-maid in his lying-in hospital, who ruptured a varicose vein by striking the affected leg on a step on her way down to the kitchen at four o'clock in the morning. She was found by her fellow-servants an hour afterwards, powerless, and almost motionless, with just sufficient strength left to tell how she had tried in vain to stop the profuse flow of blood when she fainted away. Meissner[3] tells of a countrywoman carrying vegetables into town for sale, and becoming the subject of rupture of a varicose net of vessels in the leg, who did not apply for assistance till she was so far exhausted that a few minutes after she got into the house she happened to enter, she fell down and expired. And had the poor woman, whose saphena is before us, but taken alarm at an earlier stage, she might have received help of a more hopeful kind than could possibly be ministered to her when she was finally found speechless and moribund.

Treatment.—Tanner[4] seems to entertain the idea that the hæmorrhage in cases of rupture of varicose veins comes from the distal portion of the vessel; and states that, "where there appears to be the least fear of rupture of the coats of the vessel, it is advisable to explain to the woman and her friends how, in the event of its occurrence, pressure is to be applied below the wound." But Lisfranc[5] has noted that, in cases where he was dividing varicose veins with a view to their cure, the blood flowed much more copiously from the upper than from the lower end,—a circumstance which he attributed to the "want of reaction of the walls of the vessel on the column of fluid." And the observations of Velpeau[1] and Hasse,[2] on the attenuated and obliterated condition of the valves in such cases, afford an easy explanation of the retrograde course of the blood, and enable us to understand the rapidity of the fatal result. Further research may possible show that in those cases where the issue has not been so speedily fatal—as in that noted by Velpeau,[3] where the patient survived the rupture for four-and-twenty hours—there is not such a complete destruction of the valves higher up in the vein as to allow the column of blood in the large trunks to flow backwards, so that the escape can only occur more slowly from the distal extremity. On the whole, however, the profession is pretty much agreed that direct compression of the bleeding orifice is the treatment that ought to be employed in cases where this accident has happened; but it would be well if, outside the profession, we could get people to understand and remember that by simply exposing the injured part, and putting a finger on the bleeding spot, they might often save a fellow-creature's life, instead of, as they usually do, proffering every other variety of kindly but inefficient aid. The case related by Lebrun[4] illustrates this point. Quantities of blood had flowed from a woman's leg; and when he arrived the floor was covered with the blood, which was escaping through the cloths with which they had enveloped the limb without compressing it, and the patient had had a series of syncopes. All means were put in force to excite her: alcoholic frictions to the extremities and precordial region, urtication, irritation of the nostrils with a feather, inhalation of ether, smelling salts, etc.; but all in vain: the woman died trying to utter some broken words.

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[1] Lombard, Clinique des Plaies recentes, p. 249.
[2] Moreau, Traité pratique des Accouchements, tom. i. p. 558.
[3] Meissner, Frauenzimmerkrankheiten, band iii. p. 188.
[4] Tanner, on the Signs and Diseases of Pregnancy, London, 1860, p. 90.
[5] Revue Médicale, tom. iii. p. 348; 1827.

Part Second.

REVIEWS.

Delineations of the Brain in relation to Voluntary Motion. By JOSEPH SWAN. London: Bradbury & Evans: 1864.

THIS work is an anachronism. It takes up the anatomy of the nervous system where Bell left it, and ignores apparently the whole of recent modern research. This being the case, is the author right, and all the rest of the world wrong? Mr Swan has a long established reputation, founded on his dissections of the nervous system, to which the two collegial anatomical prizes were adjudged in 1825 and 1828 by the Royal College of Surgeons in London, and which were afterwards so beautifully drawn by West, and engraved by Finden, as to form standard and classical plates. At the outset of his career, as he tells us in those earlier works, he was the pupil of Monro and Fyfe at the Edinburgh University, and obtained the encouragement and patronage of Sir Astley Cooper, Mr Abernethy, and Sir Anthony Carlisle. His opinions, therefore, especially professing to be founded on dissections, even while at variance with prevailing ideas, are entitled to consideration.

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[1] Velpeau, Traité d'Anat. chir., 3d ed. p. 104.

[2] Hasse, An Anatomical Description of the Diseases of the Organs of Circulation and Respiration, Sydenham Society, 1846, p. 40.

[3] Velpeau, Nouveaux Éléments de Médécine opératoire, tom. i. p. 265.

[4] Lebrun, Nouvelle Bibliothèque Medicale, 1827, tom. ii. p. 275.

In the preface to the present volume the author tells us that, "A short time ago he published a pamphlet as a contribution to the knowledge required for supplying some of the deficiencies in what is known concerning voluntary motion. This pamphlet consisted of two papers, written at different periods, the one on the special communications of the motive segments of the crus of the brain with the intellect; the other on the separate parts of the brain producing the three voluntary powers of the muscles. He has now succeeded in obtaining a series of drawings proper for showing the motive centres in the brain and the tracts leading to their respective nerves, and from the difficulty of understanding verbal descriptions, he presumed they would prove useful if he published them. The subject in a physiological point of view is one of great interest, but especially as it relates to a very comprehensive section of pathology, and therefore requires to be studied by all means capable of elucidating its intricacies. The interval between the publications has allowed him to make further inquiries, and their results have been produced with the delineations."

The text, thus introduced, includes, we presume the two papers mentioned, and it is followed by eighteen plates, each of several figures, representing the various sections and dissections of the brain which illustrate the author's views. We think Mr Swan would have made himself more easily understood if he had given, as a key to those delineations, a diagram of the different regions, layers, and segments into which he divides the hemispheres and peduncles of the brain. These divisions are complex and intricate, and need a diagrammatic explanation to make their position and limits clearly intelligible.

Some idea of Mr Swan's peculiar views may be derived from portions of his opening paragraphs.

"Each hemisphere of the brain is formed into three peripheric or longitudinal regions, the median, the intercedent, and the external.

"*The Median Region* is appropriated entirely to the intellect; it occupies the convolutions placed near the median line; its superior surface extends outwardly as far as its convolutions are combined with the the corresponding ones of the opposite hemisphere by the great commissure. Its convolutions do not receive tracts from any organ of the body." Etc.

"*The Intercedent Region* is placed between the median and external; its convolutions are connected with the tracts of all the cerebral and spinal nerves, and occupy the whole length or periphery. The first or anterior convolution gives origin to the olfactory nerve. The tract of the second anterior convolution passes to the fifth, or most posterior segment of the crus of the brain, terminating in the oblique layer of the annular tubercle, which gives origin to the smaller portion of the fifth nerve, the sixth, the hard portion of the seventh, and the ninth." Etc.

"*The External or Motive Region* is placed on the outer side of the intercedent, and is formed of convolutions as centres for giving a voluntary power to the motive nerves. It consists of three layers of convolutions, divided into separate sections and tracts. The median or inner layer extends over a great portion of the surface, from which the intercedent region was removed. The second or intermediate layer is placed underneath, or on the outer side of the median or inner layer, and is not so extensive. The third or outermost layer, with the convolutions attached to its surface, occupies the remainder of the exterior region, and is the largest of all." Etc.

"*Segments of the Crus of the Brain.*—For convenience, the crus of the brain has been divided into five segments. The first, or most anterior, is placed at the inner side, and rather over the second; it is composed principally of the tract descending from the smaller portion of the intermediate layer for giving origin to the third nerve. The second is next, and contains the smaller portion of the median crescentic belt, proceeding from the fourth section of the first or inner layer, and the exterior crescentic belt proceeding from the fourth section of the third layer." Etc., etc., p. 3.

In regard to the mechanism for voluntary motion in the spinal cord, we are told that, "There are therefore four distinct pairs of tracts entering the spinal cord and giving off their respective nerves. The first pair is for the flexors and extensors of the spine; the second for the flexors and extensors of the lower extremity; the third for the extensors and flexors of the upper extremity; and the fourth for the flexors and extensors included in the restiform body."

In the eighteen plates, the arrangement and course of these various layers, sections, and tracts, are represented from different preparations. Although these dissections have doubtless cost much labour, it is impossible not to observe that they present a very artificial appearance. At the present day we doubt if any one would place implicit reliance on dissections professing to show, by means of the scalpel, the course of the tracts of fibrous matter in the spinal cord or the brain. The attempts in this direction by the ablest anatomists have, by their discrepant results, proved the insuperable difficulties, and the inadequacy of such methods of investigation when pushed beyond very moderate limits. How diverse and opposed, for example, are the descriptions given by anatomists of the tracts of fibres in the medulla oblongata, and how unsatisfactory is their account of the roots of the nerves when traced into the nervous centres from which they are given off?

But Mr Swan not only adheres to the mode of anatomical inquiry pursued in the time of Sir Charles Bell, his physiological notions apparently belong to the same date. We are told "The origin of the third nerve from the tract of the smaller portion of the intermediate layer at the first segment of the crus of the brain, and the origin of the smaller portion of the fifth nerve, the sixth nerve, the hard portion of the seventh, and the ninth from the termination of the larger portion of the intermediate layer in the oblique tract of the annular tubercle, make their functions sufficiently clear." (P. 4.) Has Mr Swan no idea of the importance of the grey matter of the nervous centres? There is every reason, we fear, to presume that he has not; in his "Delineations" he makes no mention and takes no account of it. And in the instance of motor nerves, in the passage just quoted, he returns to the old idea that the function of a nerve may be referred to its connexion with the tract of white or fibrous matter in the nervous centre, with which, it appears to be continuous. But such a doctrine is no longer tenable. The roots of a nerve must be traced not to white fibrous structure, but to grey substance; and the functions of a nerve depend not on the fibres, which are mere conductors, but on the vesicular, or cell structures, from which it originates. Surely the researches of Stilling, Wagner, and Clarke (not to mention a host of other anatomists), have established that position upon undoubted and sufficient evidence, which no scientific anatomist can, without presumption, neglect.

Mr Swan seems to rely entirely on the evidence of his dissections in proof of the views he put forward of the motive functions of his "external region" of the surface of the brain. At least we do not find any physiological or pathological proof adduced in support of them, although they are in opposition to the functions usually assigned to these parts of the cerebral hemispheres. We are told, indeed, that "the external region of the brain has motive powers, because it is continuously connected with the muscles, and thus partakes of their inherent qualities." We fail to perceive the cogency of the reason assigned in the last clause; and what immediately follows is equally incomprehensible. "The same intelligent spirit contained in and pervading the whole sensory becomes modified by the appointed structures; the more pure intellect is thereby confined to its own region; whilst the same spirit, by its connexion with the motive centres, acquires motive powers, and when active must exercise them in producing contractions of the muscles, and in this degree is made separate from the more pure intellect, but it preserves sufficient understanding for knowing the situation and functions of the motive centres, and can at pleasure excite one or more of them for the instant or for continuous motion. This exercise of its power is called the motive will, but its agency can only be manifested through the motions of the muscles, and not through any observable perception in the brain at the commencement of each motion. In the same manner the working of the spirit of the more pure intellect is only known by the results of its activity."

One more extract and we have done; it refers to locomotion:—"When the right side of the brain sends the voluntary impulse to the left side for walking, the left foot is placed on the ground, and from the left foot a returning impulse passes directly upwards, by the consentient activity of the left side of the spinal cord, to the sensitive and motive centres in the left side of the brain, and the motive impulse so received crosses for moving the right leg. In this manner walking may be continued for a length of time until the will stops it, whilst the intellect is left free for various purposes, such as thinking and conversing."

How to reconcile this account with the results of experimental physiology or the pathology of paralysis, we do not know. But it is abundantly plain, from what has preceded, that however excellent an anatomist Mr Swan may be, he does not understand the conditions of the problems of the modern anatomy and physiology of the nervous system, nor the means by which these are to be worked out. To solve them more laborious and minuter research is necessary than any Mr Swan has undertaken; more, indeed, than any anatomist has yet successfully accomplished.

Therapeutics and Materia Medica. A Systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History. By ALFRED STILLÉ, M.D. Second Edition, Revised and Enlarged. Two Vols. Philadelphia: Blanchard and Lea: 1864.

Medicines, their Uses and Mode of Administration. By J. MOORE NELIGAN, M.D. Sixth Edition. Edited by RAWDON MACNAMARA, M.R.I.A. Dublin: Fannin & Co.: 1864.

A Manual of Materia Medica and Therapeutics. By J. FORBES ROYLE, M.D., F.R.S.; and FREDERICK W. HEADLAND, M.D., F.L.S. Fourth Edition. London: Churchill: 1865.

Elements of Materia Medica. By Dr WILLIAM FRAZER. Second Edition. London: Churchill: 1864.

THE recent publication of the British Pharmacopœia has introduced so many changes in Pharmacy as to have rendered necessary new editions of the existing works on Materia Medica. The authors of most of these have already responded to the requirement, and in the preceding list are to be seen some of the results.

We have placed first on the list Dr Stillé's great work on Therapeutics. When the first edition of this work made its appearance nearly five years ago, we expressed our high sense of its value as containing a full and philosophical account of the existing state of Therapeutics.[1] From the opinion expressed at that time we have nothing to retract; we have, on the contrary, to state that the introduction of numerous additions has rendered the work even more complete than formerly. We can cordially recommend to those of our readers who are interested in Therapeutics a careful perusal of Dr Stillé's work.

We shall content ourselves with giving one or two extracts. The first contains the author's conclusions regarding the action of digitalis:—

"It seems to be an inevitable conclusion from the preceding statement of the phenomena caused by the preparations of digitalis, that the primary action of the medicine in ordinary doses is not depressing, as it is generally stated to be; that, on the contrary, it is a stimulant of the healthy system, and the moderator of morbid action depending upon general or local disease. When taken in excessive or poisonous doses, it appears with equal clearness to be a direct sedative, paralyzing the heart and nervous system, and so destroying life. This double action it shares with a great number of medicines, and, among those affecting the nervous system, with none more strikingly than with opium. Finally, it may be stated as a part of the experimental history of the drug, that, according to Homolle, digitalin is altogether destitute of the nauseating and emetic properties of digitalis, while it represents the diuretic virtues of the medicine and its sedative operation upon the heart. The account which has been given above of the action of the plant and its representative principle, do not permit us to accept so absolute a conclusion. At the same time, they prove the superiority of digitalin in the mildness, safety, and certainty of its operation."

Veratrum viride has been much extolled by recent American writers as exerting a powerful sedative action on the heart, and as being a valuable remedy in cardiac, inflammatory, and other diseases. In this country the results of its employment have been less favourable, and it has been found that very severe nausea has attended or preceded its sedative action. Dr Stillé gives what we consider a very fair statement of the action of this medicine, and in his conclusions does not express by any means so favourable an opinion of it as is entertained by many of his countrymen. He says:

> "The most remarkable quality of American hellebore is its sedative action on the pulse. Osgood was the first to state that by the exhibition of full doses, the pulse, when ranging from 75 to 80 in a minute, may be reduced to 35 or 40 in the course of a few hours, and become 'small and creeping;' but Dr Branch declares that 'when used as it ought to be, it reduces the

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[1] See this Journal for September 1860.

frequency but never the strength or volume of the pulse.' Upon this point Dr Norwood is not very explicit, but it may be inferred from his statements that he admits the reduction of force as well as of frequency in the pulse, since he describes among the ordinary effects of the medicine 'great coolness of the surface and often icy coldness.' Yet he does maintain that the reduction of the frequency of the pulse is not dependent upon the occurrence of nausea or vomiting.

"The effects of this agent when given in an excessive dose have been described by Osgood, Pendleton, Platt, Colegrove, and others, in nearly the same terms. They consist of faintness, somnolency, or coma, dimness of sight, dilatation of the pupils, vertigo, headache, rarely delirium, impaired muscular action, general numbness, slow and infrequent respiration, hiccup, a pale cold skin covered with clammy sweat, persistent vomiting, excruciating pain in the præcordia, profuse watery diarrhœa in some cases, and a small, infrequent, and generally feeble pulse. Alarming and apparently dangerous as these symptoms are, it does not appear that they have terminated in death. On the contrary, they have generally been dispelled with facility by suspending the use of the medicine and administering diffusible stimulants. The effect of alcohol in counteracting the sedative action of the poison was shown in the case of a physician who took, by mistake, an ounce of the tincture at one draught. Extreme nausea, vomiting, and some dyspnœa, were the only evil consequences of this large dose. Neither in this case, nor in some others marked by extreme prostration, sedation of the heart, and vomiting, even of blood, were the bowels disordered.

"This medicine is alleged to have produced abortion in the pregnant female, by Dr W. A. Brown, of Georgia, and others. On the other hand, numerous cases might be quoted in which it was taken with the intent of producing this result, but ineffectually. Undoubtedly, however, an agent which, like this, usually excites prolonged and violent vomiting, ought not to be administered to a pregnant female by a prudent physician, although it may be true that it exerts no specific action upon the impregnated uterus.

"It is evident that conclusions so diametrically opposed as these are to one another can furnish no ground for a rational theory of the mode of action of the medicine we are examining. It will be observed that the capital discrepancy is upon the question whether it is a stimulant or a sedative, and that while some regard it as having, under all circumstances, a lowering action on the system, involving a reduction in the frequency of the heart's pulsations, others appear to consider this effect, at least so long as toxical symptoms do not arise, as resulting from an augmented tone in the action of the heart. These opposite doctrines are analogous to those which we have seen to prevail in regard to the operation of digitalis, but the grounds upon which a choice should be made between them are not as well determined as in the case of the latter medicine. The only recorded fact which appears to bear directly upon the question is, that observers are agreed in stating that although the medicine reduces the frequency, it does not diminish the strength of the pulse, until nausea, vomiting, and the associated symptoms make their appearance, from which it may be inferred that debility and general sedation are its secondary effects. More complete investigations of this point, and indeed of the whole subject, are, however, still required."

The last extract we give contains Dr Stillé's account of "*Packing in the Wet Sheet.*

"This is the most useful and efficient of the processes employed. A large and coarse sheet is thoroughly wrung out of cold water and spread over a blanket previously extended upon the bedstead. The patient lies at full length in the middle of the sheet, which is at once closely and thoroughly wrapped around him, and the blanket is applied over it in the same manner. A feather bed, or a down covering, is laid over all. This is a very powerful process, and deserves to be withdrawn from the hands of the charlatans who have converted it into a deadly weapon. Its first impression on persons in feeble, or even ordinary, health, is one of disagreeable coldness, but this soon passes away, and is followed by a mild and grateful warmth. The temperature of the skin gradually rises until it feels hot, the face becomes injected, and general perspiration breaks out, which, if desirable, can be maintained by repeatedly administering small draughts of cold water. When complete reaction has taken place the wrappings are removed, and the body is washed with cold water, or the patient plunges at once into the cold bath. This, at least, is the regimen in chronic diseases. In acute affections, the sedative action of cold is rather sought for, and as soon as reaction manifests itself, fresh wrappings are applied, and the process is repeated until the pulse is permanently lowered.

"The stimulant or sedative effects (for they may be either) of the wet sheet are obtained upon a small scale, and, locally, by means of wet compresses. When these are saturated with water, and frequently renewed, they act as refrigerants, and diminish the quantity of blood in the part to which they are applied; but if allowed to remain longer in place they are simply emollient. The so-called hydropathic belt consists of a bandage, five or six inches in diameter, and long enough to go twice or thrice around the body. After having been dipped into cold water, and carefully wrung out, it is very closely wound around the trunk, and a longer and wider dry band is accurately applied over it. It is renewed as often as it grows dry, which is nearly every hour. Compresses are applied in the same manner upon various parts of the body, and particularly upon joints affected with rheumatism, and upon the skin over diseased viscera. The exciting effects of the belt are 'manifested in what may be called a depurative sweat, which is at first clear, and is difficult to be procured; but as the treatment advances it becomes more profuse, is viscid and glutinous, of a dark yellow and brown colour, sour, and even fetid in its smell, and is sometimes impregnated with the most disagreeable odours. When these morbid phenomena appear, the perspiration may be considered of a critical nature, as if it were the elimination of a morbific humour.' The compresses, as well as the belt, produce an eruption of a vesicular, pustular, or simply an erythematous character, and the more abundantly in proportion as the clothes are drier. This effect appears to result from the softening of the cuticle, and the irritation produced by the close contact of the compress or bandage. It is sometimes a useful result, and may, in certain cases of chronic disease, be advantageously substituted for vesication or pustulation by pharmaceutical irritants."

Dr Neligan's work has long been recognised as a standard one, and it has enjoyed a circulation to which few medical books attain. Owing to the premature death of its lamented author, the present edition has not had the benefit of his supervision. But Dr Macnamara, who has edited the volume before us, informs us that his connexion with the work does not merely date from Dr Neligan's decease, but that he had been for some time co-operating with him in the revision of a new edition, the appearance of which was only postponed until the publication of the British Pharmacopœia. Dr Macnamara has performed his task in a very satisfactory manner. Of course, in the main features of the work there is little change; the therapeutical statements, in particular, are generally retained in Dr Neligan's own words. But numerous changes have been necessary; and so much new matter has been introduced that the volume now consists of 758 pages. As a specimen of Dr Macnamara's own style, we extract what he says regarding the action of the Calabar bean :—

"For the first accurate description of the effects produced by this medicine we are indebted to Professor Christison, who carefully examined into its physiological effects upon rabbits, and subsequently upon his own person. He describes the symptoms following the mastication and swallowing of twelve grains of the seed as being, first, giddiness increasing in intensity, torpor, slight twitching of the pectoral muscles, sluggishness of articulation, great irregularity of the pulse as also of the heart, accompanied with tumultuous action of that organ, great pallor of countenance, extreme prostration, accompanied with marked loss of muscular power, which he attributed rather to a want of an exercise of the powers of volition than to any absolute deficiency in the muscular power; the intellectual functions remained perfectly unaffected. In a wholesale case of poisoning with this bean in Liverpool, the symptoms described present a close resemblance to those placed on record by Dr Christison; but in addition to those he described, well-marked *contraction* of the pupil of the eye was observed, a symptom which recent observations on the action of this medicine had led the gentleman, in whose charge the cases were, to look for. The only use, so far as I am aware, to which the Calabar bean has been as yet applied is to produce contraction of the pupil. This can be done either by introducing a minute proportion of the alcoholic extract or of a paper prepared for the purpose into the eye, when contraction of the pupil will present itself in a period of time varying from ten to thirty minutes. Mr Squire has introduced to the notice of the profession two preparations of Calabar, one a paper impregnated with the alcoholic extract, a second composed of gelatine similarly impregnated, and rolled out into sheets; both are divided into small squares, one of which is generally sufficient to produce the desired effect. For this important addition to our ophthalmic therapeutics we are indebted to Drs Fraser, Robertson, Wells, etc.

"In poisoning by the Calabar bean, an accident not unlikely to occur from careless keeping, and the blandness of its taste, the treatment should be first emetics, then strong coffee and diffusible stimulants."

The next work on our list, by Drs Royle and Headland, is also too well known to require any introduction from us. It forms one of Mr Churchill's excellent series of Manuals, and has deservedly attained to the honour of a fourth edition. Though the volume is extremely portable, in consequence of the smallness of the type and the condensed style of the authors, it contains within a small space a large amount of valuable matter, and will be found a very trustworthy book of reference both by the student and practitioner.

The last work we have to allude to is by Dr Frazer, lecturer on

Materia Medica to the Carmichael School of Medicine. It is described as a second edition; but its author states that, "owing to the numerous changes which have taken place in the Materia Medica, its pages have been entirely rewritten." Dr Frazer has classified his materials according to the natural history arrangement, which we agree with him in thinking superior to one founded on the establishment of supposed therapeutical groups. After a short introduction, Dr Frazer commences with the consideration of the articles of the materia medica derived from inorganic substances; he next treats of the vegetable materia medica, the various substances being described under the natural orders to which they belong; next comes the animal materia medica; finally, there is a supplementary list, containing preparations which are not officinal. The mode in which the plan has been carried out is highly meritorious. The descriptions are accurate and well-written, and the therapeutical directions clear and precise.

In looking over the list of recent works on Materia Medica, our readers must have been struck by the absence of one name. While the London School of Medicine is worthily represented by Drs Garrod,[1] Royle, and Headland, and the Dublin by Neligan, Macnamara, and Frazer, no new edition has been issued from Edinburgh. Christison's Dispensatory is a work second to none; and in its absence we must look on our list of books on Materia Medica as incomplete. Its distinguished author has indeed gone through an amount of work which might well entitle him to retire from the field of authorship; but we are selfish enough to express the hope, in which we feel sure we shall be joined by the whole medical profession, that he will at least superintend the preparation of a new edition of his admirable work.

Hand-Book of Dental Anatomy and Surgery for the Use of Students and Practitioners. By JOHN SMITH, M.D., F.R.C.S., Surgeon-Dentist to the Royal Infirmary, Royal Hospital for Sick Children, etc., Edinburgh. London: Churchills: 1864.

DR SMITH, by the publication of this little work, has conferred a real benefit upon medical students and practitioners. Most works on Surgery say little or nothing of diseases of the teeth; while special works on Dentistry are not of a nature to suit their requirements. Accordingly, on entering upon practice, the practitioner often finds himself consulted regarding a class of cases of which he has no previous knowledge, and regarding which his books afford him little information. It has been Dr Smith's object to supply

[1] The Essentials of Materia Medica; reviewed in the number of this Journal for August last.

this want; and in the work before us, consisting of 130 pages, he has supplied exactly the kind of information required. The book commences with a brief account of the comparative anatomy of the teeth, and a clear description is given of the different forms met with in the various classes of animals. The second chapter, "On the General Characters of the Teeth in Man," gives a very excellent account of the arrangement and order of appearance of the teeth; their external characters; the peculiarities of their roots and fangs; their development, and their minute structure. The following statement no doubt accounts for cases where a third dentition has been supposed to have taken place:—"Anatomists consider 44 instead of 32 to be the typical or model number of teeth belonging to those animals comprising the mammalia, and therefore they consider that certain teeth in man, and in several other animals, are suppressed; 44 ought, as it were, to be present, but 12 of those in man are suppressed, reducing the number to 32. A knowledge of this diminution in the typical number of teeth in man is so far interesting, as it affords one explanation of the occurrence of what is sometimes believed to be a third dentition,—the growth of a third series of teeth; as well as the occasional existence of extra teeth in the mouth. Such supernumerary members, in both cases, frequently indicate merely the tendency towards a return to the original or typical number, which had been in the first instance departed from."

The third chapter treats of the General Anatomy of the Maxillary Apparatus, and the following passage is a good illustration of the manner in which Dr Smith combines practical advice with scientific information:—"These peculiarities in the alveolar cavities are important to be kept in mind with reference to extraction of the contained teeth. The exact adaptation of the alveolar walls to the form of the fang lodged within them—especially when considered along with the curvature so generally assumed by these fangs—will at once indicate the impossibility of removing the teeth from their sockets by a straight pull. On the other hand, the thinness of the external alveolar wall—coupled with the fact that, like the stones forming an arch, the fangs of all teeth are thickest on their outer aspect—indicates that a force applied so as to dislodge the teeth in an outward direction, will tend very materially to facilitate their removal. In the same manner, the soft and spongy texture of the transverse partitions of the alveolar ridge renders it apparent how, in using the elevator or tooth-punch, a root or stump will be easily extruded, on these walls or partitions yielding before the power of such an instrument when properly directed. The disadvantages, again, attending an unacquaintance with such matters are too obvious to require any comment here."

Dentition and its Disorders form the subject of the fourth chapter. The following remarks appear to us eminently sound:

> "If no indications of inflammation exist in the mouth—and no tooth may, according to the natural course of events, be expected to appear for a con-

siderable time—some substance should be given the child to rub against its gums, care being taken that such substance should not be of a nature calculated to lead to danger by breaking or passing into the mouth—such as crusts of bread, small keys, coins, and the like. In this way, two indications may be fulfilled, namely, first an increased flow of saliva is produced, and a derivative action, as well as a kind of fomentation, to the parts is obtained; and second, the alveoli and gums seem to be rendered thinner by such means, probably by the friction and pressure accelerating their absorption.

"Where, on the other hand, the gum is red or hot, or appears tense and stretched by the pressure of the crown of the tooth beneath, more active measures may be employed. The chief of these is what is termed Scarification. This consists in making an incision down upon the growing tooth, the object being to relieve congestion existing in the highly vascular tissues concerned, and sometimes to facilitate the egress of the tooth, by dividing any bands or bridles of tough mucous membrane opposing its escape. The method of performing this little operation in the easiest manner will be found to be that of seizing the affected part of the gum betwixt the thumb and forefinger of the left hand, and by means of a sharp-pointed, curved, and somewhat short bistoury—that known as Syme's abscess knife being the most suitable—making a crucial incision completely through the tissues covering the tooth. There seems little doubt however, that although this proceeding may in one way be beneficial, in another it is possible to be injurious; because, while good results may follow in the manner already specified, yet the delicate structures involved in the still rudimentary condition of the tooth must be, to a certain extent, interfered with. It should, therefore, be adopted only as a last resource, and after mature consideration. Where apparently causeless convulsions, for example, occur during the active stage of dentition, or where the eruption of previous teeth has been uniformly attended with much suffering, less hesitation may be necessary; but in other cases some local evidence of irritation should always be present before having recourse to such practice. And one other point to be mentioned is, that some attention must be paid to those periods in the process of dentition when the various teeth may be naturally expected to make their appearance; because it is at such periods that this operation is most likely to be attended with benefit."

The fifth chapter is occupied with a description of the Teeth; and the subject is treated of in a clear and thoroughly practical manner. The following passage explains, satisfactorily we think, why the lower incisors are less exposed to caries than any other teeth:

"While the first molars seem to be the teeth most liable to caries, the lower incisors are those least so. No satisfactory explanation of the prevalence of caries in certain teeth more than in others has ever been given; but the immunity of the lower incisors may probably be accounted for by the presence of the saliva lodging in larger quantities in their vicinity. And this is a circumstance deserving of some attention. Wherever the saliva lodges to the greatest extent, there we find the largest deposit of what is known as tartar or salivary calculus, as it in reality is; and wherever such substance is found to be most abundant, there decay will be found to be most rare. This is well exemplified in the case of the lower incisors. Here the saliva lodges continuously, and in considerable quantity; and the result is that the greatest accumulations of tartar are always to be met with in this locality. Now, of all other teeth the lower incisors are least affected with caries, those of the upper jaw being, on the other hand, very liable to this disease. The explanation here, then, seems to be, that the peculiar alkaline character of the saliva neutralizes any acid which might be otherwise conducive to the loss of these teeth, and in this way serves as a defence in some measure against local agencies of a destructive character to these organs."

The mode of extraction of teeth, and a description of the instruments to be employed form the subject of the sixth chapter. The remarks as usual are clear and thoroughly to the point; and no practical advice is given which is not based upon anatomical considerations. The following is what is said regarding the key:

> "Another instrument, formerly much more employed in tooth extraction than it is now, is the key. This instrument, from the immense power possessed by it, is liable to abuse in its application. Its form is too well known by all who are likely to be interested in these remarks, for any description of it to be here required. Suffice it to say, that when judiciously handled and restricted to such cases as really demand its use, the key is of very great service indeed, and to discard its employment altogether, would be relinquishing an instrument which, in certain cases, no other could replace for simplicity and effectiveness. The cases in which the key is principally useful are those where the forceps incline to break down the remainder of a tooth without obtaining a sufficient hold for its removal, and where, on the other hand, the elevator would be applied at a great disadvantage for want of sufficient purchase. Such a case may be exemplified by the circumstances of the remains of a molar tooth, upper or under, requiring removal, while as yet not only firmly impacted in the jaw, but tightly jammed between the adjoining sound teeth, especially where the decay has been extensive and deep on one or other side of the neck of the tooth, leaving the outer or inner wall tolerably sound. In such a case, by fixing the claw on the sound side of the tooth, and turning it out in the opposite direction, it will in most instances be removed more easily and expeditiously by the tooth-key than by any other instrument whatever."

The seventh chapter treats of Filling or Stopping Teeth; and the concluding one, "On Chloroform, and other Anæsthetics in Dental Surgery," is of especial value.

It only remains to us to recommend, as we do most warmly, Dr Smith's work to all who in the course of their practice are likely to be consulted regarding diseases of the teeth.

Part Third.

PERISCOPE.

PRACTICE OF MEDICINE.

CLINICAL OBSERVATIONS ON TYPHUS FEVER. BY WM. T. GAIRDNER, M.D.

GENTLEMEN,—We are now in the midst of a very considerabe epidemic of typhus fever, and under the impression that it might be useful to you, as well as a relief to the medical staff of the fever-house, I have been on duty in two fever wards since the 27th of September last, and have seen to an end considerably upwards of a hundred cases, of some of which I gave you some account in a previous lecture. My winter engagements, however, oblige me to abandon fever duty at present, and we must consider the programme of clinical instruction in fever as cut short abruptly at this date. We have had, however, a very considerable field of experience, and as I distinctly intended from the first that these cases should be made subservient to your instruction, I am unwilling to depart from them now without some attempt to clear the way to principles which may possibly be of service in the future management of the epidemic. In the former lecture I gave you details, drawn from our experience at that time, illustrative of the symptoms and course of the fever we have had lately under treatment. It would have been useful to follow out this inquiry by a more careful analysis of the fever, so to speak, and a consideration of individual symptoms and complications; but this is precisely what I find we must give up attempting at present, and therefore I have determined to publish in the January number of the Glasgow Medical Journal an abstract of all our cases, that you may see for yourselves what sort of cases they were, and be able to refer to them as the basis of facts upon which this lecture, and another which will follow it, will be founded. You will find in the Journal alluded to, the notes of 68 cases of febrile disease in the male ward, and 58 in the female ward, numbered *successively*, exactly as they were admitted. Of these 68 male cases we regarded 58 as genuine typhus, established as such within at least a few days after admission, and beyond all reasonable doubt; and of the 58 female cases we regarded 50 as genuine typhus, the remainder being cases one or two of which *may* have been typhus, but which, on the whole, we regarded as either doubtful or decidedly *not* typhus fever. Every case noted as typhus was so noted on grounds which you may read in the abstract to which I have referred, and which, I trust, will bear the strictest investigation.[1]

Now this disease—typhus fever as witnessed by us—is one of which the following brief summary may be given. It is a disease very decidedly and strongly febrile, in which the pulse has rarely been less frequent than 100, often as frequent as 120, not seldom 130, or even 140 or more, in the minute; but in this last case usually, in the adult at least, indicative of grave danger. Moreover, typhus fever as we have seen it of late, is a disease almost always indicated by a peculiar rash (which has been so often described that I will not linger on the description now)—a rash appearing during the first week, and persisting during a large part of the second week, and then gradually dying away. Again, the typhus with which we have had to do has been marked by very decided, and not at all equivocal nervous symptoms of the kind called typhoid, of which the first and least characteristic was sleeplessness, extending over several nights; the next delirium, often with tremors and agitation, muttering, and more or less contracted pupils; the last and worst, coma, with subsultus tendinum, and failing of the vital powers, pulse, and animal heat, from which last and worst state few escaped with their lives; but happily coma was present only in a small minority of the cases. We have also had a considerable number of complications, of which I will only name the following at present:—Bronchitis, frequent and severe in the latter part of our two months' experience; diarrhœa, frequent, but not very severe; vomiting; epistaxis; menorrhagia; suppuration of the ears, with nervous symptoms apparently depending thereon; abortion; jaundice; laryngeal complication; erysipelas; paralysis of portio dura; inflammation of the femoral vein; albuminuria. A considerable list assuredly; but then several of these complications were observed only once. On the other hand, several of the deaths were manifestly due to the complications, as you will see further on.

The next point I wish to notice in regard to the course of typhus fever as observed in these cases (and it is one of such importance in a practical point of view that I shall have little else to bring before you to-day), concerns the *phenomena of the crisis* (so-called), especially in the cases which have ended favourably. As a great number of patients affected with typhus fever get well, while a certain proportion die, I need hardly point out the immense importance of noting the circumstances which determine one or other of these results, and particularly the symptoms which, in particular cases, give you the earliest assurance that the disease is going to abate. To this inquiry we have given particular attention, and the result is, in the first place, that the well-known term *crisis*, in the old and figurative but still strictly true application of it to some febrile diseases, is scarcely a description of what occurs in the fever with which we have had to do. Let me recall to your remembrance the meaning of that word. Etymologically it presents to the mind a mysterious supernatural authority or law—a Nemesis (let us say)—passing *judgment* on the issue of the disease, with power to make or mar, and that instantly. At a certain hour the judgment goes forth: "This disease shall depart, and the patient shall live." And forthwith the sentence is obeyed, the disease vanishes. That is, or was, the idea of a crisis, and it was further observed to be accompanied in many cases by certain evacuations which were called on this account *critical*, and supposed to be, in fact, the means by which the disease, or the material cause of the disease (*materies morbi*) was carried off or expelled from the body. I am not concerned at present either to attack or to defend this old pathology; but there can be no doubt that this idea of a crisis harmonized well with the facts of many fevers as observed by those ancient physicians. But in typhus fever, as we see it at present, there is absolutely no trace of a crisis of this rapid and decisive kind. The fever is not, apparently, judged or condemned, and forced to vanish suddenly, after the manner of an evil spirit fleeing from the sentence of an invisible power. Nor does the resolution of the fever almost ever take place in company with marked evacuations, such as could possibly have suggested the idea of a purification of the system from a "matter of disease." On the contrary, the whole process is devoid of startling phenomena; it is gradual, distributed over days, and often for days imperceptible to all but the most minutely accurate observation; there is neither sweat, nor diarrhœa, nor any striking change in the urine; and if any of these should by chance occur, they are just as likely as not to be non-critical and of unfavourable import. This is no new doctrine as regards typhus fever, but it requires to be brought into stronger relief than in most of the current authorities; and the facts of this particular epidemic, aided by the more general recollections of a good many years past, enable me to do this with perfect confidence as to the accuracy of the picture which I shall draw for your instruction, not from books, but from direct observation. For although the disease with which we have had to do has had no *crisis* in the ancient sense of the word, it has nevertheless had a very regular and *normal course of increase and decline*, to which I now invite your best attention. The character of the disease has been, that it rises, often with perfect regularity, though quite gradually or even imperceptibly, to what may be called an acmé; and then, with no less regularity, and not less gradually, declines towards the state of complete convalescence, often without any evacuations or sudden startling phenomena of any kind throughout. The rise and the fall of the fever are equally gradual; and the curve indicating the fever—if one might represent it roughly (but still truly) in the form of a diagram—would not be after the ancient idea of crisis, thus:

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[1] One case which, on the whole, I should have preferred to place amongst the doubtful ones (Case 57, females), has by inadvertence been placed amongst the typhus cases, and was only detected after the returns were made up; but as the variation would be merely fractional, indeed hardly appreciable, and as I believe after all the case was one of typhus, I have not thought it necessary to alter the totals on this account.

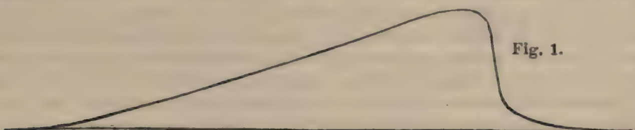

Fig. 1.

but rather thus:

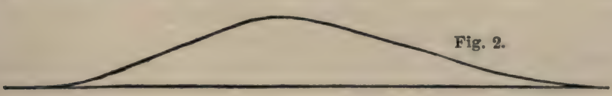

Fig. 2.

an acmé, or single summit, or maximum point of development, being a characteristic of both the diagrams; but the one which represents typhus differing from the ideal one of a crisis in the perfectly gradual character of the decline. From this normal type I hold that typhus fever very rarely departs, unless disturbed either by complications or by injudicious treatment; and therefore it is literally of immense importance to your comfort and security in treating the fever, as well as to the patient's security, which is of more importance even than yours, that you should know accurately the rules by which you are to watch for this slow and gradual crisis.

Now what are you to look to with the view of catching the first glimpses, the first favourable indications of this important movement? I believe that, speaking practically, and speaking of the great mass of cases, it is *the pulse.* The internal heat, as determined by the thermometer, is, I believe, a very valuable indication; but the thermometer is so much more cumbrous as a means of investigation, so much more difficult to apply, and takes up so much more time in the case of a busy practitioner having many cases to observe, that I believe the oldest of all studies in fever, that of the pulse, will hold its ground practically, at the bedside, and in ordinary investigations, against every other. It is easy, however, to study pulses in fever, without arriving at an idea of how valuable and delicate a test of the crisis we have in the pulse, when watched for days together, and in connexion with an estimate as accurately made as possible, of the duration of the disease. Of this I am sure, because I have again and again had my own observations criticized and doubted by good observers, and have had to come back to nature again and again to establish to my own satisfaction the points to which I have now to direct your attention, as regards the pulse-rate in typhus. What I have now to tell you has been carefully tested by the facts of the present epidemic, but is also consistent with facts observed by me on various occasions, at least since the year 1857, and particularly referred to in a publication of the year 1859, on the history of epidemic fever in Edinburgh.[1]

There is one stipulation I must make with those who desire to follow out this inquiry with the view of testing the normal mode of the crisis in typhus fever. It is, that *as many cases as possible should be left to their natural course, unaffected by either drugs or stimulants.* Of course, I should never think of recommending to you this mode of proceeding unless I believed firmly that the disease could be so treated in many cases with advantage to the patient, as well as with increase of knowledge to the physician. But this is precisely what I do believe most firmly, and what I mean accordingly to occupy another lecture in impressing upon you by arguments drawn from experience. Meantime let me state, as matter of opinion, that not only am I convinced of the safety and expediency of leaving many cases of typhus to take their *normal* course, but I further believe the normal course may be very easily altered for the worse by what is called treatment; and, in particular, as regards the period of the crisis, I believe that the habitual or constant exhibition of drugs and stimulants has a great tendency to mask the disease, to disturb or to retard the crisis, and by so doing to increase the mortality. This is an opinion formed after a most careful observation of particular cases in detail over many years; and I advance it now, instead of waiting till next lecture for its fuller development, chiefly in order to add, that you cannot possibly observe the natural course of typhus unless you can make up your mind beforehand to leave many cases to follow their natural course. I am fully convinced that there are many practitioners who scarcely ever see a normal case of typhus, owing to their perpetual and systematic interference by drugs and stimulants; and it is even a question with me if the written descriptions of previous epidemics have not been largely vitiated by this cause, the disease being to some extent, as it were, disguised, or perverted from its natural and favourable course, by the treatment. How far this may have been so in fact I have no means of knowing exactly; but I have had abundant opportunities since the year 1857 of ascertaining that typhus fever, both in Edinburgh and Glasgow, has followed the normal course now about to be briefly indicated.

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[1] Edinburgh Medical Journal, July 1859; and Clinical Medicine, 1862, p. 164.

There are, however, one or two further precautions that must be taken before you can hope, I do not say to avoid disturbing the crisis in typhus fever, but to avoid killing your patients outright, or rather, letting them die of sheer neglect. You must feed your patients, and you must feed them chiefly on milk. Milk or buttermilk is with me the staple food in typhus; and I will even say that I know no other food that can be depended on. Yet I see, and always see with a new surprise, descriptions of the treatment and dietetics of fever in which not a word is said about milk, and a great deal about beef-tea, wine, whisky, brandy, and all manner of things supposed to be more strengthening, or stimulating, than milk diet. Now, I tell you frankly that treating fever patients without plenty of milk is a thing that I do not understand at all; for I suppose I have not treated a single case of fever of any kind for the last fifteen years (I cannot make precise statements beyond that date) without milk, and I always proceed on the understanding that milk in fever is the one thing needful as diet—always to be given, and given liberally, whether specially ordered or not. To give wine, whisky, and beef-tea, while withholding milk, is simply, in my opinion to destroy your patient; and the more wine or whisky you give, while withholding milk, the more sure you will be to destroy your patient soon, because you are thereby superseding the natural appetite (or what remains of it) for a nourishing and wholesome diet, by a diet—if it can be so called—which poisons the blood and checks the secretions, and alters for the worse the whole tone of the nervous system and of the digestion and assimilation. I believe that infinite mischief has been done in typhus fever, and in all fevers, by giving wine, and withholding or not giving milk. Under a false theory of administering alcoholic food, it has resulted, not only that natural and genuine food has been withheld, but that the small remaining amount of appetite for such food has been obliterated, and not unfrequently, even at an early stage of the disease, the patient has been practically disabled from taking any proper nourishment at all. I know, unhappily, as a fact, that not only doctors, but nurses and patients and patients' friends are readily brought under the influence of this fatal delusion, that alcoholic liquors can, in fevers, take the place of natural food. But it is none the less a fatal delusion, and I warn you solemnly against it; all the more that the patient is absolutely in your hands, and you can obtain no guidance from his natural instincts, if you begin by overwhelming him with large doses of alcoholic stimulants. I have been very careful, at least for fifteen years past, to avoid this error, and I believe that any success I may have had in managing fever has been more due to this than to any other cause. But I allude to the matter now simply to say that if you would observe the natural or normal course of typhus fever, with respect to the crisis (as proposed at present), you must absolutely make up your mind to feed your patient naturally, and not to stimulate him. An opposite course leads to consequences which I may consider more in detail on another occasion; but what I am anxious to say now is, that it tends to disturb the course of the fever, and to retard the crisis.

Further, you must, if you would observe the natural course of typhus fever, give your patient plenty of fresh air. This is now so very general an admission that I need hardly have alluded to it, were it not that adequate ventilation is still apt to be forgotten in treating fevers in the houses of the poor or in badly constructed hospitals. With adequate ventilation, you must of course, especially in winter, have the means of warmth; and great attention must be paid to cleanliness, so far as this is possible in bedridden and exhausted patients.

These, I think, are all the precautions necessary, I do not say for the proper treatment of typhus fever, but for the proper observation of its natural course in normal cases.

Now I venture to assert—and I make this assertion not by way of theory or opinion, much less prediction, but as a statement of facts, founded on a rather extensive experience ever since 1855 or 1857—I venture to put it forward as a law, or generalization, with respect to typhus fever: *That in a large proportion of cases typhus fever, left to its natural course, and treated with abundant milk diet and without drugs or stimulants, will have its natural crisis before the twelfth day.* Of course you will understand that I use the word *crisis* in a modified sense; for I have just been explaining that there very rarely is in typhus a true crisis, in the ancient sense of the word. What I mean is, that most commonly before the twelfth day, very frequently as early as the ninth or tenth day, sometimes, though rather rarely, even before the ninth day, indications are to be perceived that the disease is about to terminate favourably; and that these indications are to be trusted, even when some of the most formidable-looking symptoms continue present to a later period. And this I confidently assert to be a very general law, or *norm* (to use a word borrowed from the Latin, but not yet naturalized in English, except in the adjective form *normal*), of typhus fever as observed in Edinburgh and Glasgow of late years. Observe, I do not venture to assert that it has always been so, or will always be so; on the contrary I have myself remarked the tendency to a delayed crisis as an epidemic fact at certain seasons; but in a large proportion of cases, and excluding certain exceptional groups of cases, it has been so, in the main, in every year from 1857 to 1862 in Edinburgh, and during a large portion of the epidemic of the last two years in Glasgow.

If you wish to have the satisfaction of getting the earliest information of this early crisis in typhus, *watch carefully and constantly, from day to day, or several times a-day if need be, the rate of the pulse.* For this is, speaking practically, the guiding clinical fact in the natural history of typhus fever. Making allowance for certain obvious, and a few not so obvious, causes of accidental disturbance, you will find that the pulse pursues a very regular course of rising frequency up to the acmé, and declining frequency beyond the acmé of the fever; and that although the other symptoms very surely follow the pulse in uncomplicated and regular cases, yet they often follow at some distance behind: so that the crisis can be ascertained for practical purposes, and particularly for the great purpose of guiding treatment, more accurately and effectually by frequent noting of the pulse-rate than by any other method. I have ample proof of this in the cases of the last two months, for which I will refer you to the hospital case-books, and to the abstract in the *Glasgow Medical Journal* for January 1865.

This accurate and frequent noting of the pulse-rate in typhus, in connexion with the day of evolution of the disease, is of immense importance as regards the prognosis; for not only are you able to predict the recovery with greater confidence, and at an earlier period, than you could do otherwise, but you have often the first intimation of complications, either during the fever or during the convalescence, through some unaccountable departure from the *norm*, or regular gradual increment and decrement, of the pulse. In fatal cases, where the death is from *pure* typhus, the pulse usually becomes more and more rapid, and at the same time weaker and smaller, up to the very hour of death. In complicated cases, where the complication is not very dangerous, the pulse may be merely quickened for a day or two, or the crisis may be protracted, or the pulse may begin to rise again, after a quite distinct critical subsidence. In very dangerous complications the pulse is out of all bounds, and no rule can be assigned for its irregularities. But in regular normal cases, being uncomplicated—*i. e.*, not having any positive organic or inflammatory complication,—the decline of the pulse, even by a few beats, before, at, or about the twelfth day, carries as an after consequence the decline of the fever; and, on the other hand, a delay of the crisis very much beyond this is to be regarded with suspicion, though not by any means necessarily of fatal tendency.

But the most curious inference which I have been able to draw from the cases of typhus which we have been observing for the last two months is this: —If, in a particular case, you observe that the pulse keeps steadily to the *norm* or law of uncomplicated typhus, as I have endeavoured to exhibit it; if the rate of the pulse increases steadily and gradually up to the ninth, tenth, eleventh, or twelfth day, and then, at any period between the eleventh and fourteenth day, is observed to be undergoing a correspondingly regular decrease, not rapid, but regular and gradual; *in such a case of regular crisis indicated by the pulse-rate, it is almost insignificant, as regards the prognosis, that the other symptoms continue formidable-looking for days;* for in all such cases that I have observed, without exception, the patient has got well in the end, and that without further treatment. Thus I can tell you from experience that, after a regular crisis of this kind, the tongue may be as black as pitch, and so dry that it feels like a bit of roasted leather, delirium may go on, the patient may be out of his bed many times in a night, the skin may be hot and dry, the eruption brilliant or livid for days together; still, if you have the favourable state of the pulse, and if there is no complication, *the patient will get well.* The pulse is your criterion. Whenever it is fairly turned, and declining in rate, especially if it is at the same time gaining in strengh and volume, you may be at your ease, unless there is reason to suspect a complication; and even then you may be at your ease, if the complication is not severe. But observe, *only if you do not interfere.* The proper course in such a case is to take confidence from the pulse, and avoid interference. *You can afford to wait.* And only one more word to-day, but it is a very startling one, and yet true according to my experience: even if the delirium should become worse after the crisis, as indicated by the pulse; nay, more, even if the patient has had little or no delirium before, and *begins* to be delirious for the first time after the pulse has begun to show symptoms of crisis; still, it is better to wait, for in all probability he will be quite well in a few days, provided you do not disturb the course of the fever.—*The Lancet.*

1. ON THE HYPNOTIC PROPERTIES OF THE BROMIDE OF POTASSIUM. BY DR DEBOUT.

(Bulletin Général de Thérapeutique, Aug. 15, 1864.)

2. ON THE ACTION OF THE BROMIDE OF POTASSIUM.

(L'Union Médicale, Oct. 13, 1864.)

1. Dr Debout relates several cases in which great irritability was relieved by means of the bromide of potassium. In one case there was a stricture of the urethra, which resisted the treatment by dilatation, because the irritability and febrile excitement were so great as to prohibit the introduction of the catheter with sufficient frequency and regularity. Dr Debout was therefore induced to employ the bromide, in order, if possible, to cause anæsthesia of the urethral canal, so as to make it insensible to the injurious effects of the catheters. The salt was given in large doses, four grammes (about ʒj.) being taken every day. The success of the treatment exceeded all expectation, for as soon as the bromide had been taken, the catheters could be introduced, of greater and greater dimensions, without producing any bad symptoms. The most remarkable circumstance in the case was the stupifying action produced by the bromide, for although the patient had been unable to sleep for a month previous to the treatment, yet as soon as it was administered, and when he had taken only about half a drachm of the salt, he slept all night. Another case was one of neuralgia of the neck of the bladder, preventing the patient, who was a medical man, from riding on horseback and visiting his patients, the nature of the roads preventing him from walking. After a month's trial he lost the painful sensations of which he complained, and resumed his usual avocations; he stated also that he had proved the hypnotic properties of the bromide. In another case, the bromide was used to allay an erethism of the pharynx and the neck of the bladder, and the patient was obliged to leave it off, because fifteen grains dissolved in water not only procured him sound sleep during the night, but on the next day he remained in such a state of somnolence as to be unable to attend to his business.

2. In the article in the "Union Médicale," the therapeutical properties of the

bromide of potassium are described at considerable length, and the results obtained both in England and France are recorded.

Considered at first as a medicine acting specifically in diphtheria and photophobia, and as an anaphrodisiac and narcotic, it has now become recognised as a powerful general and local sedative of the nervous system. M. Gubler, after successfully employing it, from its elective action on the mucous membrane of the fauces, in the granular or glandular sore throat of phthisical patients, has extended its use to all the painful affections of this region, independent of tuberculosis. For instance, dysphagia and herpes of the throat have been relieved by its use, as well as the spasmodic cough of hysteria, phthisis, etc. It would probably also be efficacious in hooping-cough, like bromide of ammonium. It might, perhaps, be used as a gargle in these cases, if its contact were prolonged. This local action has been made available also in moderating the irritability and excitement of this region in cases of laryngoscopic examination and in operations on the sensitive organs of speech. A solution of the salt, in the dose of 10 grammes (about 2½ drachms) to 150 of distilled water, a tablespoonful night and morning, was employed by M. Gubler in a case of relapsing chorea, and produced a well-marked improvement in two days; and in another case of the same disease the effect was equally beneficial. The same physician has also employed the remedy with success in affections characterized either by congestion of the brain, or the excito-motory power of the spinal cord, in which the usual remedies, as arsenic, belladonna, or atropine, had failed. One patient had attacks of congestion in the head, vertigo, and palpitations, connected with prolonged intellectual fatigue; in a second case, these symptoms coincided with a general excitement and priapism; in a third, the patient was affected with general nervousness and diabetes; and all three were relieved in a remarkable manner. A case of spinal paralysis, with tetanic contractions, was also improved.

The writer in the "Union Médicale" does not admit that the bromide of potassium is a true hypnotic, but considers that it causes sleep by the deep sedative effect it produces upon the whole of the nervous system, and secondarily on the circulation. Phthisical patients, tormented by coughing and sweating, notwithstanding the use of opium, have obtained sleep by the means alluded to; and in two cases of organic disease of the heart, with rapidity and intermission of the pulse, precordial anxiety and dyspnœa, M. Gubler was able, by means of the bromide, to reduce the pulse from 108 to 76, and to relieve the dyspnœa.

The properties of this salt may be summed up by stating that it is a simple moderator of nervous excitement, and that its elective action is specially exerted on the internal integument, the mucous membrane of the isthmus of the throat, that of the pharynx, and that of the genital passages. M. Vigouroux considers it the remedy *par excellence* for the nervous complaints which are so common in large cities, and the cause of which appears to him to be the excess of vascularity of a part of the nervous centres. The bromide, by diminishing the vascularity, becomes the antidote to this condition. Its presence in certain mineral springs explains their sedative action, and these springs may be usefully employed now that the cause of their action is known. Its operation appears to be antagonistic to that of iodine, with which it ought not, therefore, to be therapeutically associated, unless it is intended to counteract the effects of the latter.—*British and Foreign Medico-Chirurgical Review.*

THE BLISTER TREATMENT OF ACUTE RHEUMATISM.

THE profession will welcome any new and reasonable therapeutical means in the treatment of a disease so common and so dire in its effects as acute rheumatism. Few diseases add so largely and in so many ways to the mass of human suffering. Although it rarely kills, it frequently incapacitates the patient for active life, and, besides, it is the parent of many other definite diseases—as chorea and paralysis. In itself, or so far as the patient knows, it is a mere question of a few weeks' severe pain. Although in great distress and helpless, he is nearly certain to get through it. And at one time, before its connexion with heart affection was known, it was not thought by medical men to be a very serious disease. No treatment, however, can be worthy of the name which does not tend to keep off cardiac complication. To do this ought to be the great object of all treatment, and any plan that falls short of it, however much it may relieve the patient's suffering, is a failure. In spite of all that has been done in the treatment of this disease, we are continually meeting with cases of heart disease in the out-patients' department of our large hospitals.

Now, most physicians are agreed that one important way of preventing cardiac complication is to cure our patient quickly. To do this many plans have been urged, and the most recent is that brought forward by Dr Davies—viz., by free blistering. We think that by far the most important statement in his paper is—"In no case when the heart was sound at the time of admission did any organic lesion develop itself." We may just add, however, that there are few diseases about which there are more flatly contradictory opinions as to the results of any particular method of treatment than acute rheumatism. Thus the alkaline treatment is much relied on by many physicians, but at Guy's Hospital we see cases treated by Dr Gull by a placebo, *e.g.*, a little extract of taraxacum and water. This physician avers that this no-treatment, or rather no-drug treatment, is quite as good as the alkaline method. He speaks from experience, as he has carefully tried both. The patient gets well, he believes, as soon, and the heart is as seldom attacked under one as under the other. Dr Davies' plan seems to us to be a medium betwixt drug treatment and what we may call treatment by time. Many cases have been treated in the London Hospital since Dr Davies' paper appeared, and we now give the following example:—

The most striking feature in Dr Davies' method of treatment is, that it is absolutely and entirely local. In the cases he relates, no drugs, except an occasional purgative, were given. Wherever a joint was inflamed, there a blister was put on. The blisters are to be applied quite round the affected limb; not on, but near to, the inflamed joint. When a large joint like the knee is affected, the blisters should be two inches wide. They should be put on, he says, at "the very height of the inflammatory stage, when the local pains are the most severe and the constitutional disturbance the greatest." Poultices should be afterwards applied to prevent flow of serum. In one case nearly 300 square inches of blisters were applied. It may be thought that, as regards pain, the remedy is worse than the disease, but the contrary is the fact. The patients like the blister treatment, and it has never caused strangury in any of Dr Davies' cases, except to a trifling extent in one case.

This seems at first sight a strange way of treating a blood disease like acute rheumatism, but, as Dr Davies shows, under this treatment the urine will become neutral or even alkaline. So, then, the treatment, although local, produces that condition of the urine which is generally aimed at in the common treatment by alkalies. Dr Davies uses local means, as he thinks the poison locates itself in the inflamed joints. He believes, indeed, that the blood is not loaded with the *materies morbi*, but that it is deposited in certain tissues which have temporarily an affinity for it. Instead, then, of giving alkalies to *neutralize* the "acid poison," he thinks we should try to *eliminate* it by blisters applied near the points where it is then accumulated. Whether Dr Davies' theory of the action of his remedy be correct or not, the practice seems to have been very successful, and is deserving of careful trial in our hospitals. It is purely a question of fact. Does the blister treatment shorten the duration of acute rheumatism and prevent heart complication? If further experience shows that it does accomplish these two important objects, Dr Davies may be congratulated on having increased the powers of usefulness of our profession in no small degree.

The following case is supplied to us by Mr J. E. Adams, Assistant-Resident Medical Officer to the hospital:—

Ann S., aged 25, married, was admitted November 30, for acute rheumatism.

It had begun eight days before, and was her first attack. Her health had usually been good, but lately she had been weak—*i. e.*, since her confinement three months before.

When admitted she had all the constitutional symptoms well marked. Her tongue was furred, there was great thirst, complete anorexia, she could not sleep, her pulse was 108, and temperature 100°; secretions acid. There was intense pain in both shoulders, both hips, in the left wrist, hand, knee, and ankle, and they were hot and swollen. There was also a very soft systolic murmur audible at the apex of the heart.

Blisters were at once applied to all the affected joints, ten in number; and no medicine was given beyond an opiate at night.

The next day there was very little pain in the blistered joints, but the hand was now affected. The constitutional symptoms were about the same, but there was slight strangury. Two blisters were ordered for the right hand.

The next day there were no rheumatic pains, the pulse was diminished in frequency, tongue cleaning, but there was little sleep, and the appetite had not returned. The sweat was still slightly acid; urine and saliva neutral; strangury very slight.

Two days later the pulse was reduced to 80; temperature 99°. She had passed a comfortable night, and the appetite was returning.

From this time she improved steadily (having only once a little pain in the left elbow, but no constitutional disturbance), and left the hospital on December 20, perfectly well, the condition of the heart being the same as on admission.—*Medical Times and Gazette.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLIV.—MEETING III.

18*th January* 1865.—Dr HANDYSIDE, *Vice-President* of the Society, in the Chair.

I. DEATH FROM RUPTURE OF A VARICOSE VEIN.

Dr Alexander Simpson read a paper on this subject, which will be found at p. 720 of the present number of this Journal.

Dr Gairdner thoroughly concurred with the last remark in the paper as to the great importance of its being generally known that hæmorrhage from a varicose vein could readily be stopped. He remembered being called for about thirty years ago to go to an apothecary's shop, where an old man was said to be dying from loss of blood. When he arrived he found the patient stretched on the floor, very pale, unconscious, but still breathing. The lads in the shop were staring at the man, but were doing nothing. Dr Gairdner had the man's trousers, drawers, stockings, and a bandage, removed; between each was a layer of blood. A varicose vein had burst, but the hæmorrhage had been arrested by the syncope. A compress and bandage were applied; the man soon recovered consciousness and was sent home. This case was of no great importance in itself, but it showed how necessary it was that the public should have some knowledge of the steps to be taken in such cases.

Dr Andrew Wood, when called in in such cases, had never seen anybody try to stop the bleeding; on the contrary, the great object of the bystanders appeared to be to keep up the flow of blood by washing away the clots and so on. Formerly, when leeches were more employed than at present, many children lost their lives.

Dr P. H. Watson remarked that, considering how common varicose veins were in the lower extremities of working people, and how often they were associated with ulceration, the wonder was that hæmorrhage did not occur oftener than it did. Probably the inflammatory process produced a change in the coats of the veins, so that as ulceration extended the vessels became occluded. Where bleeding took place, it was generally found that it had not been preceded by inflammation. These cases were of such common occurrence in large hospitals, that surgeons did not generally think of publishing them. All surgical writers, however, alluded to the subject, and stated the treatment to be employed. The reason why the hæmorrhage proved fatal was, 1*st*, The thickening of the venous tissues, so that the vessel remained patulous. 2*d*, The thickening of the cellular tissue. 3*d*, The valves of the vein were insufficient in consequence of the dilation of the vessel; so that bleeding occurred both from the distal and cardiac side. Dr Watson remarked that, when a dresser in the Infirmary, a fatal case of this kind occurred. A man was brought in moribund. His trousers were soaked and one boot was full of blood. Transfusion of blood was on the point of being practised when the patient died.

II. CASE OF FOREIGN BODY IN THE ŒSOPHAGUS OF AN IDIOT.

Dr Stevenson, on the part of *Dr Crichton Browne*, read a paper on this subject, which will appear in an early number of this Journal.

III. DIPHTHERIAL PARALYSIS.

Dr Joseph Bell read notes of a case of paralysis following diphtheria, which will be published in an early number of this Journal.

Dr Argyll Robertson, on the occasion of Dr Begbie's reading his paper on diphtheria, had alluded to the ophthalmic phenomena which sometimes followed that disease, and had stated that they might be accounted for by paralysis of the ciliary muscle. The case just narrated was peculiar, as the paralysis was certainly not of the ciliary muscle, but probably of the superior oblique, the action of which was to draw the eyeball downwards and outwards. Dr Robertson had seen several cases of paralysis of the ciliary muscle, some after diphtheria, some not; in these the Calabar bean had done good. In Dr Bell's case, however, it would probably not have proved so useful.

IV. ON CLIMACTERIC INSANITY.

Dr Francis Skae read a paper on this subject, which is published at page 703 of this number of the Journal.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXIV.—MEETING I.

23*d November* 1864.—Dr PATTISON, *Vice-President*, in the Chair.

I. A CRITICAL INQUIRY ON SUPERFŒTATION.

Dr Bonnar, Cupar, read a paper on the above subject, which will be found at p. 582 of the January number of this Journal.

Dr Keiller remarked that Dr Bonnar had bestowed great labour on the subject in preparing this paper, which was one of great interest. He had started in it quite a new question, viz., How soon after delivery could a woman again become pregnant? He (Dr K.) must confess, that although he had lectured on medical jurisprudence and midwifery for a number of years, the question had never occurred to him till put, a short time ago, in a letter he had received from Dr Bonnar. He thought it must greatly depend upon the rapidity of the disappearance of the discharges. Some women have the lochia for a very short time only. Dr Sidey had informed him that he (Dr S.) had attended one woman in a second delivery exactly nine months after delivery of the previous child—both children being at the full time. Ten months, he believed, was very common. In the lower animals we had frequently cases of a female becoming pregnant immediately after delivery. Breeders of horses believe that a mare is most susceptible of impregnation nine days after foaling. With regard to superfœtation, he thought it was quite possible, though the subject was a puzzling one. Dr Thatcher used to refer all such cases to a double uterus, and thus got over the difficulty. But such cases were exceedingly rare. Cases of superfœtation, however, were also rare. Superconception, he thought, was very common. No doubt, some cases of twins were due to this cause.

Dr Moir remarked, that the physiological difficulty was still to be got over. Conception in some of the cases must have taken place when the whole of the interior of the uterus was lined by the decidua. How can conception then take place? Some cases, however, were very puzzling to explain on other grounds—as in the case where there was only 127 days between two viable children. The ready supposition of double uterus was removed in the case where, by a post-mortem examination, it was proved not to exist.

Dr Cappie suggested, that if conception occurred within fourteen days after delivery, the congested state of the uterus might enable the child to grow more rapidly.

A cordial vote of thanks was awarded to Dr Bonnar for his very interesting communication.

II. FURTHER HISTORY OF A CASE OF PELVIC TUMOUR OBSTRUCTING DELIVERY. BY DR BURTON, WALSALL.

The *Secretary*, in reading the following notes of the case, said the Society would remember a former communication by Dr Burton on this case. These were the further notes of the case since that period:—

"Tumour (ovarian?) in the recto-vaginal space. Induced labour, by puncturing the amnion. Labour in fourteen hours. Breech presentation. Delivery aided by blunt hook.

"Mrs H.—her fifth pregnancy; the last labour having been induced on 13th July 1863, by puncturing the amnion at $7\frac{10}{30}$ months, and delivery effected with the forceps,—the child being saved and reared. Was again attended by me on 30th September last, being computed as 223d day of her pregnancy.

"*Examined per vaginam.*—A large soft tumour in the recto-vaginal space bulged forwards, filling the vagina, from the bottom of the sacrum towards its promontory, as far as could be reached by the finger, and pressing forward at its upper part to within three-quarters of an inch of the symphysis pubis. In its middle part it was rounded so as to leave two distinct sulci on each side, over the sacro-iliac junction. The tumour seemed to be fixed when an attempt was made to push it upwards; but by pressing it downwards it seemed to descend a little. One or two hard nodules, about the size of filberts, could be felt on its middle surface, and a soft place, about the size of a walnut, at its upper part, gave to the finger a sense of fluctuation.

"The os uteri could not be felt, but the finger reached a part higher than the os, which seemed to indicate that the os was pushed forward upon the pubes. By introducing the hand into the vagina (minus the thumb) the os was with difficulty reached in that situation, and being patulous to about half an inch in diameter, a silver canula was with some little trouble, but without pain to the patient, introduced freely into the uterine cavity. The stilette having been passed, the membranes were punctured, and a very gratifying discharge of liquor amnii announced the success of our proceedings. There was no blood stain, nor did the patient complain of any pain, except when the sides of the tumour were inadvertently pressed upon. The abdomen became flaccid, from the evacuation of the liquor amnii, and through its parietes the fœtus could be very distinctly made out. The head lay to the left of the umbilicus, and this position it retained, notwithstanding very severe ballottement downwards and from side to side. The canula, when in the uterus, impinged upon a soft body, and I concluded that there was a breech presentation. The operation was completed at 11 A.M.

"1*st October*.—The patient has experienced a free drainage of the liquor amnii, and occasional slight uterine contractions since 7 A.M. Has kept in bed, and 'feels quite well.' Bowels had been freely opened by castor-oil taken yesterday. Tumour to be felt *in statu quo*, but no os uteri to be felt with the finger.

"4 P.M.—Was summoned, and found that active labour had set in at 1 P.M. The os was soft, and dilated to about two inches diameter. The tumour, during a pain, was pushed rather freely downwards, so that I could reach its upper part. The infant breech presented with the right ilium on the pubis at its symphisis. Labour progressed steadily, and in an hour, by partially introducing the hand into the vagina, I could reach the right groin with the finger. I inserted the blunt hook over the right thigh, and by gentle traction I succeeded during a pain in steering the breech (if I may use the expression) fairly into the pelvic brim. I then removed the hook, and with the finger brought down the legs. I administered a dose of a solution of Bonjean's ergotine: a good pain soon followed, expelling the breech with a little assistance; a second enabled me to hook down the arms; and two more pains, aided by a considerable traction, expelled the head,—the tumour having descended before it until it fairly cleared the vaginal orifice, being folded backwards on the perinæum. As soon as the head escaped, the tumour resiliated into the vagina, and before I could introduce the finger, it had resumed its original position, and was painless upon gentle pressure, although the patient, at the time of delivery, and for two hours afterwards, complained of much uneasiness in perinæum and lower part of the rectum. The fœtus was born inanimate and flaccid; but as there was slight pulsation to be felt in the cord, I was encouraged to attempt restorative means, which proved successful, and in half an hour the child became lively and cried well, although it was feeble, and unable to swallow. The placenta was thrown off by the natural efforts in about fifteen minutes after the birth of the child, unaccompanied with any more, if so much, as the usual hæmorrhage; and at 6.30 P.M. I left my patient tolerably comfortable in all respects.

"2*d*.—Has passed a good night, and feels comfortable. No pain in the vagina or in the tumour, but a little in the abdomen, above the pubes; lochia natural. The infant lived six hours, being unable to swallow food.

"Mrs H. informs me that she has continued to suckle her last infant, born on 13th July 1863, up to within a week, and proposes to apply it to her breast, as milk is secreted. To my admonition against her so doing she replies that she is resolved to try it, unless she finds it hurtful to her baby.

"5*th*.—Has suckled her child regularly and has had no trouble but after-pains, accompanied with slight rigour and headache. The bowels having moved naturally, she declines taking castor-oil; but was ordered calomel gr. iij., pulv. antimonialis gr. v.—M.

"13*th*.—Has progressed favourably until yesterday, when she was imprudently, and against my advice, carried downstairs, thereby inducing abdominal pain, and renewed lochial discharge, with a small clot, and active diarrhœa. Ordered to be in bed and an opiate administered.

"*November*.—Everything has since gone on prosperously. She is now about, active and well, and suckles her child."

III. NOTES OF A TWIN CASE.

The *Secretary* read the following notes of a twin case which occurred the previous week in the Royal Maternity Hospital, drawn up by *Mr Taylor*, house-surgeon:—

"The patient K. F. is married, and has had one child, which is now alive, and will be three years old next January. Her menstruations have never been regular, but the period of the last was the end of April.

“The period of quickening, middle of August.
„ duration of 1st stage of labour, 8 hours.
„ „ 2d „ „ 20 minutes.
„ „ 3d „ „ 3 „

“The head of first and larger child presented in first position, and simultaneously with birth of head was presented the unbroken caul of second child.

“On being born, the first child showed very feeble signs of vitality, and that only by sluggishly contracting one or more of its limbs. The stupor was unaffected by the copious administration of cold water.

“Meantime the second and smaller child was being expelled—footling presentation. It exhibited far greater signs of vigour, and was more sensitive to administration of cold water. The cords were both pulsating strongly, and at end of three minutes the placenta was withdrawn.

“Although satisfied that neither of the children could be fully resuscitated, I determined to see how long life could be maintained, and for this purpose immersed both children in warm water (temperature 100° F.), and during the first twenty minutes occasionally plunged them into cold water, immediately replacing them in the warm. During these operations the smaller child gave several violent spasmodic heaves, but never seemed to breathe.

“At the end of twenty minutes we found that both cords were still feebly beating; and by way of experiment we endeavoured to restore the natural heat by immersing the placenta into a separate basin of warm water (temperature 110° F.) The pulsations then became more distinct and frequent, and did not cease until the end of forty-three minutes after birth.

“At that time we found that the systolic action was still distinctly visible in the chest of smaller child. It was also synchronous with that of the larger child, as ascertained by auscultation. Looking, therefore, at the one, and listening to the other, we found the pulsations to be as follow:—

“At end of 44 minutes,		39 beats.	
„ 50 „		36 „	
„ 55 „		34 „	
„ 60 „		30 „	
(Placenta removed from warm water.)			
„ 65 minutes,		28 „	
„ 70 „		26 „	
(Children dried and wrapped in hot flannels.)			
„ 73 minutes,		15 „	
„ 75 „	smaller child,	... „	
	larger child,	12 „	feebly.
„ 80 „		... „	

“The mother fell down five steps of a stair on the morning of the day she was confined, and immediately thereafter the pains began.”

SESSION XXIV.—MEETING II.

21*st December* 1864.—Dr PATTISON, *Vice-President*, in the Chair.

I. CASES OF HYDRAMNIOS.

Dr Alexander Simpson made some observations on dropsy of the ovum. (To appear as a separate article in a future number of the Journal.)

Dr Andrew Inglis stated he had lately had a case of an anencephalous fœtus associated with hydramnios, very similar to the second case of Dr Alex. Simpson. He (Dr I.) ruptured the membranes, which were very tough, and shortly after turned the child on account of severe hæmorrhage setting in. The presentation he felt was peculiar and rather puzzling. The child was at the full time.

A strange circumstance was, that his patient had also been up in the North in Sutherlandshire, and had been subjected to some rough riding in a cart.

Dr Archibald had had two or three cases of a similar kind, and thought that a large quantity of liquor amnii was general in such cases.

Dr Keiller remarked, that he had shown several specimens of anencephalic children to the Society. Some of them had lived for a short time. With regard to the difficulty of at first diagnosing the presentation, he was in the habit of mentioning a case of the kind where the house-surgeon of the Maternity at the time came to him much puzzled as to the nature of the presenting part. His expression was, that there was "something smashed-like about the head," and he was afraid that the child had suffered some injury from a kick the woman had received a short time before. He (Dr K.) thought that the description of the presenting part was a good one. Beautiful dissections of the nerves at the base of the brain in some of these cases can be made by simply shaking the head well in water, and thus washing the soft brain substance away.

II. CASE OF KEPHALOTRIPSY.

The *Secretary* showed the preparation from this case, and read the following notes supplied by *Dr Balfour* of Portobello :—

"Mrs C., aged 37 years, has had seven children, three of whom are alive. Her first four children were born after a few hours' labour, and they were so easy that she always employed a midwife. She came to Portobello in 1861, and engaged a midwife to attend her in her fifth confinement, which happened in October. As no advance was made after many hours' labour, I was sent for, and found the promontory of the sacrum pressing inwards and to the left, which left a considerable space to the right, through which the head was pressing. Finding the head to be small, and thinking that it might pass by use of the forceps, I applied them, and, after considerable traction, I delivered her of a living child. She made a good recovery. I advised her, however, should she fall in that way again, to allow me to bring it on at the eighth month, as a safety to herself and the child. She agreed to this, and about the beginning of February 1863 she called me to attend her, as she was eight months gone. I induced premature labour ; and on the 15th, labour having set in, I found the parts still more contracted. I sent for Dr Moir, who thought that the child might be brought home by turning, which was attempted, and with success. He agreed with me, that should she fall in the family way again, it ought to be brought away on the seventh month ; and I told her so.

"On being called to attend one of her children in July, I found her in that way again, and spoke to her about getting it brought on prematurely, to which she would not on any account consent, assigning as her reason that her last had died four or five months after she was born, and she thought it was because she had been born too soon,—though the real reason was, she never called any one in to see and prescribe for the child. Though she admitted this, yet she said she would 'lippen to Providence' this time, and would not allow anything to be done.

"On the 28th September labour came on, and having sent for my brother, we did everything we could to try and turn, but unsuccessfully. I therefore thought it best to send for Professor Simpson, as, from the size of the child, and narrowing of the brim, I was persuaded she could not be delivered without the kephalotribe."

Dr Alexander R. Simpson added, that when Professor Simpson, with whom he also went to see the case, arrived, the state of matters was just what would be guessed at from Dr Balfour's hurried notes. The pelvic brim measured barely three inches in its conjugate diameter, and was occupied by a full-sized fœtal head, which the uterine contractions utterly failed to force into the narrow opening. Perforation was easily effected, and the kephalotribe applied. The head was then extracted without much difficulty; but it demanded the exercise of a great degree of force to pull the shoulders through the brim. The flattened head with the blades of the instrument was found to measure little more than two inches and a half in its transverse diameter.

In answer to a question by Dr Archibald, *Dr Alexander Simpson* stated that the difficulty in turning in this case was owing to the membranes having been so long ruptured, and the uterus firmly contracted on the child.

Dr Keiller had seen a case the night before, where there was a contracted brim, the waters had escaped for some time, and the uterus was firmly contracted, but where he succeeded in turning, and thus delivering the woman. At first he was afraid it would have been a case for the kephalotribe. At an early part of the labour there had been the complication of the prolapse of the cord, but it was readily reduced and kept in its place by Dr Burn, who was in attendance, by placing the woman on her elbows and knees. Though the cord was quickly replaced in this manner, in the process of turning he felt that the pulsation of the cord had ceased, and the child was still-born. The woman had had children before, but had never required instrumental aid. The promontory of the sacrum projected far forwards.

III. NOTES ON THE HISTORY OF ANCIENT GREEK MIDWIFERY.

BY DR ROBERT FINLAY.

The paper will appear in a future number of this Journal.

Dr Pattison said he had no remarks to make on this able and interesting paper, but thought the cordial thanks of the Society were due to Dr Finlay for favouring them with such a paper.

IV. ON RETAINED PLACENTA IN ABORTION.

Dr Pattison stated that he had lately attended in a case of abortion, where the child was expelled without the placenta, and though he had carefully watched there had been no appearance of the after-birth. Hæmorrhage occurred on the seventh day, for which Dr Pattison had to plug. It was now fourteen days since the abortion occurred; the patient had a very fœtid discharge; he had dilated the os and examined the uterus in company with Dr Keiller, but they were unable to find any placenta. He was at present employing injections of permanganate of potass. He brought the case before the Society for the purpose of eliciting their views of treatment in such cases.

Dr Keiller had seen the case referred to by Dr Pattison. He examined the patient, but was able to bring away a few shreds only. The placenta was no doubt in a putrid condition, and the only thing that could be done was to wash out the uterus. It is the opinion of some medical men, especially country practitioners, that you do not need to interfere in such cases of retained placenta. He one time removed the after-birth in such a case in the country, and on his way home meeting a medical man, he mentioned the case to him. He was quite surprised at the interference, and stated that he had never seen a case go wrong where left to itself. He (Dr K.) was glad this subject had come up before the Society, because in a paper lately published in the Journal by Dr Lawrence of Montrose, the author states that he is surprised that this Society held the opinion that no interference was necessary in such cases. Such was not the case, and he had no doubt the members would agree with him that no such case should be left to itself.

Dr Moir stated that he never left such cases without removing the placenta. He considered it dangerous to allow it to remain. He had seen severe hæmorrhage follow where only a small portion was left behind. He believed that in some cases where the placenta remained adherent, it might continue to grow and not become putrid. Where entirely separated, it may remain in the uterus for a time and be afterwards expelled; but if part remains adherent it becomes putrid.

Dr Archibald had had several cases where the placenta had not been expelled for some days.

Dr Gordon stated that he had a case where the placenta was left behind. Severe putrid fever set in, and it was necessary to dilate the os and remove the after-birth.

GENERAL COUNCIL OF MEDICAL EDUCATION AND REGISTRATION.

SCOTTISH BRANCH.

PHYSICIANS' HALL, EDINBURGH, *January* 13, 1865.

Sederunt.—Mr Syme, *Chairman.* Dr Fleming, Dr Alexander Wood, Dr Allen Thomson, Dr Christison, Dr Andrew Wood,—Dr William Robertson, *Registrar.*

1. The minutes of last meeting were read and confirmed.

2. The subject of the proposed amendments of the Medical Act was resumed, and the following resolutions were agreed to:—

"The Branch Council for Scotland having, in terms of the remit made to them by the General Medical Council, considered carefully the subject of the amendment of 'The Medical Acts,' particularly with reference to Clauses xx., xxxi., and xl. of 'The Medical Act, 1858,' 21 & 22 Vict., cap. 90,—Resolve, that any Act for amending the Medical Act should be restricted to specific clauses, whereby the following objects may be carried out:—(1.) To confer on the Medical Council definite powers to issue to the various licensing bodies regulations on the subjects of preliminary and professional education and examination. (2.) To amend Clause xl., so as to render it more efficient than it has hitherto been, for enabling the public to distinguish between qualified and unqualified practitioners, and for preventing unqualified practitioners from assuming medical titles to which they have no right."

In order to carry out the first of these objects, the Branch Council recommend clauses to the following effect:—

"That it shall be lawful for the General Medical Council from time to time to issue to the various licensing bodies such regulations respecting the preliminary and professional education and examination of persons desirous of obtaining any of the qualifications mentioned in Schedule (A) to the 'Medical Act,' as may appear to the Council fitted to secure, on the part of such persons, the requisite knowledge and skill for the efficient practice of their profession. That all such regulations as shall have been passed by a majority of two-thirds of the General Council shall be obligatory on all universities, colleges, and other bodies enumerated in Schedule (A) to the Medical Act of 1858. That, in the event of any of the said bodies not conforming to such regulations, it shall be lawful for the General Council, if they see fit, to intimate to the said body that it has not conformed to such regulations; and to direct that, in the event of the said body not conforming within six months after such intimation, the qualification granted by such body, after the lapse of the said period of six months, shall not be registered. That it shall be lawful for any body in regard to which such direction shall have been given, to appeal to the Privy Council, who shall have power, if they see cause, to disallow the direction of the Medical Council. That it shall be lawful for the General Council to restore any right to registration which may have been suspended by them, when they shall be satisfied that their regulations have been conformed to."

In order to carry out the second of these objects, the Branch Council recommend a clause to the following effect:—

"It shall not be lawful for any person, unless registered under this Act, to practise any branch of the profession, taking or using the name or title of a Physician, Doctor of Medicine, Licentiate in Medicine or Surgery, Master in Surgery, Bachelor of Medicine, Doctor, Surgeon, Medical Practitioner, or General Practitioner, or Surgeon-Apothecary, or Licentiate or Practitioner in Midwifery, or Professor of Medicine, or Professor of Surgery, or any other medical or surgical title; and every unregistered person so offending shall, upon summary conviction for such offence, forfeit or pay a sum not exceeding twenty pounds."

It was resolved—

3. "That the Scottish Branch Council having considered the propriety of amending Clause xxxi. of the Medical Act, are of opinion that what is objec-

tionable in it would be obviated were the combination and co-operation of the licensing bodies, as provided for by Clause xix., encouraged or even made obligatory, so that facilities may be given to medical students for acquiring the complete or double qualification, without having to pass repeated examinations on the same subjects, which is at once irksome and unnecessary; and, in order that the public may be best provided with general practitioners, who have been educated and tested in every branch of the profession, as well physic as surgery."

It was resolved—

4. "That the Branch Council consider that it is highly expedient that a bill for amending the Medical Acts should be prepared and submitted to the Home Secretary within two months from the present time, so that it may be possible to have it introduced into Parliament, and passed into a law during the ensuing session of Parliament."

5. The Registrar laid on the table the registers of medical students which had been transmitted to him by the various licensing bodies in Scotland.

In laying these returns before the Branch Council, the Registrar intimated, that, from the number of blanks occurring in them, it was impossible for him to comply with the 4th regulation of the General Medical Council, and to frame an accurate register of medical students.

The Branch Council resolve to report this to the General Medical Council, as an illustration of the necessity which exists for enabling the Council to issue regulations of a compulsory character.

6. The Treasurer's annual accounts, with Auditors' report thereon, likewise the Treasurer's statement of gross receipts during the past year, were read, approved, and ordered to be printed with the minutes.

Account, Charge and Discharge, of Treasurer's Intromissions with the Funds of the Scottish Branch of General Medical Council, from January 5, 1864, *to January* 5, 1865.

CHARGE.			
In Consols, as formerly stated,	£1907	10	0
In bank on January 5, 1864, at credit of Scottish Branch Council, ...	1032	8	4
7 Registration fees at £2, . .	14	0	0
162 do. do. at £5, . .	810	0	0
14 additional do. at 5s. . .	3	10	0
Dividend, January 1864, on £2000 consols, ...	22	2	6
Dividend, July 1864, on do., .	29	5	0
Interest allowed by Bank of Scotland on account of 1863,	19	11	2
Sum of charge, . . .	£3845	7	0

DISCHARGE.			
Balance, cash due to Registrar, as per account of Jan 5, 1864,	£6	2	0
Postages and receipt-stamps, .	3	3	0
Messengers and petty expenses,	0	11	2
Fees paid in error by, and returned to, Dr M'Innes, . .	4	15	0
Additional fee paid by Dr M'Innes, and transferred to English Branch Council, .	0	5	0
Stationery (Waterston), . . .	0	17	6
Printing (Constable), - . . .	7	0	0
Fees and travelling expenses of Branch Councillors to Jan. 5, 1865, ...	50	17	0
Registrar's salary and office rent to Nov. 11, 1864, ...	210	0	0
Remitted, on account of General Council, with bank commission, ...	570	14	8
In Consols, as formerly stated,	1907	10	0
In bank on Jan. 5, 1865, at credit of Scottish Branch Council,	1040	13	10
Balance, being cash in Registrar's hands on Jan. 5, 1865,	33	17	10
Sum of discharge, . .	£3845	7	0

Edinburgh, January 11, 1865.—The undersigned have this day examined the above account, and find it accurately stated and duly vouched. There is a balance of Thirty-three pounds, seventeen shillings, and tenpence in the Registrar's hands.

ANDREW WOOD, } *Auditors.*
ALEX. WOOD, }

Statement by Treasurers of Scottish Branch of General Medical Council of Gross Receipts, from January 5, 1864, *to January* 5, 1865.

7 Registration fees at £2,	£14	0	0
162 do. do. at £5,	810	0	0
14 additional fees at 5s.,	3	10	0
Dividend, January 1864, on £2000 consols,	29	2	6
Do., July 1864, on do.,	29	5	0
Interest allowed by Bank of Scotland on account of 1863,	19	11	2
Gross receipts of Scottish Branch Council,	£905	8	8

JAMES SYME, } *Treasurers.*
R. CHRISTISON, }

THE WEATHER OF MAY IN ITS RELATION TO HEALTH.

AT a meeting of the Scottish Meteorological Society, held on the 15th of January, Dr Arthur Mitchell read a paper on the above subject. He said:—Dr Jackson remarks that our language teems with medico-meteorological apophthegms. Among these we have a Scotch one which refers to May. Proverbial wisdom sends us this warning, "Ne'er cast a cloot till May be oot." Similar cautions abound in other tongues. With a heavy kind of wit we are counselled by the Germans, "To stick to our winter things till the 40th May." It is the same idea which underlies this advice, "Who has a good log, let him keep it for May." Even the people of Venice and Milan, in more southern latitudes, have a like proverb casting a like slur on merry May times. It has a little more fancy in it, and runs thus: "May, lovely May, to thee thy roses, but to me my furs." It would appear from these proverbs that there is abroad in the people's mind a suspicion of May weather,—a feeling that it is not what it seems to be, and that it should not be trusted. Let us see to what extent this is well founded. Everywhere in Scotland winter passes into summer through the months of May and June, and back again into winter through October and November. In the rise of temperature from winter to summer the great strides take place in May and June, and in the progress back the great falls occur in October and November. May is a warmer month than April by 7°, and June than May by 6½°. Except in October and November, no such leaps as these are made from month to month. The intervening spaces of winter and summer are comparatively equable. These four months, therefore, may be called transition months. They are the steps by which we reach and leave the warm periods of our year. This great stride of temperature which occurs in May depends on the rapid rate of increase of the power of the sun's rays at this period of the year, and the same influence is stretched into June. It depends on this solely, and not on any greater prevalence of warm winds. Indeed the very opposite of this holds, for the rise of temperature takes place in spite of the fact that at no season of the year are cold winds more frequent or steady. Taking the year as a whole, we have twice as much wind from the S.W. as we have from the N.E., but their proportions vary greatly in the different seasons. In March, April, May, and June, we have the wind nearly as often from the cold N.E. as from the S.W. It is quite otherwise both in our months of great heat and of great cold, for then the S.W. winds have their greatest frequency. Thus it happens, that in the very dead of winter our prevailing winds blow from those very quarters from which they blow in the height of summer; while in spring and early summer, when our weather is becoming rapidly warmer and warmer, keen cold winds from polar regions stream over us for about half the time. What our winters *would* be if the south winds loved them less it would be hard to tell. What our springs *are*, because the south winds love them so little, it is my present purpose to try to show.

Rain.—I need scarcely point out that these months of northerly winds are sure to be the driest of our year. Less rain falls in them, and it falls on a smaller number of days than in any of the other months. The month of least rain for several years back was May 1859, when at many of the stations of the Society *no* rain fell, and the mean rainfall for all Scotland was less than one-third of an inch. Our S.W. winds come from equatorial regions heated by a vertical sun, and charged with the moisture they have licked up from the oceans of the southern hemisphere. They pass us on their way to arctic regions, where the last of their moisture is squeezed out of them, and cold and arid they take their way back to the tropics. Of these two great and constant currents, north and south, sometimes the one and sometimes the other is uppermost. Every such change will be accompanied by a change in the rate of rainfall. The equatorial current, at its outset loaded with moisture, in its progress north has its temperature steadily lowered, except during one or two of the hottest months, and consequently has to part with some of the moisture with which it was loaded. So it is that it is a rainy wind. The N.E. wind, dry when it begins its southward course, as it goes on gets warmer, and acquires a higher power of holding vapour in suspension; and thus it is that it is not a rainy wind,—so far otherwise, indeed, that it is not only a dry, but a drying wind; it has an actual thirst which it greedily quenches at every wet or moist surface over which it blows.

Range of Temperature.—I have already told you the general character of the mean temperature of the month of May; but we get little insight into the temperature phenomena of a month or a year from the simple study of monthly or annual means. For instance, the mean annual temperatures for all the various stations in Scotland do not differ greatly, the difference not exceeding 4½°, and if corrected for elevation, it falls as low as 1½°. It would be a great error, however, to conclude from this that there is little difference between the climates of the different stations. The same mean may come from very different figures. The temperature may be differently distributed over the year at two stations, yet their mean temperature may be nearly or exactly the same. Thus the mean temperature of Sandwick is 45°·5, and of Thirlestane 45°·1; yet there are no two places in Scotland whose climates are more opposed to each other. And so it is with months. The mean temperature of a month may not differ much from that which precedes or that which follows it, yet each may have had the temperature spread over it in a way strikingly at variance with the other two. In like manner it may be observed that the mean temperature of one month is every year a certain number of degrees above that of its precursor. But it would be quite a mistake to infer that this is the result of a steady rise of both day and night temperatures. The mean may have been brought up by higher day temperatures alone, or night temperatures alone may have risen, or there may have been unusual fluctuations in both. It is clearly not a matter of indifference in what way the rise is accounted for—out of what elements the mean flows. Though the mean of a month be high, yet if a few days of great cold occur during its course with such regularity as to be a character of its weather, or if the mean flow from excessive daily or monthly fluctuations, we may look for an influence on health differing from that which we could expect to attend the mean of the month as a whole. Now, as regards daily range of temperature, May stands apart from all other months of the year in this, that the range culminates in that month. It does so in every year, and does so for every part of Scotland. It appears that the daily fluctuations of temperature are excessive in this month, and that the great rise of temperature which occurs in May is produced more by a rise in the day than in the night temperature; so that there is less difference between the nights of April and those of May than between the days of the one and the other.

The Cold Period in May from the 8th to the 14th.—In his valuable papers on "Weekly Extreme Temperature in Scotland," Mr Buchan observes that, if we compare the lowest temperatures at the different stations for the week beginning

17th May with those of the preceding week, a remarkable rise will be found—in most cases about 6°. A similar rise, though not so great a one, takes place in the day temperature. It would appear from this that it is the rule for the week before that beginning with the 17th to be unusually cold—that is, colder than the week before or the week after. Now, it happens that Dové, the chief of living meteorologists, pointed out some years ago that a remarkable cold occurs generally in Europe about the 11th May. M. Maedlar stated in 1845 that a lowering of the temperature occurred steadily about the 12th May. M. Ch. Martins, also a meteorologist and philosopher of high repute, pointed out in 1849 that he had found, from thirty years' observations at Paris, that there are not in the month of May three consecutive days whose temperature is as low as those of the 13th, 14th, and 15th, and he adds that Brussels observations also show a regularly recurring depression of temperature coincident with that at Paris. M. Crahay, in the same year, announced that the temperature of Louvain and Maestricht exhibited the same peculiarities. Quetelet says that it appears to occur in Belgium between the 14th and 23d of the month, and that it is better marked in the north than in the south of Europe.

It was not left, however, to philosophical research to detect this feature of the weather of May. It is disclosed in our folk-lore, which is not void of wisdom, though it is so well filled with nonsense. There is a saying among the French that "in the middle of May comes the tail (or trail) of winter;" also, among the Germans, "Who shears his sheep before Servatius' Day, loves more his wool than sheep." Now, Servatius' Day falls on the 13th May, and I noticed with great interest that he, St Mamert, St Pancras, and St Boniface, whose festival days run from the 11th to the 14th, are called, in some parts of Germany and Holland, "*Frost Saints*," because they are believed to bring cold with them. From the diagrams of temperature hung round the walls, it distinctly appears that in Scotland between the 8th and 14th May the temperature is lower than it was before the 8th or will be after the 14th—that is, there is a short period of cold which reproduces itself in the second week of that month from year to year. It does not appear, however, that this week of cold is due to an increased prevalence of north-east winds.

Relations of Mortality to Temperature.—These are constant and intimate in all climates; but they are not the same in all. In some the mortality rises as the temperature rises; in others, the mortality rises as the temperature falls. Our country, to a marked degree, is in this last condition. By far the greater number of our deaths occur during winter; and for eight or nine months, mortality and temperature are related inversely to each other. It is so, indeed, in all those countries where the tendency is to die of diseases of the organs of respiration. And it is just the reverse where the tendency to death is through disease of the abdominal viscera, for there it is the hottest month, and not the coldest, that opens the mouth of the grave to its widest. In our climate during the hottest months, deaths from diseases of the respiratory organs fall, and those from diseases of the digestive organs rise. This takes place whenever the mean temperature rises to 60°. Below 55°, just as the temperature falls, the mortality increases. Thus the chances of death from certain diseases vary with the seasons, and the march of temperature through the year regulates the manner of the people's dying. This is a fact well established, but not generally and to its full extent realized. In 1856, from 26th November to 5th December, a period of ten days occurred when the temperature fell considerably. During these ten days 1844 deaths occurred; while during the ten warmer ones before and after only 1505 occurred, giving a difference of 339, or 34 daily. But, though temperature influences mortality in a much greater degree than any other element of climate, yet the general mortality does not fall in exact proportion to the rise of temperature. Thus, during April, May, and June, the temperature goes rapidly up, but the mortality does not go rapidly down. It goes down, but not rapidly down, as we should expect if temperature alone influenced the results. There must therefore be something in the weather of these three months, in addition to temperature, which influences the mortality—something inimical to health, which must neutralize the health-giving effects of sunshine and warmth. An examination of the Registrar-General's tables shows that the death-rate of these months is kept up by consumption—a disease well described as "the great, constant, inexorable destroyer of our men and women in the prime of life, being the cause of nearly half the deaths which happen between the ages of fifteen and thirty-five." March, April, May, and June are the months when this dreadful disease exacts from us its heaviest tribute, and our payments in each of these four months are nearly equally heavy. In the height of summer its demands are at their lowest, and they remain comparatively low during all the cold and snow and frosts of winter. Diseases of the brain appear also to follow the same law, though not in so marked a manner, April and May being the months of highest mortality for this class of diseases. This injurious influence of spring weather upon the nervous system is in accordance with popular belief.

We now come to the consideration of the peculiarities in the weather of May which do probably interfere with the healthful action of its greater warmth. The cold week in May does not come and go without marking its passage by an increased mortality. It is not easy to prove this, because, until the beginning of the present year we have not had for Scotland the mortality from week to week with which to institute a comparison with temperature. The weekly mortality for London, however, in connexion with its weekly temperature, shows that this cold week of May swells the mortality. In May 1862, when the depression of temperature was well marked, both the whole weekly mortality and the weekly deaths from consumption show a rise for the week following that of low temperature. Thus, there is good reason to believe that the mortality of May is increased by this peculiarity of its weather. The cold arid character of the east winds probably exercises a still greater influence on the death-rate at this time of the year. These winds being, as we have said, not only dry but drying, lick up moisture from everything they touch—from the skin and from the mucous membrane of the lungs—and thus rob the body of an enormous quantity of heat, which is absorbed and rendered latent in the conversion of the moisture into vapour. That they are injurious to human health and life is a common belief, and I think a well-founded one. That they interfere with comfort of body and contentedness of mind, he is a robust man who will not assert from his own experience. Poets *generally* have sung the praises of May as no meteorologist would have done. One recent poet thinks we should reckon the May of the poets of the old style; and of the actual May he indignantly asks—

"Is she not sprung of April's wayward race,
The sickly daughter of the unripened year,
With showers and sunshine in her fickle eyes,
With hollow smiles proclaiming treacherous peace,
With blushes harbouring in their thin disguise,
The blast that riots on the spring's increase?"

ON THE METAL INDIUM AND RECENT DISCOVERIES ON SPECTRUM ANALYSIS.

BY PROFESSOR ROSCOE.

SINCE the spring of 1862, when the speaker delivered a course of three lectures on the Spectrum Discoveries, much has been done to increase our knowledge of Spectrum Analysis, but the whole subject is still in its infancy, and the further we advance the more we find remains to be known.

No less than four new elementary bodies have already been discovered by means of Spectrum Analysis: Cæsium and Rubidium, by Bunsen; Thallium by Mr Crookes; and Indium, by Reich and Richter of Freiberg; whilst the foundations of solar chemistry, laid by Kirchhoff, have been rendered more secure by the observations of Cooke, in America; Donati, in Italy; and Miller and Huggins, in England.

The last discovered metal has been termed Indium, from the two splendid indigo-blue lines which characterize its spectrum. Through the kindness of Professor Richter, the speaker had been placed in possession of a few grains of this new metal, the spectrum of which was exhibited by the electric lamp. In its chemical relations it resembles zinc, with which it is associated in nature; the metal can be reduced before the blowpipe to a malleable bead, when it forms a soft, ductile bead, which imparts streaks to paper on rubbing, and possesses a colour lighter than that of lead, being about the same as that of tin. The metallic bead dissolves in hydrochloric acid with the evolution of hydrogen. The oxide of indium is formed as a yellow fusible incrustation when the metal is heated before the blowpipe on charcoal. Indium differs from zinc in the insolubility of the hydrated oxide in excess of both ammonia and caustic potash. This new element may be separated from all the known metals by precipitating its sulphide in alkaline solution, and by throwing down the hydrated oxide first with ammonia and then with caustic potash; and, lastly, by precipitating the iron with dilute solution of bicarbonate of sodium. The hydrated oxide of indium then remains in solution in the pure state. Indium may be readily detected when present in its pure compounds by the deep purple tint which these impart to flame. The characteristic lines are, however best seen when a small bead of indium salt is placed between two poles, from which an electric spark passes; the lines In a and In B fall respectively upon divisions 107·5, and 140 of the photographic scale of the spectroscope, when Na = 50, and Sr c = 100·5. Up to the present time, indium has been only found in the very smallest quantity, and hence the atomic weight of the metal and the composition of its salts have not yet been determined; in fact, the speaker was led to infer that Professor Richter sent him nearly all the compound of the metal remaining from the investigation of its properties, for the purpose of illustrating this discourse. It has only as yet been detected in the zinc blende of Freiberg; but it will, doubtless, soon be discovered in larger quantities, and its compounds more closely studied.

As an illustration of the application of abstract scientific principles to useful practical purposes, the speaker stated that he had lately applied spectrum analysis to the manufacture of steel by the Bessemer process. One of the great drawbacks to the successful working of Mr Bessemer's beautiful process for converting cast-iron directly into steel, has been the difficulty of determining the exact point at which the blast of air passing through the molten metal is to be stopped. The conversion of five tons of cast-iron into cast-steel usually occupies from fifteen to twenty minutes, according to the varying conditions of weather, quality of the iron, strength of the blast, etc. If the blast be continued for ten seconds after the proper point has been attained, or if it be discontinued ten seconds before that point is reached, the charge becomes either so viscid that it cannot be poured from the converting vessel into the moulds, or it contains so much carbon as to crumble under the hammer. Up to the present time the manufacturer has judged of the condition of the metal by the general appearance of the flame which issues from the mouth of the converting vessel. Long experience enables the workman thus to detect, with more or less exactitude, the point at which the blast must be cut off. It appeared to the speaker that an examination of the spectrum of this flame might render it possible to determine this point with scientific accuracy, and that thus an insight might be gained into the somewhat complicated chemical changes which occur in this conversion of cast-iron into steel. At the request of Messrs John Brown & Co., of the Atlas Works, Sheffield, the speaker investigated the subject, and succeeded in obtaining very satisfactory and interesting results. The instrument employed was an ordinary Steinheil's spectroscope, furnished with photographic scale and lamp, and provided with a convenient arrangement for directing the tube carrying the slit towards any wished-for part of the flame, and for clamping the whole instrument in the required position. By help of such an arrangement the spectrum of the flame can be most readily observed, and the changes which periodically occur can be most accurately noted.

The light which is given off by the flame in this process is most intense—indeed, a more magnificent example of combustion in oxygen cannot be imagined; and a cursory examination of the flame spectrum in its various phases reveals complicated masses of dark absorption bands and bright lines, showing that a variety of substances are present in the flame in the state of incandescent gas. By a simultaneous comparison of these lines in the flame-spectrum with the well-known spectra of certain elementary bodies, the speaker has succeeded in detecting the presence of the following substances in the Bessemer flame:—Sodium, potassium, lithium, iron, carbon, phosphorus, hydrogen, and nitrogen.

A further investigation, with an instrument of higher dispersive and magnifying powers than that employed, will doubtless add to the above list; and an accurate and prolonged study of this spectrum will probably yield very important information respecting the nature of the reactions occurring within the vessel. Already the investigation is so far advanced that the point in the condition of the metal at which it has been found necessary to stop the blast can be ascertained with precision; and thus, by the application of the principles of Spectrum Analysis, that which previously depended on the quickness of vision of a skilled eye has become a matter of exact scientific observation.

Another interesting practical application of our knowledge concerning the properties of the kind of light which certain bodies emit when heated, is the employment of the light evolved by burning magnesium-wire for photographic purposes. The spectrum of this light is exceedingly rich in violet and ultra-violet rays, due partly to the incandescent vapour of magnesium, and partly to the intensely-heated magnesia formed by the combustion. Professor Bunsen and the speaker, in 1859, determined the chemically active power possessed by this light, and compared it with that of the sun; and they suggested the application of this light for the purpose of photography. They showed that a burning surface of magnesium-wire, which, seen from a point at the sea's level, has an apparent magnitude equal to that of the sun, effects on that point the same chemical action as the sun would do if shining from a cloudless sky at a height of 9° 53′ above the horizon. On comparing the *visible* brightness of these two sources of light, it was found that the brightness of the sun's disc, as measured by the eye, is 524·7 times as great as that of burning magnesium-wire when the sun's zenith distance is 67° 22′; whilst at the same zenith distance, the sun's *chemical* brightness is only 36·6 times as great. Hence the value of this light as a source of the chemically active rays for photographic purposes becomes at once apparent.

Professor Bunsen and the speaker state in the memoir above referred to, that, "the steady and equable light evolved by magnesium-wire, burning in the air, and the immense chemical action thus produced, render this source of light valuable as a simple means of obtaining a given amount of chemical illumination, and that the combustion of this metal constitutes so definite and simple a source of light for the purpose of photo-chemical measurement, that the wide distribution of magnesium becomes desirable. The application of this metal as a source of light may even become of technical importance. A burning magnesium-wire of the thickness of 0·297 millimetre, evolves, according to the measurement we have made, as much light as 74 stearine candles of which five go to the pound. If this light lasted one minute, 0·987 metre of wire, weighing 0·120 gramme would be burnt. In order to produce a light equal to 74 candles burning for ten hours, whereby about 20 lb. of stearine are consumed, 72·2 grammes (2¼ ounces) of magnesium would be required. The magnesium-wire can be easily prepared by forcing out the metal from a heated steel-press having a fine opening at bottom. This wire might be rolled up in coils on a spindle, which could be made to revolve by clockwork; and thus the end of the wire, guided by passing through a groove or between rollers, could be continually pushed forward into a gas or spirit-lamp flame, in which it would burn."

It afforded the speaker great pleasure to state that the foregoing suggestion had now been actually carried out. Mr Edward Sonstadt has succeeded in preparing magnesium on the large scale, and great credit is due to this gentlemen for the able manner in which he has brought the difficult subject of the metallurgy of magnesium to its present very satisfactory position.

Some fine specimens of crude and distilled magnesium, weighing 3 lbs., were exhibited as manufactured by Mr Sonstadt's process, by Messrs Meller and Co., of Manchester.

The wire is now to be had at the comparatively low rate of 3*d.* per foot; and half an inch of the wire evolves, on burning, light enough to transfer a positive image to a dry collodion plate; whilst, by the combustion of 10 grains, a perfect photographic portrait may be taken, so that the speaker believed that for photographic purposes alone the magnesium light will prove most important. The photo-chemical power of the light was illustrated by taking a portrait during the discourse. In doing this the speaker was aided by Mr Brothers of Manchester, who was the first to use the light for portraiture.—*Pharmaceutical Journal.*

DEATH OF DR BAIKIE.

It is with great grief we announce the death of Dr William Balfour Baikie, which took place at Sierra Leone, on the 30th of November 1864. The loss is a national one, but nowhere is his death more deplored than in this city, where he has left many friends. After a long sojourn in Central Africa, he was returning home, but unfortunately was for some time detained on the coast, where he contracted the dysentery, which proved fatal. Dr Baikie's own opinion had long been, that the climate of the interior of Africa was not unhealthy, although at many parts of the coast it is very deadly. The correctness of this opinion has been painfully verified in his own case. It does, indeed, seem a mysterious dispensation of Providence, that he should have been cut off just as he had finished his labours and was on his return home, to enjoy the repose he had so amply merited.

Dr Baikie was a native of Kirkwall, and we extract the following particulars from the *Orkney Herald*:—

"About the age of sixteen Dr Baikie proceeded to the University of Edinburgh for the purpose of studying medicine, and passed through the regular curriculum in that world renowned medical school. Professor Traill—a native of Orkney like himself—was one of his instructors, and he was a special favourite with Professor Christison, who employed him as clinical clerk in the Royal Infirmary—a kind of training which he would find of great service in after years. So profound was the respect and affection entertained by Dr Baikie for this eminent Professor, that he gave the name of Mount Christison to a lofty hill along the reaches of the Binue. Natural history was one of his favourite studies; and in 1848, at the close of his University curriculum, there was issued from the press 'A Catalogue of the Mammalia and Birds observed in the Orkney Islands,' by Messrs W. B. Baikie and Robert Heddle—the latter gentleman being also an Orcadian. It was soon after the publication of this little treatise that Dr Baikie obtained an appointment in the Royal Navy. It was probably while cruising in one of her Majesty's ships about the African coast, that he first became impressed with the trading capabilities of the interior of the continent, and formed the resolution to offer his services when an exploring expedition was fitted out by Government.

"In the summer of 1854, Government determined to send an expedition to the river Chedda or Tsadda in Central Africa, and we find it stated in the Admiralty instructions to the late Consul Beecroft—'You will be accompanied by Dr Baikie, a medical officer of the Royal Navy, who is a naturalist, and who has been preparing himself during the past year with this object.' Dr Baikie's first experiences as an African explorer were given to the world in the form of a bulky and interesting volume, entitled 'Narrative of an Exploring Voyage up the rivers Kwora and Binue (commonly known as the Niger and Tsadda) in 1854.' The expedition, described in this narrative, produced important results, and mainly owed its success to Dr Baikie's energy and ability. . . . . .

"The Pleiad, screw-schooner, which had been built specially for the expedition, took her departure from Dublin on the 20th May 1854, and it was arranged that the officers of the expedition should join her at Fernando Po. In company with Dr Bleek, a German ethnologist, Dr Baikie, who had been appointed medical officer and naturalist, left Plymouth by the Forerunner packet on the 24th May 1854, to join the Pleiad. According to arrangement, Mr Beecroft, her Majesty's consul at Fernando Po, was, on the Pleiad reaching that port, to take command of the expedition; but before the vessel arrived, Mr Beecroft had died. Dr Baikie being next in seniority then took the command and proceeded on the voyage. . . . .

"While cruising up the Binue, amid strange wild scenery, and stopping occasionally to make excursions into the surrounding country, for the double purpose of exploration and of bartering with the various communities of natives, Dr Baikie on one occasion wandered from his companions, and lost his way. It was past sunset, and getting rapidly dark, and he resolved to roost on a tree for the night. He clambered some fifteen or twenty feet up a baobab tree, placed himself on a branch about a foot in diameter, and leant against the main trunk, peering into the obscure. He passed a cord loosely round the branch, so that he could put his arm through it and steady himself. He lay down on his face along the branch, passed each of his hands into the opposite sleeve, and was soon in a state of oblivion. Four hours he slept on that strange couch, and when he awoke all the strange sounds of midnight solitude were around him, from the howls of the leopard and hyæna to the chirping of crickets, the croak of frogs, and the deadly buzz of mosquitoes. After being eleven hours in the tree, Dr Baikie descended as the morning advanced, and was guided back by natives to his companions.

"Owing to the hostility of the natives, it was deemed advisable to return on the 29th September, and the Pleiad again reached the coast, after a trip of sixteen weeks' duration. The commander of the expedition felt grateful that all had been spared to return in health and strength. On the 28th November he sailed for England, and was subject, during the earlier part of the voyage, to remittent fever; but, curiously enough, the stay at Sierra Leone had then a beneficial effect. The expedition which Dr Baikie commanded had thus resulted in the discovery of a navigable river leading into the heart of an extensive and thickly-populated continent, teeming with animal life, and abounding in rich vegetable products. The natives were, for the most part, found anxious to trade with white men, and thus another important outlet for home manufactures had also been discovered. Dr Baikie's observations led him to hold strongly the opinion, that, for the promotion of commerce, it was necessary that Europeans should become traders or settlers in Africa. He concluded his narrative with an urgent appeal to Government to lose no time in fitting out another expedition.

"Dr Baikie visited his friends in Orkney after his first expedition. In 1856 the narrative of his exploration was published, and he seems immediately to have set his whole heart upon returning to Africa. An opportunity soon presented itself. He left Kirkwall in March 1857, and immediately afterwards embarked for Africa. In his second expedition up the Niger, which was destined to extend over the lengthened period of eight years, he was accompanied at first by Lieutenant H. Glover, R.N., an old messmate, who remained with him a couple of years, and who is now a Commander in the Royal Navy, and Colonial Secretary at Lagos. Dr Baikie's aim, in exiling himself for so many years in Central Africa, was to become thoroughly acquainted with the people and the trading capabilities of the country. For this purpose he established his headquarters chiefly at Lukoja, on the banks of the Niger, near the confluence of the Tsadda; but he also went occasionally several hundreds of miles inland, to Soccata and other places, where he could have no communication from England, and could only himself seize every rare chance of sending letters to this country, by caravans crossing the desert of Tripoli. Letters from England, when they reached him, were sometimes a year old."

A notice of Dr Baikie, in the *Athenæum* of 14th ult., concludes as follows:—"Satisfactory arrangements have been made by the Foreign Office, to render the work Dr Baikie has done, and in the accomplishment of which he sacrificed his life, permanently useful. The numerous extracts from his despatches published by the Government, and his papers in the *Transactions* of our various scientific societies, show the energy he developed in so trying a climate, and the great range of his observations. We trust that they may all be carefully collected into a separate work, so that the general public may see the vast result obtained, with but slender means, and the great loss we have sustained by the death of this excellent pioneer of commerce and civilisation."

CONVERSAZIONE OF THE ROYAL COLLEGE OF SURGEONS.

On Thursday the 19th of January, a Conversazione of the Royal College of Surgeons was held in their Hall, Nicolson Street, when Dr Rutherford Haldane delivered a Lecture on "The Modern Practice of Medicine," which will be found at page 671 of the present Number of this Journal. On the motion of Lord Deas, seconded by Professor Syme, a vote of thanks was accorded to Dr Haldane.

ABSTRACT OF MOVEMENT OF MEDICAL PROFESSION,

AS PER SCOTTISH LOCAL MEDICAL REGISTER.

(*To the Editor of the Edinburgh Medical Journal.*)

Edinburgh, 18th January 1865.

My dear Sir,—The annexed abstract will probably interest such of your readers as occupy themselves with the subject of Vital Statistics.

The table requires no explanation, and it is merely necessary to add, that the Scottish Local Medical Register contains the name of almost every duly qualified practitioner resident in Scotland, and, in addition, the names of a few practitioners resident in the other divisions of the United Kingdom, and also the names of a larger number who are in the public service, or who are at present practising in the Colonies or in foreign countries.—I am, my dear Sir, yours very truly, William Robertson, M.D.

Registered at		During each Year.			Lives at risk for Year.
1st Jan.	Nos.	Cancelled.	Died.	New Entrants.	
1859	1615	4	29	155	1692
1860	1737	...	31	127	1800
1861	1833	...	37	158	1912
1862	1954	...	49	116	2012
1863	2021	...	46	174	2108
1864	2149	1	74	168	2233
1865	2242				
			266	898	11757

On the Roll, at 1st January 1865, ...	2242
Cancelled, up to do., ...	5
Died, up to do., ...	266
New Entrants between 1st Jan. 1859, and 1st Jan. 1865, .	898
Years of Life at risk during above period, ...	11757 years.

Annual Rate of Mortality 2·2625, or 1 death out of every 44·2 living.

VIRCHOW AND THE CELLULAR PATHOLOGY.

A CORRESPONDENT from Berlin writes:—Perhaps the most important thing I have to tell you is as to a recent change in Virchow's opinion regarding the cell theory. This change has been caused by the discoveries of Recklinghausen (Virchow's Archives, about a year ago) in the cornea. He has shown that the corneal cells have not special cell-walls, but are merely *spaces* between masses of intercellular substances. The nuclei in the angles he therefore considers *free*, and he says that many of them can move along the canaliculi from one angle to another. Moreover he says that the interior of these canals is continuous with that of the lymphatics; you can inject the lymphatics from them; so that, according to him, the origin of the lymphatics is to be found in the canaliculi of the so-called connective-tissue corpuscles. Then he says that the corpuscles of tendon and connective tissue are merely spaces with contained nuclei,—a view which, of course, is *not new* to an Edinburgh man. Virchow admits all this; he admits that the corneal corpuscles are not cells. He seems rather reluctant to admit that those of tendon and connective tissue are the same, but he does not deny it; and he told me personally that he *now did not regard a cell-wall as an "essential part of the cell,"* as stated in Cellular Pathology; but that a nucleus surrounded by a molecular blastema was sufficient to constitute a cell: then he says that the outer part of this cell blastema consolidates and forms a cell-wall, as Beale has shown, and that this takes place in the amoeba when placed in fresh water. This, of course, is a great triumph for Goodsir, who long ago was cautious enough not to say that the cell-wall is always present.

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Hillier, — Handbook of Skin Diseases for Students and Practitioners. By Thomas Hillier, M.D. London, 1865.
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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On some Laws of the Production of Twins.* By J. MATTHEWS DUNCAN, M.D., F.R.S.E., F.R.C.P.E., Lecturer on Midwifery, etc., etc.

(*Read before the Medico-Chirurgical Society, February* 1, 1865.)

HITHERTO the variations of fecundity have been most frequently and most easily observed in animals which have ordinarily multiple births, and the number of whose young born at different litters varies. Such difference in number at different times cannot fail to strike the observant eye, and little ingenuity is needed to make the number of young a test of the degree of fecundity. To such an argument, I can find no objection.

Among women the birth of twins occurs once in about eighty deliveries. Triplets, quadruplets, quintuplets, and even higher figures are occasionally observed; they are very uncommon, and the rarity is progressive with the number. The normal or ordinary rule in woman, is to bear one child at a time, and the next most frequent condition is temporary or persistent sterility,—two points in which she signally differs from what is generally believed of the animals subjected to the observations referred to.

It is easy to understand how a regularly bearing animal, the number of whose young born at different litters varies, may in its individual history give good illustration, if not evidence, of some law of the variation of fecundity. In irregularly bearing uniparous woman no such illustration or evidence of any law can be got by observing an individual, and recourse must be had to statistics, to the analysis of the details regarding the pregnancies of large numbers of women, subjected to the conditions essential to fecundity.

The rarity of a plural birth in woman, and the increased danger to both mother and offspring in these circumstances, render such an event, in a certain limited sense, an abnormity. This abnormity, if used alone, is not a good test of fecundity, and it has been resorted to for this purpose, in consequence of the enticement of its apparently simple and easy application to woman as to the lower animals.

It is, at first sight, evident that a woman even repeatedly bearing twins, may be surpassed in fecundity by another bearing single children rapidly or for a longer period. Neglect of these and other such truths has led to errors, and I shall cite a popular hasty conclusion regarding the comparative fecundity of races in illustration. The greater frequency of plural births in one race than in others, as in the Irish compared with the English, has been represented as showing that the former is more prolific than the latter. Whether the conclusion be true or not, I do not here propose to inquire;[1] certainly, the grounds of the conclusion are insufficient to establish it, and unsatisfactory, seeing that an opposite conclusion is not absolutely inconsistent with them, as I have just shown. And a little ingenuity easily discovers other arguments against the popular view; for example, it might be said that twins as an unnatural and excessive strain upon the female reproductive powers may exhaust the fecund energies, and lead to comparative barrenness subsequently.

In illustration of the treatment of this subject by physiologists, I cite the following passage from the works of Burdach:[2]—"In regard to age (says he), fecundity is diminished in the first and last portions of the continuance of the aptitude for procreation. The elk, the bear, etc., have at first only a single young one, then they come to have most frequently two, and at last again only one. The young hamster produces only from three to six young ones, whilst that of a more advanced age produces from eight to sixteen. The same is true of the pig. This rule appears to be general, since it applies also to the entomostraceæ; according to Jurine, the number of the young of the monoculus pulex is at first from four to five, afterwards rising gradually as high as eighteen. We scarcely ever encounter the births of three or four children except in women who have passed the thirtieth year.[3] Precocious marriages are not only less fertile, but the children also which are the result of them have an increased rate of mortality. According to Sadler, every marriage in the families of the peers of England yields 4·40 children when the woman was married below 16 years of age, 4·63 from this age to 20, 5·21 from 20 to 23, and 5·43 from 24 to 27."

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[1] The facts as they appear in Collins' data and in my statistics of Edinburgh and Glasgow in 1855 are as follows:—In the Dublin Lying-in Hospital 16,385 women produced 480 twin children. In Edinburgh and Glasgow in 1855, 16,301 wives produced 396 twin children. As already often repeated, the two sets of cases are not suited for exact comparison. The result (including all sources of error), is, that the women delivered in Edinburgh and Glasgow should have produced 477 twin children instead of 396, if they were resolved not to allow themselves to be surpassed in double births. The variation of the frequency of twin births in different countries is so great (Oesterlen, Handbuch der medicinischen Statistik) as to remove all probability from the notion or belief that the greater or less frequency of twins shows greater or less general fertility.

[2] Physiologie, tome ii. p. 117.

[3] There is a general deficiency of evidence for Burdach's statements. In order to test his assertion concerning the ages of women bearing triplets and quadruplets, I have hastily collected the following ten authentic cases of triplets from the works of Collins, M'Clintock and Hardy, Braun Chiari and Spæth, Hugenberger, and from my note-book. They speak for themselves, and it will be observed that triplets are by no means exclusively confined to women above

In support of Burdach's statements generally, I here quote an extract from a letter on this subject from the highly intelligent gamekeeper of a Scottish nobleman. It forms a very favourable example of the kind of information I have been able to collect on this topic. "My observation has led me to believe that, as a general rule, a bitch has fewer puppies the first, and gradually increases year by year till her strength begins to fail. Then a rapid diminution follows, ending in one or two. This rule, however, is very materially affected by circumstances, and one may come to very erroneous conclusions by overlooking these. Such for instance is, the state of the health, condition, amount of work. According to the state of the health, there may be many or few (puppies), weak or strong production; according to the condition, there will be few or many. If the animal is in good fair condition, there will be many; if overfed, few. And, as regards the amount of work a dog has to perform, so will the decline be rapid or gradual; and hence, if a bitch is worked hard year after year, she will fail rapidly, and the diminution of her puppies will be accordingly; but if worked moderately, and well kept, she will fail gradually, and the diminution will be less rapid.

"The above rule holds good in reference to breeding sows, but it is a common thing in their case for condition (fatness) to interrupt the rule. When a sow or bitch in an overfed state is brought in contact with the male, and afterwards increases in fat, which in this state they are very liable to do rapidly, consequently there must be a pressure upon the womb, and hence the embryo will be crushed, and thus production will be prevented. I believe it is well known how easily the embryo can be injured or destroyed, but I do not think it is well known how much it will endure in the fœtal or more advanced state. We had an instance of this last year, when the great spate was in the month of May, when so many of our pheasant eggs were carried away by the flood and covered in water, many of which we lifted from under one and two feet of water, and

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thirty years of age. Yet it is noticeable that not one occurs among the younger childbearing women, and not one in a first pregnancy.

Table of Ten Cases of Triplets.

Age of mother	27	27	29	30	30	33	35	35	36	37
No. of pregnancy	3	3	2	3	11	2	3	6	3	2

An interesting fact in connexion with this subject is mentioned in Hugenberger's report of the St Petersburg Midwives' Institute (1863.) Three women admitted there between 1845–59 in their fifteenth pregnancies had triplets, and each had triplets three times in succession.

many we got floating about in the ditches, which had been a night and a good part of a day in the cold water. Now, we knew that many of these eggs had been a good while sat upon; and being anxious if possible to save some of them, got them set under common hens, and many of them hatched wonderfully well: some of them came out a few days after we set them. Now, had these eggs been in the embryo state, a little shake and being in the cold water an hour or two would have rendered them quite useless.

"I have never observed any cause that led to a greater proportion of males or females in a litter. From what I have observed, I do not think that age or strength on either side has anything to do with it."

I. *The Number of Twins born of Women of different Ages.*

I have made a collection of 1512 twins from the sources indicated in the footnote,[1] and have arranged them so as to show the various ages of their mothers in quinquennial periods. This table shows that in the general population, it so happens that the number of twins born increases with the age of the mother until the age of from 25 to 29 inclusive is reached, and that after this age is passed the number of twins born regularly diminishes, —a result which accords with what is observed of the fertility generally of the whole female population; the actual fertility of the female population increasing from the commencement of the child-bearing period of life until the age of thirty is reached, and then declining to its extinction with the child-bearing faculty.[2]

TABLE I.—*Showing the Ages of* 756 *Mothers of Twins.*

AGES,	15-19	20-24	25-29	30-34	35-39	40-44	45	Total.
Collins,	3	53	76	71	28	8	1	240
M'Clintock,	2	23	45	41	17	1	...	129
M'Clintock and Hardy,	1	20	34	26	12	2	...	95
Chiari, Braun, and Spæth,	1	25	36	26	4	2	...	94
Statistics of 1855,	3	28	46	58	52	11	...	198
Total,	10	149	237	222	113	24	1	756

But while there is this general accordance of results, an examination of the table at once reveals to the observer great differences between the general fertility of women and their fertility in twins. These differences will be the subject of further description; in the meantime, I shall only adduce sufficient evidence to show that there is great difference, so much indeed as at once to demonstrate that the production of or fertility in twins is in woman not subjected to the same laws as fertility generally.

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[1] Collins, Practical Treatise; Private letter from Dr M'Clintock, giving extracts from the Dublin Hospital Register; M'Clintock and Hardy on Midwifery and Puerperal Diseases; Chiari, Braun und Spæth, Klinik der Geburtskunde; my own extracts from the Registers of Edinburgh and Glasgow for 1855.

[2] Transactions of the Royal Society of Edinburgh for 1863–64, p. 479.

The mean age of 16,385 parturient women included in Dr Collins' tables is 27 years. The mean age of 240 women in the same lists bearing twins is 29. The twin-bearer is older than the general run of bearers.

The number of twins born by women under and above 30 years in Dr Collins' lists is 153 and 87, showing a majority of 66 on the side of the younger women, and thus a smaller proportional number of twins born of the young women than of all children born of the same.

But the data of Dr Collins are not the best I can adduce to elucidate this point. His are derived from a class of cases submitted to selection, the conditions being all those connected with admission to the Lying-in Institution of Dublin. I shall bring forward data derived from an analysis of the whole legitimate births in Edinburgh and Glasgow in 1855. These show still more markedly and satisfactorily than the data of Collins, that a far larger proportion of twins than of children generally is born of elderly women. It is easy to imagine reasons for the greater though similar difference shown by my statistics than by those of Collins; and there can scarcely be a doubt that mine are, in regard to this point, far more reliable than his. The mean age of 16,301 mothers of legitimate children in Edinburgh and Glasgow, in 1855, was above 29 years. The mean age of 198 wives bearing twins was 31 years. The twin-bearer is here again older than the general run of bearers. In the same population, the number of twins born by women under 30 years of age was 86; the number born by women above 30 years was 112,—showing a majority of 26 on the side of the elder women. Or, while of all births among these 16,301 wives three-fifths occurred among women under 30 years of age, there occurred only two-fifths of the twins among these younger women.[1]

II. *The Influence of Age on Woman's Fertility in Twins.*

I here first produce a table formed by adding the data of Dr Collins to my own, and showing the very remarkable result that, speaking generally, the older a mother is the more likely is she to have twins.

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[1] It is necessary to remark that the statements here given are not absolute or exact. For while the Dublin statistics include all births at or near the full time, the statistics of Edinburgh and Glasgow include the same, with the exception of those born dead. But it is evident that were the figures to be exactly true, not only should the dead-born be included, but also all born in miscarriages or abortions. Until such comparative statistics are procured as include all births, mature and premature, living and dead, no statement, even of the comparative frequency of twin births, can be relied on; for fewer plural pregnancies come to maturity than pregnancies with single children. Chiari, Braun, and Spæth have shown that abortions are comparatively more frequent in plural pregnancies than in ordinary pregnancies.

TABLE II.—*Showing the Actual Fertility in Twins of Mothers of Various Ages.*[1]

AGES, . . .	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	Total.
Mothers, ...	1138	8550	10346	7667	3617	1237	118	12	1	32686
Mothers of Twins, .	6	81	122	129	80	19	1	...	...	438
Proportion of latter to former is 1 in	189·6	105·5	84·8	59·4	45·2	65·1	...	...	...	74·6

While of mothers from 15 to 19 years of age only every 189th had twins at a birth, mothers rising in age were more prolific in twins till at the age of from 35 to 39 years the climax of fertility in twins was reached, every 45th woman producing two at a birth, or fully four times as many as the women under twenty. This statement of gradually increasing fertility in twins does not appear to hold good after the age of 40 is reached,—a circumstance probably somewhat explained by or connected with the fact that at that age the general fecundity of woman diminishes somewhat suddenly and rapidly, as I have elsewhere shown. And I shall have presently to point out an analogous divergence from the same statement, but in an opposite direction, when I treat of the influence of primiparity in increasing twin-births. I venture therefore, founding on the above observations, to state the law, that from the earliest child-bearing period till the age of 40 is reached, that is, till a period when fecundity has become extraordinarily diminished, the fertility of mothers in twins gradually increases.

I know of no table of data illustrating the general productiveness of fertile women with which my second table may be justly compared. The kind of table I desiderate is one showing the fertility of mothers according to age, or, in other words, the varying fertility of fertile women of various ages. And I believe that the results of such a

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[1] The numbers in this table are got by adding together the numbers given by Collins and those extracted by myself from the registers of Edinburgh and Glasgow for 1855.

The following table shows the like facts; only it is confined to the legitimate viable births in Edinburgh and Glasgow in 1855. The results also are similar, with this difference, that twin births are more frequent in the first than in the second quinquennial period :—

TABLE III.—*Showing the Actual and Comparative Fertility in Twins, of Wives Mothers of Various Ages.*

AGES, ...	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
Wives mothers, ...	376	3688	5037	3850	2407	840	103	16301
Wives mothers of twins, . .	3	28	46	58	52	11	...	198
Propor. of latter to former is 1 in	125	132	109	66	46	76	...	82

table would be to some small extent in accord with those just adduced regarding twins.[1]

But this second table, showing a fertility in twins gradually increasing with age, is almost exactly opposed to what our knowledge of the fecundity of women generally would lead us to expect. The general productiveness of a mass of wives is greatest at the commencement of the child-bearing period of life, and after that epoch gradually diminishes.

In the second and third tables, twin-bearing mothers are compared with the whole mothers. I here produce a table of twin-bearing mothers compared with married women of corresponding ages. This fourth table is, in more respects than one, not very satisfactory. It cannot, I think, be expected to yield much. Yet its evidence is to the effect, that when woman generally is most fecund and productive she is least likely to produce twins. Between the ages of twenty and thirty years fewest wives have twins. Before and after that period of high general fecundity the special productiveness in twins increases. And this result is in general accordance with what we have already shown regarding productiveness of twins,—that is, it is the opposite of what we know of general fecundity; moreover, it may find some special support from the evidence of the third table, and of the seventh.

TABLE IV.—*Showing the Fertility in Twins of Wives of Various Ages.*[2]

AGES, ...	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Wives, ...	756	8874	14622	14579	11871	10506	7537
Wives mothers of twins, . . .	3	28	46	58	52	11	...
Proportion of latter to former is 1 in	252·0	316·9	317·8	251·3	228·3	955·1	...

III. *Initial Fertility in Twins at different Ages.*

The results arrived at in the former chapter are confirmed by a comparison of the initial fecundity of wives generally with the special initial fecundity in twins of the same women. The Fifth Table is unfortunately not large enough in numbers to afford results of a high degree of reliability. I believe that a more extensive collection will probably show a regular increase of initial productiveness of twins with increasing age. I leave it as it stands, showing that the wives married youngest have the fewest twins, and that there is an increase as age advances.

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[1] I may take the liberty to state, that this opinion is founded not on conjecture, but on a comparative study of tables of the fertility of mothers which I have in preparation.

[2] The wives are estimated for Edinburgh and Glasgow for 1855. The mothers of twins are those of Edinburgh and Glasgow in 1855.

TABLE V.—*Showing Comparison between Wives newly Married, and Mothers of Twins within Two Years of Marriage.*[1]

AGES, ...	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of wives newly married, .	700	1835	1120	402	205	110	75	4447
No. of mothers of twins within two years of marriage, .	2	15	16	4	2	...	...	39
Prop$^{\text{n}}$. of latter to former is 1 in	350	122	70	100	102	...	...	114

I have already said, that fertility in twins is better or more justly contrasted with the fertility of fertile women than with the fecundity of a mass of wives both sterile and fertile. In the former case, all the women brought into comparison bear children, and thus show their fecundity and fertility, and their adaptation for comparison, while in the latter case women fertile in twins are (as in Table V.) compared with both women who are fertile and with those who are sterile. In the sixth table I establish a comparison between two sets of fertile women, the one bearing single children, the other bearing twins. What does this table show? It remarkably confirms the law already stated as to the increase of twins as fertile women grow older. And there is here seen a regular increase up till the age of 40 is reached. Every 153d woman among the youngest fertile women bears twins (within two years after marriage); among the older women, from 35 to 40 years of age, every forty-second woman bears twins within two years after marriage, or nearly four times as many.

TABLE VI.—*Showing Comparison between Mothers within Two years of Marriage, and Mothers of Twins within Two Years of Marriage.*[2]

AGES, ...	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of mothers within two years of marriage, . . .	306	1661	849	253	84	17	2	3172
No. of mothers of twins within two years of marriage, .	2	15	16	4	2	...	...	39
Prop$^{\text{n}}$. of latter to former is 1 in	153	111	53	63	42	...	...	81

I here interpolate another, seventh table, which is supplementary to the sixth. It is composed of all mothers except those who have born children within the first two years of marriage. Its evidence is to the same general effect as the preceding, only it shows the increased fertility in twins of the youngest women,—a circumstance to which I have already called attention.

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[1] Here not only are the twins comparatively few, but also the numbers of wives are not real or actual, but estimated for Edinburgh and Glasgow in 1855. See Trans. Roy. Soc. of Edin. 1863–64. The mothers of twins within two years of marriage are extracted from the registers of Edinburgh and Glasgow for 1855.

[2] In this table the figures may with justice be regarded as the actual numbers (not estimated) in Edinburgh and Glasgow in 1855.

TABLE VII.—*Showing Comparison between Mothers Two Years Married and upwards, and Mothers of Twins Two Years Married and upwards.*

AGES, ...	15-19	20-24	25-29	30-34	35-39	40-44	45 & up.	Total.
No. of mothers two years married and upwards, .	70	2027	4188	3597	2323	823	101	13129
No. of mothers of twins two years married and upwards,	1	13	30	54	50	11	...	159
Prop^{n}. of latter to former is 1 in	70	156	139	66	46	75	...	82

The especial value of these tables of initial fertility, and of subsequent fertility at various ages, lies in their eliminating entirely, or almost entirely, the disturbing element of the number of the pregnancy, and leaving evidence of the effects of age simply. In short, they contribute to the demonstration of the law of the influence of age stated in the second chapter of this paper.

IV. *The Relation of the frequency of Twins to the Number of the Mother's Pregnancy.*

If it be true that the older a woman is who still retains a considerable degree of fecundity, the more likely is she to bear twins,

TABLE VIII.—*Showing the Actual and Comparative Number of Twins born in first and subsequent Pregnancies.*[1]

Number of Pregnancy.	No. of Children.	No. of Twins.	Per-centage of latter to former.
1st	3722	45	1·21
2d	2893	19	·66
3d	2534	24	·94
4th	1982	19	·96
5th	1543	19	1·23
6th	1221	18	1·47
7th	848	17	2·00
8th	641	14	2·18
9th	425	14	3·29
10th	222	3	1·35
11th	152	2	1·31
12th	61	3	4·92
13th	34	...	...
14th	11	...	...
15th	6	...	...
16th	2	1	50·00

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[1] It is necessary to note a source of error that exists in this table. In the third column are given the twins registered in 1855, and no other twins are supposed to have been in the family previously. The registers only give the number of previous viable children, whether twins or not; they do not give the number of pregnancies. These are taken directly from the number of viable children.

then we should without further data guess that twins were comparatively more frequent in late than in early pregnancies. And this is confirmed by an investigation of the subject. In the preceding table are given the number of children born in Edinburgh and Glasgow in 1855 in first and subsequent pregnancies, and beside them are placed for comparison the number of twins born in the same. A glance at the table shows that up to the ninth pregnancy, far beyond the average number of pregnancies, and as far as we have considerable numbers to guide us, the proportional frequency of twins increases with the number of the pregnancy. To this general statement there is an exception in the case of first pregnancies. Woman has apparently an increased chance of bearing twins in her first pregnancy, which leads to a disorder of the general rule above stated. With this notable exception, the rule holds manifestly good, at least till the ninth pregnancy is passed. After the ninth pregnancy, the table, from the smallness of the numbers and the irregularity of the results, cannot be much relied on. Yet it shows, on an average, a still increasing fertility in twins as the number of the pregnancy advances. According to Table VIII., among a thousand child-bearing women pregnant for a first, second, or third time, as these pregnancies happen to occur in our population, nine only will produce twins; among a thousand similar women in fourth, fifth, and sixth pregnancies, there will be twelve twin-births; among a thousand similar women in seventh, eighth, and ninth pregnancies, there will be twenty-four twin-births; and among a thousand similar women in tenth, eleventh, and twelfth pregnancies, there will be twenty-five double births.

It may then be stated that, after passing the first pregnancy, a woman's chance of bearing twins increases with each subsequent pregnancy.

The broad statement that multiparous women are more likely to have twins than primiparous, has been made by Chiari, Braun, and Spæth, and by Hugenberger. But this is only an imperfect development of the general statement just enunciated.

I have carefully abstained from giving this general statement the dignity of a law; for it may be only a coincidence resulting from the circumstance that age of mothers increases as the number of the pregnancy increases. The law of increased frequency of twins with advancing age may afford the explanation of the increased frequency as the number of the pregnancy advances. It remains to be determined, then, whether this general statement be a law or only a corollary to the law of age.

In order to the more complete discussion of the influence of the number of the pregnancy on the frequency of twin-births, I here produce another table, whose interesting results throw light on the subject.

TABLE IX.—*Showing the Frequency of Twin Births and of all Births in First and Subsequent Pregnancies.*[1]

Number of Birth in Family.	Number of Twin-bearing Mothers.	Per-centage of Twin-bearing Mothers.	Per-centage of Mothers.
1st	45	22·7	22·8
2d	19	9·6	17·7
3d	24	12·1	15·5
4th	19	9·6	12·1
5th	19	9·6	9·4
6th	18	9·1	7·4
7th	17	8·6	5·2
8th	14	7·1	3·9
9th	14	7·1	2·6
10th	3	1·5	1·3
11th	2	1·0	·9
12th	3	1·5	·3
13th	...	...	...
14th	...	...	...
15th	...	...	...
16th	1	0·5	·01

The third column of this table shows how a hundred twin-births are distributed according to the number of the pregnancy. It is evident that, speaking generally, twinning becomes rarer as the number of the pregnancy increases; and, at the top of the list, far surpassing all the rest, is the first pregnancy with the large number of nearly 23 per cent. of all twin-births. Actually, then, twinning diminishes as the number of the pregnancy increases.

But we must go further into the matter, and we find that as the number of the pregnancy increases so the number of all births, single and plural, diminishes. The fourth column in table ninth shows how a hundred births are distributed among the pregnancies in which twins occurred. There is a regular diminution as the number of the pregnancy increases; and, in order to comprehend the relation of twin-bearing to child-bearing generally, this column must be contrasted with the preceding. Doing so we find the first four pregnancies forming a contrast to the subsequent pregnancies. In the former or first four pregnancies the proportion of twinners in a hundred twinners is smaller than the proportion of child-bearers in a hundred: in the latter or the fifth and subsequent pregnancies the proportion of twinners in a hundred twinners surpasses the proportion of child-bearers in a hundred, and the preponderance goes on increasing from the fifth, at least as far as the ninth pregnancy.

In this chapter I have shown that—

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[1] This table reads as follows:—22·7 per cent. of twin-bearing mothers are bearing their first viable children; 22·8 per cent. of all mothers are bearing their first viable children; and so on.

1. The actual number of twins born in different pregnancies decreases as the number of the pregnancy increases.
2. The comparative number of twins born in different pregnancies increases as the number of the pregnancy increases.
3. The increase of the comparative number of twins with the number of the pregnancy does not appear to hold good with the first pregnancy as compared with the three immediately subsequent pregnancies; women in their first pregnancies being more likely to bear twins than in those immediately subsequent.

V. *The Size of Families in which Twins occur.*

It is very desirable to know what influence the bearing of twins has upon women's fertility. Do women bearing twins have larger families than those never bearing but one at a birth? To this question one naturally gives an affirmative answer. Of course, if women's subsequent fertility be not affected by twin-bearing, there will be just an excess above ordinary families of a unit for every pair born in a family, and I daresay I am right in saying this is the popular belief. It receives some sort of support from the circumstance that twins are relatively more frequently additions to an already existing considerable family than they are either the first of a family or additional to an already existing small family. Moreover, twins occur with greater proportional frequency in elderly than in younger women, and are therefore certainly less likely and perhaps less liable to interfere with further or subsequent productiveness than if they occurred chiefly among the younger women.

To contribute to the solution of this interesting question I have framed the following table tenth. It also appears to support the affirmative response to the question just given. It shows that the average size of families of women married the same number of years is greater in the twin-bearing than in ordinary families, counting down to the birth of the twins. This is all that I can say in favour of the view or supposition that twin-bearing women have larger families than their neighbours. But the view is very far from being so demonstrated true.

TABLE X.—*Showing the Average Size of Families after Different Durations of Marriage in Mothers generally, and in Mothers bearing Twins.*[1]

Number of Years Married, . .	Under 5	5–9	10–14	15–19	20–24	25–29
Average size of Families, . .	1·699	3·940	6·063	7·967	9·868	13·075
Average size of Twin Families,	2·523	4·936	7·397	9·793	9·533	...

The tenth table seems to me certainly to show that twinning has no retrospective injurious influence on the fertility of woman; that a woman destined to have twins at some future time is in the meantime as fertile as any other; that twinning does not occur to supplement pre-existing deficiency. So far from this being the case, the table shows that twin-bearing women have on the whole been already more fertile than their neighbours; that the twins come as additions to families already numbering above the average. For, keeping in mind that twins are comparatively most frequent in the latter pregnancies of women, it is easily seen that while, in the first column that is among women under five years married, or in the earlier pregnancies of women, twinning does not add so much as a unit to the average size of the family in which the twins occur; in the next column, or among women from five to ten years married, twinning does add a unit; in the third and fourth columns containing the greater proportional number of twin-bearers the twinning adds more than a unit, or, in other words, the women, even if they did not have twins, have larger families than their neighbours. The table shows then that the great majority of women having twins are already more prolific than usual. The last column of women is evidently exceptional; it relates to women from twenty to twenty-four years married, and who have passed (as shown in other ways) their ninth pregnancy; and, as already shown, it is founded on a very narrow foundation of figures.

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[1] The mothers in this table are those only who continue fertile up till the different durations of marriage.

But all this is far from demonstrating the affirmative, that twin-bearers are more fertile than others. No doubt a twinner is in that birth more fertile than a uniparous woman. No doubt, as just shown, a twin-bearer is, counting up till the time of twinning, more fertile than a woman bearing one at a time. But the real question is not answered. Does a woman who has finished bearing a family, and has in that family had twins, produce more or fewer than a woman always uniparous? In other words, do twin-bearing women, who have shown all the fertility of their lives, produce larger or smaller families than women uniformly uniparous? This is the real question. To it the above-mentioned arguments do not supply a conclusive answer. And I regret to say I must leave it unanswered. Only I admit that the affirmative is probably the true answer. To procure a reliable solution mothers must be compared who have born their last children. I have no such data. Table tenth is not a table of women who have born their last children. It carries the women of a population down only to their children born and registered in 1855.

In bringing this paper to a conclusion, I may remark that the chief results of it appear to me to be well established by the evidence. Yet I cannot but feel that a larger accumulation of data would have added to their security and firmness. Besides, had the laws of the fertility of woman been previously worked out so as to be used for comparative investigation, we should have thence probably been able to add much in illustration, contrast or corroboration, and possibly something also in the way of correction of the conclusions here arrived at.

It is interesting, first of all, to note that twin-bearing is not an accident, that it is subjected to laws of which we now have a glimpse. A philosopher might have fancied twinning to be the result of the act of conception taking place on the rupture of a Graafian vesicle fortuitously containing a double ovum or two ova, or on the rupture of two Graafian vesicles accidentally matured simultaneously; in other words, that twinning was the result of some transcendental primordial energies in the ovary, whose products might be attributed to chance, because their origin was not only unknown but inscrutable by any known means of investigation. The data, as here arranged, compared and reasoned upon, seem already to carry us so far as to remove twin-bearing from the category of the accidental, and to indicate to us laws of their occurrence which may be steps in the ladder of ascent to higher knowledge and wonderful discovery.

In the course of this paper I have attempted to demonstrate the following conclusions:—

1. The largest number of twins is produced by women of from twenty-five to twenty-nine years of age; and on each side of this climax of fertility in twins there is a gradually increasing falling off in their number, as age diminishes on the one side and increases on the other. (Table I.)

2. Twins are not regularly distributed among births generally; their production, therefore, is not subjected to the same laws as govern ordinary fertility.

3. The mean age of twin-bearing mothers is greater than that of mothers generally.

4. Twins increase in frequency as mothers become older (Table II.). This forms a striking contrast to the fecundity of a mass of wives (not mothers) which diminishes as their age increases.

5. Newly married women are more likely to have twins the older they are (Tables V. and VI.)

6. While the fecundity of the average individual increases with age till twenty-five is reached, and then gradually diminishes, there is some probability that the opposite is true, so far as regards twins alone, fertility in twins being greatest when fecundity is least, and *vice versa*. (Tables III., IV., VII.)

7. The actual number of twins born of a mass of women in different pregnancies decreases as the number of the pregnancy increases. (Table VIII.)

8. The number of twins relatively to the number of children born in different pregnancies increases with the number of the pregnancy. In other words, a woman is more likely to have twins in each succeeding pregnancy than in the former pregnancy.

The first pregnancy perhaps forms some exception to this rule.[1] (Tables VIII. and IX.)

9. In an individual twin-bearing is, of course, a sign of high fertility at the time. It also, in a mass of women, shows a high amount of fertility, at least till the time of the birth of the twins. (Table X.)

10. It is probable, though not proved, that twin-bearing women have larger families than women uniformly uniparous.

ARTICLE II.—*On the Influence which Consanguinity in the Parentage exercises upon the Offspring.* By ARTHUR MITCHELL, M.D., Deputy Commissioner in Lunacy for Scotland.

A SERIES of quotations might easily be given to show that there exists a remarkable difference of opinion as to the influence which consanguinity in the parentage exercises upon the offspring.

Further,—such a series of quotations could not be made, on anything like an extended scale, without finding that Scotland is pointed at as occupying a peculiar position in relation to the subject.

The voice of the people in this country, whatever its practice may do, condemns blood-alliances, and declares them to be productive of evil. Educated and uneducated may be said to entertain this belief equally; and as every one has considerable opportunities of testing its accuracy by personal observation and experience, the probability of its being simply and wholly a traditional error becomes small. Even among professional men, there is scarcely less unanimity in this general condemnation. But their wider knowledge and habits of closer observation lead often to qualifications and doubts; while occasionally we find an investigator wholly denying the evil, and characterizing the dread of such unions as "a superstitious fear."

If we carefully study the literature of the subject we shall find that it abounds in unsupported assertion, and that important conclusions are very often made to rest on a basis which is undefined or clearly too narrow. Yet, somehow, in spite of this, we rise from such a study with little doubt as to the reality of some evil effect, though we may feel strongly that its character and its measure are not well-known, and that of the nature of those conditions by which it is evidently modified we are still very ignorant.

Both general and professional opinions on this subject rest, in no small degree, on a peculiar and faulty kind of evidence. When we are presented with the question,—"Do these marriages in your experience appear to injure the offspring?" memory searches for instances of such unions, from the history of which the answer is to be framed. Now, it is certain that all those cases which have been marked by misfortune will be first called up, while many of those which have exhibited no evil effect or no peculiarity of any sort will be passed over or forgotten. The attention, in all likelihood, has been frequently drawn to the first, while nothing may have occurred in the progress of the last to keep alive the recollection of the *kinship* in the union. I need scarcely say that facts collected in this manner are almost sure to lead to inferences beyond the truth, yet it is from such data that conclusions on this subject have frequently, if not usually, been drawn.

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[1] It is not easy to get a regular view of Hugenberger's statistics of twins. But a study of them leads me to entertain some doubt of this exceptional character of first pregnancies.

During the course of 1860, I collected some evidence of this character myself. It embraces the history, more or less complete, of 45 cases. They belong chiefly to one district of Scotland, and are nearly all from the upper and middle classes of society. Without exception, every case communicated to me during the period of my inquiry is included; my informants were intelligent and observing men, and their statements I believe to be substantially correct. The result I give below:—

In 8 cases no evil result was observed.
In 8 cases sterility (virtual or actual) followed.

The 29 cases which remain gave,—

8 Idiots.
5 Imbeciles.
11 Insane (mania, melancholia, dementia, etc.).
2 Epileptics.
4 Paralytics.
2 Deaf-mutes.
3 Blind (congenital?).
2 With defective vision.
3 Deformed (spinal curvature, etc.).
6 Lame (character of lameness not given).
1 Rachitic.
22 Phthisical, scrofulous, or manifestly of weak constitution.

My notes show 146 as the total number of children born of these 45 unions, but I think it improbable that this is correct, as several of the marriages were very prolific, and as I have reason to think that in some cases my information related only to the defective.

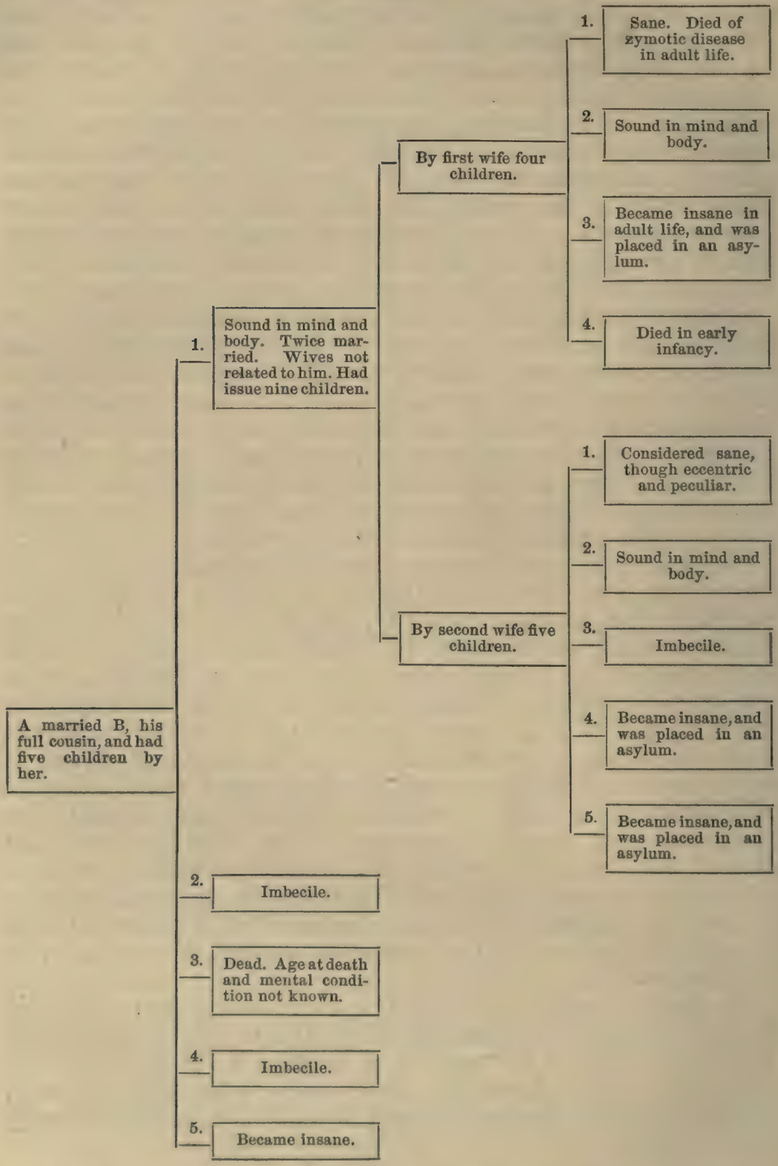
Nothing would more certainly be unsound than deductions from these figures. Without intention, they are actually selected cases, and it would be a pure accident if they were found to embody *the* *rule.* I am certain that I could easily find in Scotland 45 marriages, where no kinship existed, which would exhibit in the offspring even a sadder picture. Though it is just possible that in the particular instances before us the unsound condition of the children was largely due to the consanguinity of their parents, yet it is quite certain that from such data alone we cannot correctly determine the measure of the evil influence which such consanguinity exercises on the average. By a selection of cases a false power might, in like manner, be given to any of the other causes of such calamities. There, however, the selection would be forced, *and the forcing apparent;* while here it is natural, unacknowledged, and undesignedly made, but not the less real on that account.

There is another kind of evidence which has been largely used in discussions on this subject, and which is somewhat allied to that of which I have been speaking, though even more likely than it to lead to error. Startling illustrations of calamitous sequences to cousin-marriages have been detailed, and pointed at with a finger of warning, *the relation of cause and effect being assumed.* Such a *relation* may have existed, but it is equally possible that it may not. It must always be remembered that a blood-alliance between the parents is not *the only cause* of defective offspring.

Supposing the proof complete that it is *a cause*, it is still only one of many, and we cannot therefore point with confidence to a particular case, and say positively, that the calamity there is due to consanguinity of parentage, for it may really be due to injuries in parturition, to hooping-cough, to a blow on the head, or to starvation in infancy. Consanguinity in the parents *may* very decidedly *tend* to injure the offspring, yet it by no means follows that every defect in the children born of blood-related parents is an expression of this tendency, for the general causes of defect will exist among them as among other children, and will give results at least equally disastrous. It is clear, therefore, that isolated cases cannot be used in this or in any similar question to indicate the measure of the evil which may be expected, nor even *to prove its existence.* The minute examination of individual cases is far from valueless, but they cannot be adduced to teach the rule in this matter.

In the 45 cases already alluded to, there occur some of as startling a character, I think, as any which I have ever seen recorded. It would be difficult, for instance, to imagine a greater intensification of one form of mischief than occurs in the following case, which I represent diagrammatically in order to make it more easily understood :—

CASE I.

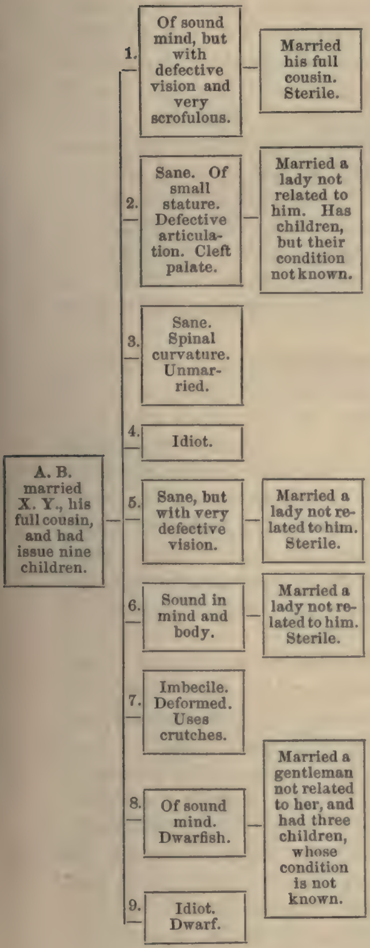


A married B, his full cousin, and had five children by her.

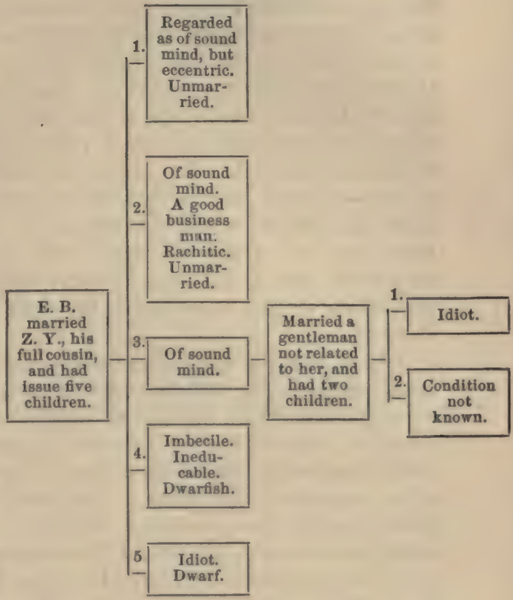
- 1. Sound in mind and body. Twice married. Wives not related to him. Had issue nine children.
    - By first wife four children.
        - 1. Sane. Died of zymotic disease in adult life.
        - 2. Sound in mind and body.
        - 3. Became insane in adult life, and was placed in an asylum.
        - 4. Died in early infancy.
    - By second wife five children.
        - 1. Considered sane, though eccentric and peculiar.
        - 2. Sound in mind and body.
        - 3. Imbecile.
        - 4. Became insane, and was placed in an asylum.
        - 5. Became insane, and was placed in an asylum.
- 2. Imbecile.
- 3. Dead. Age at death and mental condition not known.
- 4. Imbecile.
- 5. Became insane.

If it be possible to conceive a family history more melancholy than that presented in the foregoing diagram, we shall find it in the cases which follow.

CASE II.


A. B. married X. Y., his full cousin, and had issue nine children.
1. Of sound mind, but with defective vision and very scrofulous.
Married his full cousin. Sterile.
2. Sane. Of small stature. Defective articulation. Cleft palate.
Married a lady not related to him. Has children, but their condition not known.
3. Sane. Spinal curvature. Unmarried.
4. Idiot.
5. Sane, but with very defective vision.
Married a lady not related to him. Sterile.
6. Sound in mind and body.
Married a lady not related to him. Sterile.
7. Imbecile. Deformed. Uses crutches.
8. Of sound mind. Dwarfish.
Married a gentleman not related to her, and had three children, whose condition is not known.
9. Idiot. Dwarf.

CASE III.


E. B. married Z. Y., his full cousin, and had issue five children.
1. Regarded as of sound mind, but eccentric. Unmarried.
2. Of sound mind. A good business man. Rachitic. Unmarried.
3. Of sound mind.
Married a gentleman not related to her, and had two children.
1. Idiot.
2. Condition not known.
4. Imbecile. Ineducable. Dwarfish.
5 Idiot. Dwarf.

As differing somewhat from the foregoing, but still revealing a great amount of family misfortune, I subjoin the history of other two unions of consanguinity.

CASE IV.—M. married F., his cousin, and had issue five children, of whom one was sane, one was paralytic, one was lame, and two were idiotic.

CASE V.—M. married F., his cousin, and had issue ten children, of whom two were sane and arrived at maturity, one was an idiot, one was an imbecile, one was deaf and dumb, and five died in early infancy.

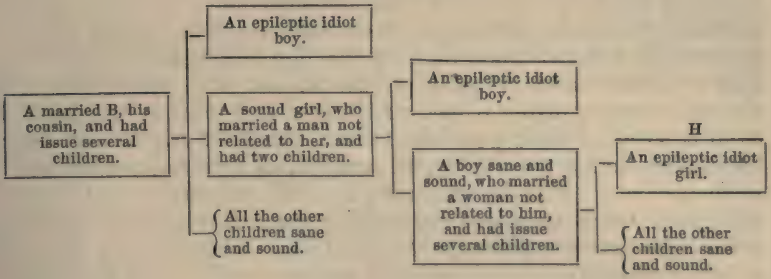
When I resolved to attempt the investigation of this subject, I felt a perfect indifference as to whether I should be led to the conclusion that a blood-relationship between the parents did *much*, or *little*, or *no* injury to the offspring, and I have endeavoured to conduct the inquiry without prejudice. The result is, that I have convinced myself that it does injure the offspring, and I shall by-and-by detail, as fairly and fully as I can, all the facts on which this conviction is founded. My object in now stating the general conclusion at which I have arrived is to enable me to make the foregoing cases the text of some comments, by the introduction of which at this stage of my paper I shall avoid iteration. Besides, it does not appear to me that it would serve any good purpose to avoid the indication of this conclusion till after the facts are stated on which it rests.

I have said already, that it would be unphilosophical to found a belief even in the existence of an evil done to children by a bloodship between their parents on such evidence as the detail of a few startling cases like the five which precede, and that it would be still more unphilosophical to look to such evidence for the teaching of a rule in the question. For, in actual fact, we know that however viewed, these are most exceptional cases; and, what is more, we also know that it would be easy to set off against them cases quite as deplorable in their character, where the most careful inquiry has failed in detecting any kinship among the progenitors of the defective children for generations back.

Yet, this minute examination of individual cases has its use. For instance, it leads us to suspect that the evil may sometimes not manifest itself in the immediate offspring of such unions, yet may do so in the grandchildren and great-grandchildren. Cases I., II., and III., all appear to show this, and it has been observed in scores of other instances. Sometimes, indeed, a defect has been found to occur, which at first sight appeared purely due to hereditary transmission, or to be altogether without explanation in the history of the ascendants, yet which, on a close investigation, presented itself as the possible transmission or manifestation of an evil which had originated in a union of kinship. There is thus reason to suspect that in the seemingly sound children of blood-related parents a *potential* defect may exist and may eventually manifest itself as *actual* defect in their offspring. If this be real, we are led to the inference, that even the most careful estimate of the measure of the evil under examination may prove erroneous, since defects originating in these marriages may appear eventually in society as hereditary tendencies, and it may scarcely be possible, in a particular instance, to speak otherwise of the defect than as a manifestation of an inherited proclivity.

The following case, perhaps, better illustrates these remarks than those I have already given. I must beg, however, that it be remembered that it is given merely in illustration, and not as proof. Absolute, stateable proof of such views can scarcely be given, yet they may be fair and sound conclusions from actual observation in a wide field—of such probable accuracy, at least, as to make it prudent in us to shape our conduct by them.

CASE VI.


A married B, his cousin, and had issue several children.
An epileptic idiot boy.
A sound girl, who married a man not related to her, and had two children.
All the other children sane and sound.
An epileptic idiot boy.
A boy sane and sound, who married a woman not related to him, and had issue several children.
H
An epileptic idiot girl.
All the other children sane and sound.

The parents of the epileptic idiot girl H were not blood-related, nor were her grandparents, but her great-grandparents were cousins, and in their children, grandchildren, and great-grandchildren, epilepsy had appeared, though it had never done so in any of the collateral branches of their family.

It occurs to me here to remark, that a single form of defect often occurs either in one generation, or in successive generations. This form may be epileptic idiocy, as in the last case, or uncomplicated mental disease, as in Case I., or deaf-mutism, blindness, deformity, club-foot, hare-lip, etc., as in many other cases which have come under observation, but which it would be impossible here to detail. Cases II., III., and IV., on the other hand, illustrate the combination of mental and physical defects, and afford an interesting manifestation of sterility. It is, of course, more usual to have these mixed results, than to find the defect confined to one form. In Case V. we have idiocy, imbecility, deaf-dumbness, and lameness, combined with a large infant mortality. With regard to this last, there is a very general belief among the common people of Scotland, that the children of cousins are weakly, and of low viability, and my notes contain numerous cases which would support this. For instance, I have recorded of one cousin-union that there were ten children, of whom five died in early infancy. I saw all the survivors, and regarded them as sound and healthy; so that nothing but a tendency to defective viability appeared to manifest itself in the offspring in this case, though, of course, it is possible that among those who perished so early there may have been defects both of mind and body which were not recognised.

Cases in which two brothers have married two sisters, their cousins, are not uncommon; and, it has been said, that certain families appear almost to exhibit a tendency to cousin-marriages. From this it has been suspected that there exists in such persons some exceptional character or condition which is passed on directly to their children, and that thus it is a mistake to speak of defects among these latter as due to the kinship between their parents. In the majority of cases, however, it would be no mistake so to speak of them; for, admitting the existence of some such family peculiarity as that spoken of, there could be no better way of securing its intensification than by effecting unions between cousins, *both of whom* would probably possess it, and who would thus run the risk of transmitting it to their children in a form so exaggerated as to be positive disease or defect there, though it might have been little other than a peculiarity, or, perhaps, an agreeable eccentricity, in themselves. If it be granted that such marriages tend thus to exaggerate such peculiarities, nothing more is asked, for nothing more is necessary to show that *they do harm.* The practical question is this,—should they or should they not be avoided? and the answer we are led to give is that they should, for the reason that they tend to injure the offspring. It matters not, practically, whether this injurious influence is the result of some mysterious effect, intrinsic in the *consanguinity* itself, or merely the result of this, that consanguinity increases the risks of finding undesirable or morbid peculiarities transmitted from parents to children in an intensified and dangerous form. If the results are disastrous, they will not be less so on the one theory than on the other, and the lesson will be the same. If relations by blood are liable to possess the same morbid tendencies, and if by pairing among themselves for procreation they are likely to transmit these tendencies in a dangerously increased form to their children, then it is surely their duty to avoid such unions, and to seek among strangers alliances with individuals *more likely* to possess qualities calculated to modify or counteract the morbid predispositions in question. It may be that there is absolutely nothing whatever in the bare fact of consanguinity, and that a marriage of kinship should be avoided on the same grounds as a marriage between any man and woman *both* predisposed say to insanity. In the case of cousins, though there may be nothing common to them of so marked a character as a declared tendency to insanity, still there may be common to them any one of a hundred transmissible peculiarities, which it would be most undesirable to send down to their children in an exaggerated form. Even a strong temperament common to both might thus be intensified into disease in their offspring. It follows, therefore,—as the chances of possessing similar peculiarities are great among relatives, and as intermarriage tends to give a dangerous strength to these in the offspring,—that to avoid such risks the prudent will avoid such unions as appear to increase them.

Families and knots of the community among whom marriages of kinship prevail may be found to be so circumstanced otherwise as to favour the development of morbid peculiarities, but if the blood-marriages are in practice found to strengthen these peculiarities in the generations which succeed each other, then they do harm and should be discouraged, whether the mere fact of consanguinity has anything to do with the result or not. This remains an open question, though, I think, many will be ready to go the length of admitting that—Given a man and woman not cousins both predisposed say to x, and a man and woman similarly predisposed who are cousins, the chances of transmitting x to the offspring will be greater in the last case than in the first; in other words, that the kinship will not be altogether silent.

If the general views just expressed be correct, it follows as possible that in a particular case the relationship of the parents may do no injury to the offspring. In other words, children of as great perfection may be born of parents who are cousins as of parents who are not. A man may nowhere find a better wife than in one related to him by blood—better I mean for his offspring. Facts, I think, bear this out. The explanation lies in this, that there may be full cousinship of blood without strong cousinship of quality. The two things do not *necessarily* go together; so that a man may possibly find in his own relative the very qualities best fitted to tone down or neutralize in his children those strong transmissible peculiarities in himself, which it would be undesirable to have exaggerated in them—exaggerated perhaps into acknowledged defect or disease.

But while all this is possible (and I believe it to be true of several cases well known to myself), it must be remembered that the chances are the other way; and for this reason unions of kinship should be avoided. When we deal with large numbers, I think it can be proved that this is the safer and more prudent course, and the best for society.

It by no means follows from what I have said that a man should desire in the mother of his children only qualities *extremely* opposed to those which he himself manifests. There is a *beatissimum medium* in this matter as in all others. In point of fact it is often to a slight intensification in the offspring of certain qualities of body or mind, possessed equally by both parents, that we owe those salient physical or mental characteristics which impart a power of achieving great things. In a later part of my paper I shall have more to say on this point. In the meantime I shall proceed to detail and examine those facts on which I rest my conclusion, that consanguinity of parentage does tend to injure the offspring. The views I have briefly propounded as to the *modus operandi* may be sound or not, but the facts remain, and the inference I have drawn from them appears to be fair and well founded.

I have already stated that, in order to be able to determine the existence and measure of an evil resulting to children from consanguinity in their parents, it is necessary that we should have evidence of a more satisfactory character than any to which reference has yet been made. In the detail of startling cases, or in the grouping of cases furnished from the memory of the collector and from that of his friends, we do not obtain evidence which is likely to lead us to sound conclusions. We must, therefore, turn to some other mode of investigation for a supply of data from which we can draw inferences with greater security and confidence. It appears to me that the following line of inquiry is calculated to meet this demand.

1. Take a large number of instances of any defect which kinship in marriage is alleged to cause in the offspring, and ascertain how many are the issue of parents related to each other by blood, and how many of parents not so related. Either the number must be so large as to preclude the possibility of selection in any form, or it must include as nearly as possible *all* instances of the defect which occur in the section of the community from which they are drawn; but in any case the number should not be small.

The results must then be compared with the proportion of cousin marriages to other marriages in the same community.

Unsoundness of mind is believed to be one of the defects in question, and I accordingly embraced the opportunities afforded by my position for ascertaining with accuracy and precision the history of the parentage of all insane, idiotic, or imbecile persons in a particular district of the country.

Deaf-mutism is generally received as another of these defects, and I have endeavoured to find out what proportion of the deaf-and-dumb in this country are the children of blood-related parents.

After giving the results of these inquiries, I shall briefly state those of similar investigations in other countries.

2. The second mode of investigation consists in taking certain localities and collecting the family history of every marriage among the people there, and then comparing the results of those in which a kinship existed with the results of those in which it did not exist. The falsifying effects of unintentional selection are reduced to a minimum in this line of inquiry, which, if carried out on a large scale, would lead us more certainly to the truth in this matter than any other. But in that case, it would require to embrace a field so wide as to make it impossible for any private individual to undertake the inquiry.[1]

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[1] The question under discussion in this paper has been regarded as one of such interest and importance in France, that last year the following letter was addressed to the various Prefects by the Minister of Agriculture, Commerce, and Public Works:—"Sir,—The question so warmly debated in learned bodies as to the influence of marriages of consanguinity upon the physical aptitudes of the generations which are the result of these, gives quite a special importance to the table which the annual movement of the population should furnish me with respect to the number of marriages. Now, information derived from trustworthy sources authorizes me to believe that these indications are remarkably incomplete as regards marriages between cousins-german. Omissions of

I have succeeded in doing a little, and that little appears to me to have value, though not such a value as I hoped would be the case. I was prepared to encounter difficulties and disappointments, but I did not expect them to be quite so great as they turned out to be.

After personally visiting and examining the places chosen, (when that was possible), I placed a schedule of queries in the hands of willing and competent persons. These queries were numerous and comprehensive, and I aimed at much more than in any case I have accomplished. In many instances the deficiency is such as to make the whole useless, and my labour is lost. Regarding other places, however, I have received much accurate, interesting, and valuable information.

On examining the results I find each place so isolated and separate from the others by individual characteristics as to make grouping or even close comparisons impossible. I shall, therefore, be obliged to state the results of each inquiry in detail, endeavouring to give only what appears to be relevant to my subject. In doing this it is possible that what I have regarded as relevant some may regard as irrelevant matter, but I think it fairer and safer to run the risk of this charge, rather than mislead by omission.

Having given this general description of the mode of investigation I have proposed to myself, I shall now proceed to state the results, and I have first to show what my inquiries have disclosed as to the connexion between unions of kinship and the actual idiocy and deaf-dumbness of our country.

The Number of Idiots and Imbeciles in nine of the Counties of Scotland who are the Offspring of Unions of Consanguinity.

Ever since 1858, when visiting those lunatics in Scotland who live in private dwellings, the relationship of the parents has been a thing generally inquired into. It was not, however, invariably done, and when difficulties occurred, as frequently happened, about obtaining definite information, no serious effort was made to overcome these. For nine counties, however, inspected during part of 1860 and 1861, I made careful inquiry *in every case*, exerting myself to the utmost to obtain the information desired. Notwithstanding this I often failed. Sometimes where the information was possessed, it was not given; but much more frequently it was not given, because not possessed. At times the great age of the patient

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this kind are very easily explained when we bear in mind that the marriages in question not being, as are those contracted between brothers-in-law and sisters-in-law, uncles and nieces, aunts and nephews, the object of legal prohibition, the local authorities have no regular means of recognising them. I beg of you, then, to issue special instructions inviting the mayors to make direct inquiries in the case of all future marriages, when the papers laid before them do not contain the necessary information whether the parties are related in the degree of cousins-german or even of cousins the issue of cousins-german."

explained this; at other times he had been born in a different part of the country, or even in England or Ireland, and both parents had long been dead; at times, again, neither of the idiot's parents was known to his guardians, or one might be well known, and nothing known of the other. It must be remembered that the idiot himself could in no case be my informant.

The counties to which I have referred were Aberdeeen, Bute, Clackmannan, Fife, Kincardine, Kinross, Perth, Ross and Cromarty, and Wigtown. They represent a population of 716,210, and embrace 299 parishes, and form a considerable proportion of Scotland. The result of my investigation is as follows:—

The whole number of idiots examined was 711, including those in receipt as well as those not in receipt of parochial aid. Of these, 421 were ascertained to be the children of parents not related by blood, and 98 were the offspring of parents between whom there was a more or less close kinship. In 84 instances the relationship was not known, and 108 of the whole number were born out of wedlock. In a tabular form the results stand thus:—

(1.) Whole number of idiots and imbeciles examined		711
(2.) Of these—illegitimate	108	
... parentage not known	84	
		192
(3.) Total number whose parentage was known		519
Of these—parents not related	421	
... parents related	98	
		519

Taking the whole number of idiots examined, including both the illegitimate and those of whose parentage I could learn nothing, we have 13·6 per cent. of the entire number born of parents between whom there was a blood-relationship. In order to believe that such relationship does not influence the amount of idiocy, marriages of kinship would require in these counties to be to other marriages at least in the ratio of 1 to 7, which they notoriously are not, though unfortunately no facts exist to show precisely their relative frequency. I think, however, that it may be regarded as certain that such a ratio is at least ten times higher than the reality.

But in order properly to test this influence of consanguinity, we must at least deduct the cases of whose parentage I could obtain no information. Those acquainted with the difficulties of such investigations will admit that the number of these is not great. This deduction then being made (711 − 84 = 627), the proportion rises at once to 15·6 per cent. This last may be regarded as referring to the whole community, since there is no reason for supposing that among the 84 of whose parentage nothing was ascertained a greatly different proportion would be found to be the offspring of blood alliances.

It may appear to some that a further deduction should be made.

The paternity of the illegitimate is practically an unknown thing, and I have elsewhere shown that illegitimacy itself tends to produce defective children. The illegitimate idiots should, therefore, be deducted, *so that those idiots born in marriage of parents related by blood may be compared with those born in marriage of parents not so related.* If this be done it will be found that the former constitute 23·2 per cent. of the latter. Instead, therefore, of every seventh or eighth marriage in the community, we should require every fifth, to be between persons related by blood to each other, in order to show that consanguinity of parentage does not influence the amount of idiocy.

Of the 98 idiots whose parents were related, the degree of relationship was as follows :—

Cousins in	42 cases.
Second cousins in . .	35 „
Third cousins in . .	21 „
	98 cases.

It is probable that more second and third cousins intermarry than cousins, yet these last produce a larger number of idiots. The closer, in short, that the alliance is, the greater appears to be the danger. This, at least, is the teaching of the statement just made.

During the course of these investigations, 64 cases came to my knowledge in which more than one idiot existed in the family. In all of these but 5, I obtained the history of the parents. In the remaining 59, no less than 26 instances of blood-relationship occurred, or 44 per cent. This is a startling and instructive fact. When we select cases in which the tendency to idiocy appears with force, then kinship of parentage presents itself with a marked increase of frequency. Thus, while it appears that in nearly 1 out of every 2 cases in which more than one idiot occurs in a family, consanguinity of parentage is found ; in those cases, on the other hand, where only one idiot occurs, such relationship only exists in 1 out of 5 or 6 cases. The exact results of this part of the inquiry I give below.

1*st*, OF PARENTS RELATED.

	Degree of relationship.	
12 Cases with 2	5 Cousins, ... 6 Second cousins, . . . 1 Third cousins, ...	giving 24 idiot children.
10 Cases with 3	8 Cousins, ... 1 Second cousins, . . . 1 Third cousins, ...	„ 30 „
2 Cases with 4	0 Cousins, ... 1 Second cousins, . . . 1 Third cousins, ...	„ 8 „
1 Case with 5	1 Third cousins, . . .	„ 5 „
1 Case with 7	1 Third cousins, . . .	„ 7 „
26		74

2*d*, Of Parents not related.

24 Cases with 2, giving	48
8 Cases with 3, „	24
1 Case with 4, „	4
33	76

The whole 59 cases, therefore, give 150 idiotic children, but 26 of them give 74 of the whole, or within two of the number yielded by the 33 cases where no relationship existed, affording still another evidence that unions of kinship influence the amount of idiocy in our country.

(*To be continued.*)

Article III.—*Notes on the Early History of Midwifery.* By Robert Finlay, M.D.

(*Read before the Obstetrical Society*, 21*st December* 1864.)

Many would seem to be of opinion that the study of the early history of Medicine is comparatively useless, that any time devoted to it is lost, and that the interests of science are best consulted by those who devote themselves exclusively to its present advancement, without ever casting a glance backwards to ascertain the nature and extent of the progress that has been made. And this extreme is doubtless a thousand times better than the other, by which the mind of Europe was so long held in thraldom; when men could see nothing in physics or in metaphysics but what Aristotle had taught, and when the portraits of disease preserved in the writings of the ancients were studied to the exclusion of the originals from which they had been drawn. But I am persuaded that the truth lies between the two, and that the study of the ancient writers on medicine is calculated to yield instruction as well as pleasure. Of course no one would dream of wasting time over the minutiæ of the Chinese and Indian systems of medicine, their fanciful physiology, their endless catalogues of imaginary diseases, and their cumbrous materia medica. But when we find a system of medicine like that of the ancient Greeks, founded on definite principles, carried out under the guidance of common sense, it is surely worth while to inquire what results were actually obtained; what checked the farther progress of the science or led it astray; and how these obstacles have in modern times been surmounted. Perhaps the one field in which the medical writers of antiquity have most to fear from a comparison with those of the present day is that of midwifery, since, from the operation of causes to which I shall draw attention, its progress was far slower than that of surgery or medicine. Yet this very circumstance may render its study in some respects more valuable, inasmuch as a fuller knowledge of the conditions that retarded progress may lessen the danger of a relapse. Often, too, in turning over the pages of the earlier writers on obstetrics, we come upon traces of principles and of operations long forgotten and revived in modern times as new. I venture to think, therefore, that a sketch of the ancient history of midwifery may not be altogether uninteresting.

In the long period from the days of the early Greeks to the time when the arts and sciences became involved in the great fall of the Roman Empire, three epochs more especially invite attention,—the latter part of the fifth and the beginning of the fourth century B.C.; the first years of the Christian era; and the end of the fifth with the earlier years of the sixth century after Christ. These are represented respectively by the Hippocratic writings, by those of Celsus, and by the medical encyclopædia compiled by Aetius of Amida. I propose to set before the reader in a collected form the information on obstetric matters that is at present scattered through the works of these various authors.

All that we know about the authorship of the greater part of the writings bearing on obstetric matters that pass under the name of Hippocrates is that they were not written by him; but it has with great probability been conjectured that the most important of them proceed from the rival school of Cnidus.[1] They were written probably in the last years of the fifth century B.C., and contain much that enables us to form an idea of the state in which midwifery was at that time. But we must not suppose that we are here presented with a view of all that was known on this subject at that date. The greater part of the medical literature of those days is lost, and what remains is but a sample of the whole. But we may attain to a tolerably accurate conception of the state of knowledge with regard to midwifery by bringing together the doctrines scattered through a number of treatises in the Hippocratic collection, with the aid of a few illustrative cases from some books of the Epidemics.

One of the earliest of the innumerable conjectures made as to the powers at work in conception is set before us in the treatise *De Semine*. It is there supposed that the seminal fluid is supplied by the female as well as the male, that it is derived from the whole body and from each of its various parts, and that in either sex it contains a male and a female element. By variously combining and arranging the materials supplied by this hypothesis, the writer proceeds to explain at length how the sex of the child is determined, how it is that the child resembles one parent more than another, and how the defects of the parent are transmitted to the offspring. The conception of twins is explained by the assumption that conception in these cases takes place separately in two of the pouches into which the uterus was supposed to be divided, as it is in many of the lower animals,—for these alone were dissected to obtain information on such points; and we shall find that the midwifery of the ancients was all along obstructed by mistaken ideas arising from this source.

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[1] Littré's Hippocrates, vol. viii. p. 6.

The relation borne by the fœtus *in utero* to its mother is compared[1] to that of a plant to the earth in which it grows. It was believed that the different parts of the body might be distinguished in the male after thirty, and in the female after forty days,—that the former lay upon the right, and the latter on the left side of the body, and that the greater robustness of boys as compared with girls was due to the fact that the right was the warmer side of the two. This alleged difference of position was held for many centuries to afford the means of solving the problem whether it was a boy or a girl that a woman was pregnant with. Other means of answering this question are given in the Hippocratic writings. If the child is a male, the complexion, it is then said, continues good, and the nipples are pointed somewhat upwards, while, if it be a girl, the face becomes freckled and the nipples droop. Some still more curious expedients are suggested,[2] but it is hardly worth while to transcribe the account of them. The lengthy discussions, too, in the treatise on the diseases of women as to the effect that a predominance of bile, of phlegm, or of water in the blood of a pregnant woman has upon her and her offspring possess little interest but as proofs of the strong hold that the humoral pathology had on the minds of men in those days.

Two treatises[3] are devoted to the discussion of the alleged fact that, while children born in the seventh month of pregnancy may live, those whose birth takes place in the eighth are not viable. The explanation was found in the statement that the sixth period of forty days, embracing of course the eighth month, spent by the fœtus *in utero*, is one of unrest and illness, disquieting the mother by the uneasy movements thence arising, and rendering the child incapable, if born at that time, of surviving the shock of birth, the change of temperature, and the new mode of nourishment attending its entrance into the world. On the other hand, these disturbing causes had not come into play at the seventh month, which, it was believed, was characterized by a sudden increase in the rate of growth of the new-born child, making it fit to sustain a separate existence, and often bringing on labour from the pressure exerted by it upon the membranes. Some mention is also made of a change of position *in utero* on the part of the fœtus at this period. We might suppose that this was some allusion to the fact that it is about the seventh month that the head comes to occupy the position in which at the full time we find it. But we find at another place in the same treatise a description given of the way in which just before birth the child suddenly turns in the womb, converting what was a breech presentation into one of the head, and sometimes entangling its neck in the umbilical cord as it sweeps round the uterus.

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[1] De Naturâ Pueri.

[2] De Sterilibus.

[3] De Septimestri Partu, and De Octimestri Partu.

The allusions made to the subject of intra-uterine disease are few. We find the statement that any congenital defect, such as blindness or lameness, is but the result of a greater than usual severity of the disorder of the fœtal system during the eighth month,—and in the fourth book, "Concerning Diseases," the curious assertion that tapeworms always arise in the fœtal state, with a still more curious reason for it, viz., that only before the excretions of after-life have begun is there sufficient repose of the intestinal contents for the production of such creatures. It had also been observed that general dropsy of a pregnant woman, which was attributed to a watery state of the blood, was often followed by the birth of a child either dead or not viable; and in the seventh book of the Epidemics is recorded a case illustrative of this point and not uninteresting in itself.

"The sister of Harpalides was seized in the fourth and fifth months of her pregnancy with general œdema. There was diarrhœa and cough with orthopnœa, so that any little sleep could be got only in the sitting posture; and it was inferred that the child was dead, from the way in which it fell about in the womb according to the movements of the woman. The asthmatic condition lasted for about two months, but was relieved by the use of beans with honey and cumin of Ethiopia. The discharge of a great deal of white and well-digested sputa ensued. The child was female."

The approach of labour at the full time was ascribed to the fact that the system of the mother no longer afforded nourishment enough for the child, which became restless, and brought on labour by pressing on the membranes and finally rupturing them. That gestation could continue longer than the tenth month is denied, and cases of this kind are referred to spurious pregnancy, which was supposed to result from a collection of gas or from an unusual accumulation of blood about the uterus, as a consequence of the suppression of the menstrual discharge.

Very short is the space devoted to the subject of superfœtation in the treatise that goes by that name. No case seems to have been observed in which the second child was viable,—indeed it is asserted that this is impossible, and that, unless the second product of conception speedily follow the first, the woman suffers from pain, with a fetid discharge and fever, attended by swelling of the face, abdomen, and legs, and loss of appetite, these symptoms disappearing only on the expulsion of the ἐπικύημα. Such unpleasant results were observed in the following case, which we find recorded in the fifth book of the Epidemics:—

"In Larissa, the wife of Gorgias did not menstruate, except very slightly, for four years, and on whatever side she lay had a feeling of weight and pulsation in the womb. She became pregnant, and had a superfœtation. In the ninth month there was born a live female child, marked with a wound on the hip. The after-birth followed, and a very great discharge with clots of blood came on, and continued on the second, third, and fourth days. There was fever till the first ten days were over, and during the rest of the time there was a discharge of red blood. Great œdema appeared in the face, in the legs and feet, and in one thigh. There was no appetite, but great thirst, which was assuaged by the coldest water, while wine could not be borne. The abdomen, after the birth of the first child, was, to some slight extent, relaxed, but the swelling did not altogether fall. It was rather hard, but without any pain. On the fortieth day the product of superfœtation came away,—a mole. The abdomen fell, the œdema, the white flux, and the discharge of fetid blood disappeared, and the patient became well."

It may be thought that I have entered rather too much into details with regard to these less practical parts of the obstetric teaching of the Hippocratic writings. But many of the ideas there embodied are, however trivial they may appear in themselves, of importance, as having maintained their place for very many centuries, and as re-appearing from time to time in the works of almost every ancient writer on midwifery.

Matters more directly bearing on the practice of the art now claim attention.

The presentation of the head was regarded as the only one that was attended with any degree of safety to mother and child. Those that were recognised besides were the oblique, the footling, and the breech presentations. The difficulty of the first of these was illustrated by a somewhat homely simile. "If," says the author of the treatise on the Diseases of Women, "you put a fruitstone into a narrow-necked flask, you may find it impossible to bring it out crossways, and even so is it with the child when it lies across the mouth of the womb."

The danger of presentations of the lower extremities was enormously exaggerated. Nothing but the fact that the accoucheur was called in by the midwife only in cases of extreme difficulty, and had thus no opportunity to render himself familiar with the ordinary processes of labour, could have brought into currency the proposition, that for such cases the cephalic version was necessary. They are, it is said, dangerous to the life of the mother or child, or of both; and yet, in the treatise *De Superfœtatione*, we find directions given for removing the head when the rest of the body was born, which would seem to show that the way in which nature manages such cases had not altogether escaped observation. Another cause of difficult labour is mentioned—death of the child, for it was at that time believed that the child was as active in advancing the labour as in bringing it on. This idea long held its ground, and led at a later date to a curious misstatement as to a point of fact. We know that from the greater size of the head of the male, the labour is more severe than with the female child, but it was asserted in these days that the case was reversed, and that with female children the labour was more severe, the reason given being, that boys are stronger and better able to make their way into the world.

The axiom, that a sick labour is a speedy one, was found as true by the ancient Greeks as it is by the nurses of the present day.[1] When, however, the labour was tedious, a variety of means was employed for its facilitation. In all cases the passages were plentifully anointed; specially prepared ointments, supposed to have the effect of quickening the pains, were applied to the os uteri, which was also dilated with the fingers. This operation was performed by the midwife, and she is specially warned that it must be carried out with all gentleness and caution.[2] Fumigations and hip-baths were also freely employed with the view of relaxing the passages, and very many formulæ are given for preparations that were supposed to have the effect of quickening the pains. Here are two of them:[3]—

"Mix the resin of the terebinth tree and honey, with twice as much olive oil and fragrant wine of the sweetest quality. Warm and give to drink rapidly. This will also allay any inflammation of the womb."

Again, "Scrape down the root or the young shoots of laurel, warm and give to drink in water."

In the case of plethoric young women, venesection at the ankle and the withdrawal of a quantity of blood in proportion to the strength of the patient were recommended. When such means failed, other expedients of a more mechanical kind were resorted to. Sternutatories were given, and the mouth and nose held fast when they began to take effect, that the bearing down thus produced might aid the pains, just as nurses now-a-days direct the patient to procure the expulsion of the placenta by blowing into a bottle. If this did not suffice, a still rougher mode of practice was adopted. The woman was laid on her back in bed, while the shoulders and upper part of the body were bound fast. The end of the bed next her head was then raised and allowed to fall with a jerk, which was supposed to aid the expulsion of the fœtus;[4] or, according to the directions given in the treatise, *De exsectione fœtus*, four women seized each an arm or a leg, and thus jerked the patient up and down as she lay in bed. It was even imagined that this succussion might be of use in the case of a presentation requiring turning. In these cases it was carried out with the head lower than the feet, under the belief that the fœtus might be shaken up into the roomier part of the womb, and there turn into the natural position. The effects of this practice would seem, however, to have been dreaded by some, for we find a case given in the seventh book of the

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[1] Prænotiones Coacæ, 536.

[2] De Morbis Muliebribus. [3] Ib. [4] Ib.

Epidemics, in which it was apparently regarded as the cause of the fatal result:—

"The wife of Simus, having undergone succussion in the course of labour, was seized with a pain in the side of the chest. There was cough, with fever and purulent expectoration. Phthisis developed itself. The feverishness lasted six months with continued diarrhœa. The fever and diarrhœa disappeared at the same time, and after seven days the patient died."

The only form of version practised was the cephalic, and it was thought necessary in all cases in which the head did not present, in footling and in breech as well as in cross presentations. The patient was laid on her back, the presenting part, whatever it might be, pushed up and the head brought down. If the child was already dead, and some difficulty was experienced in returning the projecting foot or hand into the uterus, it was at once cut off to facilitate the operation. Should the labour not advance, version having been performed, the surgeon (for in such cases of difficulty the surgeon took the place of the midwife) proceeded to perform the operation of craniotomy. A curved bistoury, protected by the point of the forefinger, was carried up to the head, and the scalp slit up. The skull was then crushed with an instrument termed *πίεστρον*, and the fragments of bone removed with another which bore the appropriate name of *ὀστεόλογος*. Traction was then effected gradually and at intervals, by means of an instrument hooked round the clavicle. The extraction was further facilitated by removing the arms at the shoulder-joints, and by slitting up the breast and dividing the ribs, so that the thorax might collapse. Dismemberment was also in some cases effected by the aid of a small, sharp instrument, termed *ὄνυξ*, which was fixed to the thumb of the operator. As to the means by which it was ascertained that the child was dead before such formidable operations were undertaken, we are left somewhat in the dark. In one place, indeed, it is stated that, if the fœtus is dead, it is felt to fall about in the womb according to the movements of the mother.

Of the use of the forceps there is at this time not a trace. When the head was long in the birth, the operator was directed to pass the hand up and, inserting a finger into the mouth of the child, aid the pains by traction.

When the placenta was retained, its expulsion was promoted sometimes by the use of sternutatories, and sometimes by tension on the cord. The principle of the latter practice is the same with that in vogue at the present time, but the manner in which it was carried out was quaint and curious in the extreme. As it was not usual to divide the cord until after the placenta had come away (and, indeed, its retention is said to result from too early a division), advantage was taken of this in the process. The infant was laid on the ground, resting upon a couple of skins filled with water, and the mother was seated on a perforated chair at such a height from the ground as to render the cord tense. The skins were then pierced with a needle, so that the pull on the cord was gradually increased as the water slowly escaped. When the child had been separated, but the placenta was still retained, the requisite traction was obtained by attaching weights to the cord.

Little mention is made of hæmorrhage as a complication of labour. We merely find the statement that it is likely to be followed by the death of the child, and in the fifth book of the Epidemics a case of this kind is given:—

"A woman in Larissa suffered much from hæmorrhage during the four days preceding delivery, and more especially the last three. The child was dead, and presented a curious malformation—the right arm being adherent to the side. The placenta was not expelled till the third day. A white discharge succeeded, and was followed by fever for two days, with pain in the haunches and abdomen."

Such was the practice of midwifery in Greece 400 years B.C. The greatest blot upon it is undoubtedly the treatment then pursued in cases of breech and footling presentations. This could arise only from the fact that, in all but the most unfavourable cases, the management was left in the hands of the midwife, and that the operator was thus cut off from the means of attaining a thorough familiarity with the procedures of nature in ordinary circumstances,—the first of all requisites for a judicious interference in case of any abnormality.

Little or nothing is met with calculated to throw light on the farther progress of midwifery, until we reach the first years of the Christian era, as they are represented in the works of Celsus. The chapter devoted by him to obstetric operations is so very clear and satisfactory, that it is greatly to be regretted that its range is so limited. That it is so, must be attributed to the continued operation of the cause to which I have just alluded. The ordinary practice of midwifery was entirely in the hands of women, who generally called in further aid only when the life of the child had been already sacrificed, and that of the mother was in danger. The surgeon was then appealed to: and in Rome there would seem to have been drawn in this matter a sharp line of distinction between his province and that of the physician; for we find Galen expressing his regret that since he had settled in Rome he had been unable to prosecute the study of midwifery, inasmuch as etiquette demanded that its practice should be left to the surgeon. The stage at which the latter was applied to determined the point of view from which he looked at the obstetric portion of his art. The life of the mother alone remained to be saved, and the child was regarded merely as a foreign body, whose presence caused her distress and danger, and which was to be got rid of—no matter in what way. Celsus, in the opening sentence of his chapter upon this subject, expressly informs us that his remarks apply only to cases in which the child was dead. The substance of his teaching is as follows:—

It was judiciously advised that, in all cases in which interference was needed, it should be undertaken before the danger of any operation had been increased by a swollen and inflamed state of the passages. The patient was laid upon her back, with the thighs drawn up, so as to rest upon the belly, and the surgeon introduced his fingers one by one, until the whole hand was within the mouth of the womb. The position of the child was then ascertained—the presentations spoken of being those of the head, of the feet, and of the hand or shoulder. In case of the last, version was practised, either the podalic or cephalic; the latter it was attempted to effect by pulling one of the hands, a sharp hook was then fixed in the bones of the head, and, while by means of it steady traction was made with the right hand, the passage of the body outwards was guided by the left. When, on the other hand, the podalic version had been effected, the child was removed in the ordinary way by traction on the feet. In cases where the child was swollen, from the effusion of the products of putrefaction, its bulk was diminished as far as possible, by piercing the body with the fingers, and thus allowing the fluids to escape; while the hook was not employed, lest it should slip, and injure the soft parts of the mother. In some cases, however, of transverse presentation, it was found impossible to turn. A hook was then inserted into the armpit, and pulled down, so as to bend the head upon the body. The neck was then severed by means of a hook, sharp only on the inner edge, and the head and body were removed separately.

In all cases, as soon as the body of the child had been removed, the surgeon, without dividing the cord, rendered it tense with the left hand, and passing the right along it, brought away the placenta and membranes, with any clots that might have formed.

The most important part of the teaching of Celsus is, without doubt, his admission of the comparative safety of footling presentations, and his recommendation of the podalic version. On both of these points, midwifery had made an enormous stride since the days of Hippocrates, and the reintroduction of the latter was a great step in the more recent history of the art. But, on the whole, the information that we derive from Celsus, on obstetric subjects, is meagre, and interesting chiefly as setting forth clearly the point of view from which the manners of that age had led accoucheurs to regard their art.

We may now proceed to inquire, whether the same obstacles were still in force at the time of the fall of the Roman empire, and can best find an answer in the huge compilation of Aetius of Amida. His work is to be regarded, not as an expression of his own views, but as a collection of the principles of practice that were most in vogue 500 years after the Christian era, and is therefore the more valuable for our purpose.

The account which we there find of the physiology of conception, pregnancy, and parturition is very clear, and in advance of the statements of more ancient authors on these points. After some remarks on the anatomy of the female pelvis, the recommendation is added, that any one who wishes to acquire a fuller knowledge of the subject should apply himself to the dissection of a cow or a she-goat. Here we have one source of fallacy, the same that we have had occasion to remark in connexion with the Hippocratic writings; and another is, that descriptions are occasionally given, not of what had been seen, but of what was thought probable. In the following description we see both of these at work together:—

At the spot where each artery or vein opens on the inside of the uterus, there is, it is stated, a small cup-shaped elevation or cotyledon. After coitus the seminal fluid adheres to the walls of the uterus, and from each of these cotyledons springs a vein or artery, according to the nature of the vessel on which it is situated. These new vessels interlace with one another, and are united by a membranous deposit. The vessels of each kind reunite to form the *two* umbilical veins and the two umbilical arteries. From the junction of the former results a large vein, which runs up to the liver of the fœtus; while the two latter form an artery, which, passing round the bladder, joins a large arterial trunk at the back of the child, thus conveying to it the *spiritus vitalis* necessary for its support. In the midst of these four vessels lies the urachus.

A conjecture of a very mechanical kind had been formed as to the way in which the uterus adapts itself to the increased bulk of its contents. This was thought to be the result of mere physical distention; so that, says our author, the walls of the uterus are thinnest when labour comes on, just like a bladder when it is fully inflated. When this point has been reached, the inmate is straitened for room; the nourishment supplied him is no longer enough for his wants; he becomes restive, and forces his way out.

The accoucheurs of this period still busied themselves with the old problem as to the sex of the child *in utero*. That which is given as the safest and best sign of all bears a direct relation to the belief of the early Greek authors as to the position of the child of either sex in the womb. When the pulse of the right arm is the stronger, a boy may, it is said, be expected, and a girl when it is weaker than that of the left.

When we turn to the more practical parts of the treatise of Aetius, we cannot help seeing, that in spite of all obstacles, essential progress had been made in the obstetric art.

The signs of conception are laid down with as much accuracy as we can pretend to at the present day, and are followed by the rules for the management of pregnant women. These are so judicious, and so superior to what prevailed during the earlier period of the modern history of midwifery, that they may be quoted at some length:—

"Pregnant women, who have lately conceived, are to be preserved from sorrow and every strong emotion of the mind. Riding in chariots, and violent exercise of all kinds are forbidden, with holding the breath, and blows on the hips. The patient must lift no heavy burden, nor leap, nor sit on hard seats. Her food must not be acrid or flatulent. She must employ no acrid clysters, and we allow neither a superfluity nor a deficiency of diet. Any flow of blood from the nose, from hæmorrhoids, or from any other source, is dangerous. She should accordingly use a moderate diet, and one suited to her stomach. She should take the air in a litter; walking, gentle friction, working at wool, are to be recommended; and about the eighth month, which is the most troublesome of all [here again is an allusion to the Hippocratic doctrines], the food is to be abridged, and all violent motion proscribed. If costiveness be caused by the pressure of the uterus on the *rectum*, the food may be such as to open the bowels; for example, barley water with boiled sorrel, mallows, and lettuce. Again, in the ninth month, the bath should be used, to relax the body, and every precaution taken that the woman may be able to bear stoutly the approaching pains of labour."

The treatment of natural labour, says Aetius, need not be described, for, from practice, not only midwives but other women also know very well what is to be done. And we may be surprised that the account given soon afterwards of the causes of difficult labour is so correct, when we read that the surgeon is to inform himself of the state of matters not by personal examination, but by hearing the report of the midwife.

Among the causes of difficult labour on the part of the mother are enumerated,—an oblique position of the cervix uteri—fleshy growths attached to it—inflammation, abscess, or induration—narrowness of the passages, and small size of the womb—a loaded state of the bladder or rectum, a calculus in the former compressing the vagina—weakness of the mother in body or in mind or in both.

On the side of the child are mentioned,—too early a rupture of the membranes, leaving the parts without moisture when they need it most—unusual toughness of the membranes—great size of the child, or of any one part of it—malformations of the same—the simultaneous approach of twins to the *os*, weakness of the child, preventing it from giving any aid by its struggles in the process of delivery—death of the child, recognised by coldness of the scalp and absence of motion.

It was supposed that in the normal process of birth the head came first, while the hands were laid along the thighs. The natural presentation might be marred by displacement of the head, so that one side of it presented, or by the protrusion of a hand along with it. Footling presentations, when both feet appeared at once, were regarded as not unfavourable; but when only one foot was protruded, turning was thought necessary; as also in cases of breech presentations, which were regarded as among the most unfavourable possible. Version was prescribed also for those cases in which the side or the belly presented and the child lay obliquely in the womb. In all cases the surgeon was recommended not to be too ready to interfere—*ne procedat temere ad chirurgiam*—while he yet did not allow the uterus to suffer from prolonged manipulation on the part of the midwife; and whenever there was coma present, or convulsions, or a very full and bounding pulse, interference was altogether declined, lest the death of the patient should bring discredit on the art.

When an operation had been resolved on, the patient was laid on her back, in the position recommended by Celsus, while her strength was kept up by wine. By the use of a sort of speculum, by which the vagina was dilated, the cause of obstruction was ascertained, and any fleshy growth was cut away. When unusual size of the head was the obstacle, the podalic version was performed, a procedure which seems not unlikely again to come into vogue for such cases; but if the impaction was too great to admit of this, a hooked instrument was inserted on either side of the child's head, so as to form a rude sort of forceps, and delivery was effected by traction in the direction of the axes, for the surgeon is directed to pull not only *in directum*, but also *ad obliquum*. Should this procedure also be impracticable, the scalp was laid open with a bistoury, with the view of diminishing its bulk, should it be distended with water;—if not, the surgeon proceeded to break down the bones, and remove them piecemeal with his fingers. Here we have the operation of craniotomy; but, from another passage, that of cephalotripsy would seem also to have been practised, the bulk of the head being diminished by crushing the bones without dividing the skin.:—

"*At si duplicatus fœtus occurrerit et dirigi non poterit, si caput magis expositum fuerit, ossa ejus conteremus, cute non dissectâ—deinde alicui ejus parti attractorium instrumentum infigemus ac attrahemus et cruribus directis educimus.*"

The treatment of retained placenta was substantially the same with that pursued now-a-days. The failure of all efforts to remove it would, however, seem to have been regarded with great indifference. No apprehensions need, it is said, be entertained on that score, as the placenta will in a few days come away of itself in a state of putrefaction. Such at least was the opinion of one of the authors followed by Aetius; but, in another place, when treating of abortion, he lays down that, though no violent measures should after a miscarriage be employed to bring away the placenta, it should on no account be allowed to remain in the uterus, otherwise certain death will be the result. Under the same head is given a curious list of substances that were supposed to have the power of preventing abortion, such as—the stone *aetites* worn on the belly—a grain of corn found in bread bound to the left arm—a stone found in the intestines of the stag. In other circumstances, indeed, abortion was artificially induced. If a pregnant woman suffered from some tumour or malformation of the genital passages, which rendered safe delivery impossible, her life was saved by sacrificing that of the child. Its performance in such circumstances is said by another ancient author to be as justifiable as the conduct of the mariner who in a storm throws his cargo overboard that he may save his ship. Even in cases where there was no such necessity for the operation, abortion would seem in ancient times to have been brought on, even by regularly-trained physicians, to an extent now happily unknown. The very frequency with which the practice is denounced shows how extensively it prevailed. Hippocrates made his pupils swear that they would never give an abortive to any woman; but the author of the treatise *De naturâ pueri* complacently narrates the success of an expedient suggested by him to a female slave whose value would have been diminished by pregnancy. Aristotle speaks of the crime with great indifference, and Juvenal lived in the age,—

Quum tot abortivis fecundam Julia valvam,
Solveret et patruo similes effunderet offas.

When we remember that before the diffusion of Christianity the exposure of infants was not less openly practised, we need not look for an explanation of its prevalence to any theories prevalent among the philosophers of antiquity, as to the right of the unborn child to be called a living being, however much they may have tried in this way to render the practice justifiable in their own eyes.[1] The agents resorted to for the production of abortion were drugs of various kinds and pessaries.

Of another operation, whose object is not to destroy but to save the life of the child, we find traces in antiquity. Even in the early tales of the Greek mythology, mention is made of cases in which the child was delivered after the death of the mother by the Cæsarean section. When Coronis had died by the hand of the jealous Apollo and was laid on the funeral pyre,—

Non tulit in cineres labi sua Phœbus eosdem
Semina, sed natum flammis uteroque parentis
Eripuit geminique tulit Chironis in antrum;

And when Semele had been consumed by the approach of Jove,—

Imperfectus adhuc infans genitricis ab alvo
Eripitur, patrioque tener—si credere dignum est—
Insuitur femori maternaque tempora complet.

In the medical writings of Greece little mention is made of the operation, but in Rome there existed from time immemorial a law which forbade that any woman who had died pregnant should be buried until an attempt had been made by opening her body to save the life of the child.

Pliny speaks of the operation in such circumstances as a matter of course, and says that it was held a happy omen for a child to be so born. This practice continued to prevail during the Middle Ages, and its performance was peremptorily enjoined by the church, with the view of securing to the infants all the benefits that were supposed to flow from baptism.

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[1] For further remarks on this subject, see Siebold, *Versuch einer Geschichte der Geburtshülfe.*

On a general survey of the state of midwifery in ancient times, we cannot but be struck with the enormous progress that has now been made in this department. The result of this progress has been not the complication but the simplification of the art; and it is no less evident to what this happy change is due. Every step has consisted in a closer approximation to the procedure of nature, and has resulted from a more diligent study of what she is competent to effect. But the accoucheurs of antiquity—if we can give that title to the surgeons who were called in by the attending midwives in cases of extreme difficulty—were debarred from this path of improvement. They were summoned only to cases of an abnormal kind, and only when the efforts of nature to surmount that abnormality had failed. Ignorant, therefore, of what nature was every day doing by her unaided efforts, they were disqualified for deciding when operative measures were really demanded, and what procedures were indicated by cases of an analogous nature terminated without any interference whatever. And if the ground that has been gained is not to be again lost, no heed must be given to those injudicious proposals which are from time to time made, that we should of our own accord return to those conditions by which the progress of midwifery was so long retarded.

ARTICLE IV.—*On the Characters of the Expectoration in Cases of Fetid Bronchitis and Gangrene of the Lung.* By ARTHUR GAMGEE, M.D., Assistant to the Professor of Medical Jurisprudence in the University of Edinburgh, late Resident Physician in the Royal Infirmary, and late Senior President of the Royal Medical Society, Edinburgh.

(*Read before the Medico-Chirurgical Society*, 1*st February* 1865.)

NOTWITHSTANDING the impetus which the study of pathological chemistry has of late years received both in our own country and on the continent of Europe, the labours of those who have devoted themselves to its advancement have told us little of the properties of the majority of the animal secretions and excretions in a state of disease. As a result of those labours we possess merely a tolerably complete account of the condition of the urine in disease, and a necessarily much less accurate and complete account of the diseased conditions of the blood.

That the other secretions and excretions deserve the minute attention of the physician, is a matter upon which we can have little doubt; that we may ultimately derive information of an accurate and useful character from a careful examination of the saliva, sweat, sputum, and feces, is, I venture to say, almost certain. In publishing the observations which I lately made in two cases of fetid bronchitis and gangrene of the lung, I am guided very much by the fact that the chemistry of the sputum has hitherto been almost entirely neglected, and that the facts with which we are accurately acquainted are so scanty that any little accession to our knowledge of the subject must be not altogether uninteresting.

Before proceeding to the narration of the cases which form the basis of this communication, I would remark that the point of practical importance which I wish to elucidate more particularly is one concerning the semeiological value of the presence of butyric acid in the expectoration.

In the *Medical Times and Gazette* for May 1857 (page 479), was published the substance of a clinical lecture on fetid bronchitis, delivered by Professor Laycock in the Royal Infirmary of this city. The paper was entitled "Substance of a Clinical Lecture on Fetid Bronchitis, with Discovery of Butyric Acid in the Sputum." This lecture was based upon three cases of chest disease, which had been observed by Dr Laycock, in all of which the expectoration was characterized by great fetor, and which were looked upon by Dr Laycock as examples of that most unsatisfactorily studied disease, fetid bronchitis. The sputum of one of these cases was, at Dr Laycock's request, subjected to chemical investigation by the late Professor Gregory. The odour was, it is stated, found to be due to the presence of methylamine with butyric and acetic acids. In his comments on this case, Dr Laycock remarked that it would formerly have been regarded as an example of pulmonary gangrene, but it resembled in the leading symptoms the class of cases known as fetid bronchitis. He remarked that in fetid bronchitis the odour is not that of putrid flesh, but very characteristic of butyric acid, and the new odorous compounds, the butyrates of ethyl, now used to flavour confectionary. To this discovery of butyric acid in the sputum Dr Laycock attached considerable importance, concluding "that the production in the lungs of the peculiar compound to which the odour of the sputa was due might be referred to some change in the ganglia of the pneumogastric and the sympathetic in connexion with the pulmonary mucous surfaces, of an asthenic character."

Were the presence of butyric acid in the sputum really a distinguishing sign between bronchitis attended with fetid expectoration, and that infinitely more formidable disease gangrene of the lung, too much value could not be attached to the observations of Dr Laycock; for the physician would thereby have acquired a most valuable diagnostic sign, guiding him in his treatment, and especially in his prognosis of a class of cases which, of rare occurrence, are often involved in doubt and obscurity.

During the winter session 1863–64, I was asked by Dr Laycock to examine the sputa of a case of fetid bronchitis. The patient, I was informed, expectorated large quantities of sputum, which, always fetid, assumed at certain periods a somewhat ethereal odour. The sputum which I had the opportunity of examining possessed an intensely fetid odour, or rather it possessed two odours, of which one—rotten cabbage like—was far more repulsive than the other, which was sour.

On distilling the secretion, which had an alkaline reaction, an intensely fetid distillate was obtained, of alkaline reaction, and possessing, in a concentrated form, the odour of the original expectoration. On acidifying the fluid and redistilling it, the distillate retained exactly the odour of the original sputum. When heated with caustic potash and again distilled, it passed over unchanged. By these processes I obtained a perfectly neutral fluid, having exactly the odour of the sputum. What was the nature of the peculiarly fetid substance upon which the odour of the sputum depended? Was it methylamine or a compound of butyric acid? Certainly not, for had it been the former (to which I may say it did not present the slightest resemblance), it would, when subjected to distillation with sulphuric acid, not have passed over; and had it been a compound of butyric acid, the distillation with sulphuric acid must undoubtedly have decomposed it, and caused the appearance of butyric acid.

The fetid substance was most probably a sulphur compound, for the fluid which distilled was subjected to analysis, and found to contain unoxidized sulphur. That it was not a substance readily prone to decomposition was proved by its being unchanged by distillation with acids and alkalies, and by the fact that it was kept in a stoppered bottle for some months, and possessed at last all the freshness of its original fetor. Having so far investigated the characters of the peculiar fetid substance, I directed my attention to the discovery of compounds of butyric acid. On adding dilute sulphuric acid to that portion of the sputum which remained in the retort after the fetid substance had been distilled off, and applying heat, a fluid distilled, which had an acid reaction, and the odour of butyric acid. The resemblance was perceived by several chemical friends, perfectly competent to form an opinion on this point. The fluid thus obtained was neutralized with carbonate of soda, and cautiously evaporated to dryness. The residue was placed in a small retort, acidified with sulphuric acid and heated, when a fluid was obtained still more closely resembling in smell pure butyric acid. I repeated the process of purification, then neutralized the fluid with chalk, and obtained, on evaporation, a solid substance having the physical appearance of butyrate of lime, and which, when examined under the microscope, was found to consist of crystals resembling exactly in character crystals of pure butyrate of lime with which they were compared. The sputum was in this way shown to contain butyric acid in a state of combination, in addition to the peculiarly fetid organic substance upon which the odour has been shown to have depended.

Having satisfied myself of the presence of butyric acid in the above case, I determined, when opportunities occurred, to examine the expectoration in other cases of chest affection, and more especially cases characterized by fetid expectoration. During the summer recess a most interesting case of gangrene of the lung occurred in Ward VII. of the Royal Infirmary, under the care of Dr Haldane. Through that gentleman's kindness I had the opportunity of investigating with considerable care this case, of which the following is a detailed account:—

T. T., æt. 67, a widower, residing in Leith, was admitted into the Royal Infirmary on the 23d of August 1864.

Until six weeks before the date of his admission the patient had enjoyed the most perfect health. Of originally strong constitution and temperate habits, he had been able to pursue his duties as a labourer in the works of the Shotts Iron Company for twenty years, without a single day's absence.

At the period already alluded to the patient quite suddenly commenced to suffer from swelling of the legs and feet, which compelled him to stop at home. A week later he was seized quite suddenly with intense pain in the lateral thoracic regions on both sides, and simultaneously a cough made its appearance. The cough after some days increased, and was accompanied by excessively profuse expectoration of a black colour and horrible fetor.

On the 1st of September, the sputum of this patient was sent me for analysis. It was of a greyish colour, and possessed a very fetid odour, reminding me very strongly of the sputum of the case just narrated; it also possessed a very decidedly sour odour. Its reaction was acid. When placed in water it was found to float very readily. When distilled, there passed into the receiver a fluid possessed both of the fetid and sour smells of the original sputum. On neutralizing this fluid with carbonate of soda, and re-distilling it, I obtained the more fetid of the two smells, quite free from the sour odour. On now acidifying with sulphuric acid, and again distilling, I obtained a distillate of neutral reaction, possessed of the peculiar fetor of the original sputum, and destitute of acid odour. It was almost identical in smell with the fluid obtained from the sputum of Dr Laycock's case, and its chemical deportment was the same. I found in this, as in the former case, that the stinking distillate contained unoxidized sulphur.[1]

The original distillate from which the fetid substance had been separated, and which had been neutralized with carbonate of soda, was now acidified with sulphuric acid and distilled; as a result, a distillate was obtained which had an acid reaction, and possessed strongly the odour of butyric acid. It was found not to reduce the salts of silver, and not to be coloured red by perchloride of iron, proving that it contained neither formic nor acetic acids. Another portion of the sputum was evaporated to dryness, treated with an alcoholic solution of oxalic acid, and the solution thus obtained filtered. Oxide of lead was added to the fluid, which was again filtered, treated with sulphuretted hydrogen, heated to drive off the excess of gas, and then boiled with oxide of zinc. On filtering and evaporating the solution, a yellowish residue was obtained, which was found to contain, especially at its edges, numerous very characteristic crystals of lactate of zinc, showing that lactic acid must have existed in the expectoration.

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[1] Precautions had been taken to ascertain that the fluid contained no traces of sulphuretted hydrogen.

On the 3d of September I again examined the expectoration of this patient. The total quantity of expectoration in twenty-four hours amounted to 1018 grains. This was of acid reaction, and was distilled to dryness in the water bath. The distillate was extremely fetid and decidedly acid. The total amount of free acid in the distillate was determined by means of a standard solution of caustic soda. Assuming all the free acid to have been butyric acid, the quantity present in the sputum was ·067 gramme (= ·10338 grain). The fetid substance was found to possess the same characters as when first analyzed. Besides the free butyric acid, the residue in the retort contained this acid in combination with bases, for, on acidifying the residue with sulphuric acid, an additional quantity of acid distillate was obtained. In order to separate it completely the process was conducted in a chloride of zinc bath.

The residue in the retort was dissolved in alcohol and filtered. The filtrate was treated with neutral acetate of lead, and the fluid filtered from the abundant white precipitate which fell. A current of sulphuretted hydrogen was passed through it, and after being heated and filtered, it was tested with Fehling's solution and found to contain considerable quantities of glucose.

On the 5th of September, I again examined the sputum. The quantity which the patient expectorated in twenty-three hours amounted to 245·2 grammes (8·64 ounces). It was of acid reaction. When examined microscopically it was found to contain mucus and pus cells and epithelium. Without detailing the lengthened examination which I made of it, the results may be stated as follows:—It contained water, free butyric acid, butyric acid in combination with bases, the peculiar fetid compound which I have described, a little fat, albumen, mucin, some glucose, undetermined organic matters, and salts.

Water and volatile constituents amounted in 100 parts to	88·412
Substances soluble in ether	·297
" " alcohol	3·29
Mucin and albumen	5·66
Salts	1·015
Undetermined organic matter, and loss	1·326
	100·000

I was, in addition, enabled to separate from this large quantity of sputum enough butyric acid to determine the atomic weight of the acid. I obtained a baryta salt of the acid, which on analysis yielded 49·63 per cent. of baryta, instead of 49·23, the theoretical amount. In this case I have therefore been able to prove beyond dispute the presence of butyric acid in the sputum.

On the 6th of September, I examined the patient for the first time. He then appeared wan and emaciated, and was troubled with a cough, and tolerably abundant expectoration. He complained of pain over the lower lateral thoracic region on the left side, and over the lower part of the left back. Over this region there was dulness on percussion. The percussion note was normal in the other thoracic regions. On auscultation, loud bronchitic rales were heard all over the chest. Over the region of dulness on the left side, the breath sound was more bronchial than on the right, and there was some degree of bronchophony. The expectoration was abundant. The urine was examined and found to be quite normal.

From this time the patient became progressively weaker; night-sweats made their appearance, and coincidently the physical signs underwent a modification. On the 15th of September, a certain degree of dulness was found over the lower right back; and on the left side, in a line with the lower angle of the scapula, and between it and the spine, the breath sound became intensely tubular —almost cavernous. The voice sound had the characters of true pectoriloquy. On causing the patient to cough, distinct gurgling was heard. During the period intervening from the date of the last analysis I continued to examine the sputa, both microscopically and chemically. On suspending and agitating in water some of the substance expectorated on the 12th of September, I noticed a considerable number of fibrinous bodies, which presented the appearance of casts of the bronchial tubes. I, at first, supposed them to be portions of expectorated lung tissue,—a supposition which, however, was proved to be erroneous by Dr Haldane, who unhesitatingly pronounced them to have the characters of fibrinous bronchial casts. I found that they possessed one very remarkable character: iodine colouring them of a purple colour;—the reaction produced by iodine alone resembling the reaction obtained under the most favourable circumstances, by acting with iodine and sulphuric acid, on organs affected with amyloid degeneration. The very central portions of the casts possessed this peculiar property of being coloured purple by iodine.

The patient continued to sink rapidly, and ultimately died on the 27th of September.

The body was examined on the succeeding day by Dr Stewart, who has kindly given me the following notes of the sectio:[1]—

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[1] Before the post-mortem examination was commenced, I specially requested Dr Stewart to search for bronchial casts, and to notice the mode in which they reacted with iodine.

"The body was much emaciated. The pericardium contained a small amount of clear fluid. The heart was soft, and the tissues appeared somewhat fatty. The valves were natural. The right pleura was slightly adherent at the apex and the base; the left pleura was adherent throughout, loosely in front, and more firmly behind. Near the lower border, posteriorly, there was a collection of fetid pus about three inches long by two broad, of a depth which could not be ascertained. This communicated with a large gangrenous abscess situated in the lower lobe of the lung. This abscess contained numerous shreds of broken-down tissue, and a large quantity of very fetid pus. On its inner surface there projected numerous knobs and processes like the columnæ carneæ of the heart. These consisted of bronchial tubes and vessels, whose walls were thickened and inflamed, and cavities occluded by fibrinous coagula in different stages of transformation. Many of the arteries and veins in the neighbourhood of the abscess contained purulent looking matter, and in one of the main branches of the pulmonary artery there was a firm old coagulum with processes passing into different branches; close to the coagulum was a large bronchial gland suppurating. The upper part of the lung was œdematous, as was also the right lung. There was a solid, nearly pneumonic, condition of the lower lobe of the right lung; many of its bronchi contained casts. The liver, spleen, and intestines were natural; some of the renal tubes contained exudation, and there was a good deal of fat in the epithelial cells. The renal arteries were more atheromatous than any others examined. The other organs were natural. The brain was not examined. Some of the superficial veins, particularly the right cephalic, contained firm old clots. On examining microscopically some of the bronchial casts from the right lung, numerous mucus corpuscles and epithelial cells were observed, and amongst them there were many groups of cells and amorphous masses which assumed, on the addition of iodine, a purple colour. This fact had been observed during life by Dr Arthur Gamgee, and confirmed by Dr Haldane. The margin of the left lung was emphysematous. That lung also contained one mass of cheesy tubercle about the size of a plumstone. There was no recent tubercle, nor any trace of it, at either apex."

The case which I have just described possesses, it appears to me, many points of the greatest interest. The physical signs possessed, undoubtedly, no remarkable complexity; they pointed conclusively to the existence, at first, of extensive consolidation of one of the lungs, and subsequently to the undoubted existence of a cavity. The odour of the sputum and the breath, and the increasing prostration, left little doubt that this cavity was gangrenous. But what was the origin of this state? was the case one originally of simple pneumonia, terminating (as so few cases do) in abscess of the lung, or was it one of phthisis pulmonalis running its course rapidly? It is my impression that neither of these suppositions is the correct one. The post-mortem examination revealed, it is true, a small mass of cheesy tubercle in the more affected lung, but this was unaccompanied by recent tubercular infiltration. It disclosed, however, a most important pathological condition,—the existence of a firm and old clot in one of the main branches of the pulmonary artery. The hypothesis which, it appears to me, explains better than any other the history of the case, is that the obstruction of the pulmonary artery was of the nature of an embolism, and was the primary cause of all the pathological conditions which followed. Experimentally it has been shown, that by introducing foreign bodies into the large veins going to the heart, embolism of the pulmonary artery is induced, and as a result, often at least, inflammation of the lungs; and clinical records supply us with cases in which, all the circumstances favourable to the occurrence of embolism existing, the pulmonary artery has been occluded, and as a result gangrene of the lung has followed.[1] The very sudden occurrence of the disease in a man of very vigorous constitution bears out the supposition as to the embolic nature of the case.

Amongst the minor points of interest is the remarkable reaction which the fibrinous casts, found in the sputum during life, and in the bronchi after death, presented when treated with iodine—a reaction which had not, I believe, previously been noticed in the case of any animal product. It appeared to me that possibly the protein substance of which the casts were composed might, under the influence of putrefaction, and in the presence of butyric acid, have become so altered as to give with iodine a purple reaction; and I accordingly performed a series of experiments, which consisted in causing coagulated egg albumen and blood fibrin to putrefy at the temperature of the body, and then testing with iodine. In some cases I mixed butyric acid with the albumen or fibrin before causing them to putrefy, and in one experiment I placed coagulated albumen in the putrid fluid obtained from a macerating tub. On testing with iodine, I never observed, however, the purple reaction which was produced by the action of iodine on the casts.

The chief points of interest are connected with the chemical history of the case of gangrene of the lung which I have related, and which proves, I think, in the most conclusive manner, how little characteristic of the disease, fetid bronchitis, is the occurrence of butyric acid, and teaches us how careful we should be before looking upon the occurrence of butyric acid as a manifestation of a vitiated condition of the nervous system. No case could have occurred where the nervous system participated less in the morbid phenomena, or appeared so little to have influenced their production. In all probability a mechanical cause induced the gangrene of the lung, which followed almost as a natural consequence upon it, and amongst other results of putrefaction, a considerable quantity of butyric acid occurred in the sputum. For besides being produced in the decomposition of saccharine matters, and as a further stage of the lactic acid fermentation, butyric acid is developed during the imperfect oxidation of albuminoid matters, and during their putrefaction, especially under such favourable circumstances as to temperature, moisture, etc., as exist in a gangrenous lung.

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[1] See Trousseau, Clinique de l'Hôtel Dieu, vol. i. p. 579.

But besides explaining in this way the occurrence of a large quantity of butyric acid in the sputum, I have to direct attention to a most important fact which I have recently made out, viz., that butyric acid, or one of the homologous volatile fatty acids, is present in the sputum of almost all, if not all, cases of chest disease; in other words, that the sputum always, or nearly always, contains a volatile acid.

On taking the most odourless expectoration of a case of simple acute, or chronic bronchitis, or the mucous expectoration of early phthisis, or the muco-purulent expectoration of the advanced disease, acidifying with sulphuric acid and boiling the fluid, vapours are evolved which have an acid reaction to test paper, and possess in a marked manner the odour of one of the volatile fatty acids—generally of butyric acid. If the process be carefully carried on in a retort, and the distillate be neutralized with an alkali, evaporated to dryness and then acidulated, a fluid is obtained possessing in a marked manner the odour of the fatty acid occurring in the sputum—the odour being the best available test which we can employ in the detection of small quantities of these acids; being as characteristic as it is delicate.

On examining the sputum of a patient recovering from acute bronchitis, I discovered that the acid which was evolved, after acidifying and boiling, had very much the odour of caprylic acid. The muco-purulent sputum of a case of phthisis yielded an acid vapour which had at first the odour of formic acid. On boiling the fluid farther, a butyric-acid odour was evolved. The acids which I think occur in the sputum are butyric acid, propionic acid, formic acid, acetic acid, and possibly caprylic acid. The occurrence of these acids in the sputum is not a matter for surprise. The researches of Scherer, Gorup-Besanez, and Schottin, have proved the existence of most of these in the muscular juice, milk, etc., which most probably derive them preformed from the blood. Formed by the oxidation of various fatty matters, they are probably constantly present in small quantities in the blood, where they are partly consumed, and partly separated by the various excretory organs, and, like other volatile substances, especially by the lungs. When the air passages contain fluid it is only natural that they should be dissolved by the fluid, and thus find their way into the sputa. It will be a subject for future inquiry whether the quantity of the volatile acids in the sputum is not greatest in those cases where the function of the lungs is so much impaired as to interfere materially with the oxidation of the blood.

The above observations answer, it appears to me, in a satisfactory manner, the questions which I purposed solving in this communication;—they show that the occurrence of butyric acid in the sputa cannot at present be proved to have any semeiological value; and that its presence is in no way characteristic of fetid bronchitis.

In reference to fetid bronchitis, it appears to me that the facts which have been collected with regard to it are scarcely of such a nature as to warrant our admitting a disease bearing this name into our nosologies. Like all other excretions, that of the air passages is occasionally liable to exhale a disagreeable odour. This sometimes occurs in acute and chronic bronchitis. No distinct and uniform series of phenomena have, however, been shown to characterize those cases of bronchitis in which the expectoration is fetid. We should, I think, avoid a needless and unphilosophical complication of our nosologies were we to speak of a bronchitis accompanied by fetid expectoration, instead of a disease *sui generis* "fetid bronchitis."

ARTICLE V.—*Notes on a Case of Paralysis following Diphtheria.* By JOSEPH BELL, M.D., Assistant-Surgeon Surgical Clinical Wards, Royal Infirmary.

(*Read before the Medico-Chirurgical Society, January* 18, 1865.)

AFTER an attack of diphtheritic inflammation in the fauces, affecting chiefly the right tonsil and right side of the uvula, but otherwise in no way remarkable except from the absence of any affection of respiration, and the slightness of the constitutional symptoms, the following various and almost curious temporary lesions of the nervous system occurred.

For the sake of brevity and clearness the symptoms shall first be mentioned, as far as possible, in the order in which they occurred, while a few words of supplement will suffice to describe the treatment and the result.

At about the beginning of the fourth week after the first appearance of the disease, when convalescence was nearly complete, and swallowing, which had previously been painful, from the state of the throat, was again becoming easy, attention was drawn to a peculiar indistinctness and nasal twang of the voice, first and specially noticed after any prolonged speaking or reading. While to the listener merely nasal in twang, it felt to the speaker as if something loose and flapping to and fro in the throat prevented the egress of the sound. This symptom grew rapidly and steadily more marked, till, in about three weeks, the voice was rendered almost unintelligible. The downward progress was hastened by every exertion, specially by a daily and unavoidable strain on it, lasting for an hour.

A day or two after the voice was first noticed to be altered, double vision was observed. It was constant, specially affecting objects at a distance beyond ten or twelve feet. The left-hand object was always seen at a slightly higher level than the other, and also slightly inclined from the perpendicular. Great difficulty was experienced in discovering which of the two individuals approaching in the street was the real one, especially if, as sometimes happened in forgetfulness, an attempt was made to pass between them.

The appearance of objects at a short distance, such as the letters of a book, was unaffected, except to a very slight extent, and only occasionally.

Almost coincident with the defect of vision a difficulty of swallowing began to be noticed, quite apart from the pain and rawness of the throat after the diphtheritic attack. The œsophagus seemed to have completely lost its power over the bolus of food or the mouthful of fluid. Liquids, instead of being gulped down, only could be allowed to trickle. The bolus manifested a much greater tendency to ascend to the posterior nares than to descend. As this symptom increased it was noticed that fluids were much the most difficult to manage, a certain portion, despite of all care, always returning through the nares, their relative pungency, when applied to the mucous membrane of the nose unaccustomed to such stimuli, varying from tepid tea, which was merely disagreeable, to pale sherry, which was positive torture. Solids, especially such coherent slops as calves'-foot jelly, cream, etc., were more easily manageable, though certain portions invariably stuck in the posterior nares, requiring one or more pinches of snuff for their dislodgement.

Coincidently with these throat and eye symptoms a feeling of general *malaise*, weakness, and discomfort, with occasional pain and sinking in the epigastric region, was gradually gaining ground; and about the end of the sixth week after the commencement of the attack, a feeling of oppression on the chest, slight difficulty of breathing, combined with a very weak, slow, and strikingly intermittent pulse, almost gave reason for believing that the heart itself was slightly affected as to its enervation. These latter symptoms, however, were noticed for only about two days.

At this time the lower limbs, which both in strength and steadiness had been gaining ground, began very perceptibly to lose it again, and gradually the gait became distinctly paraplegic, and going upstairs was a thing not to be attempted for every trifle, though a walk of three or four miles on level ground, and with the aid of a stick, was still quite practicable.

The summer session being now over, country air was ordered; but, notwithstanding change and rest, all the paralytic symptoms steadily gained ground. Walking was gradually curtailed, till from miles the distance came to be counted by yards. First a stick was sufficient, then the arm of one assistant, and at length two were needed. The hands and arms became gradually affected also, till, by the end of the eleventh week after the first attack, the condition was one of tolerable helplessness, the patient being unable to rise from a chair unassisted, to walk a step without an arm, or to handle his own knife and fork except very awkwardly.

By this time, however, some symptoms had turned the corner and begun to ameliorate,—speech and swallowing had both been restored, the former very gradually; the latter, as evinced by the restored power of drinking actively instead of merely letting fluid slip down very rapidly and suddenly. Double vision had gone.

A curious, though extremely annoying paralysis, both as to motion and sensation of one half of the tongue, and a corresponding portion of the mucous membrane of the mouth did not last long—and fortunately; for the helpless half, which was exactly defined by the middle line, was constantly getting into trouble between the teeth, and also became covered with a thick offensive greenish fur; the other half being clean, rosy, and active.

The general health was now apparently perfect; appetite and all the digestive functions excellent; the whole day being spent in the open air, and long drives affording every facility for changing the air.

Still the legs and thighs were so feeble, that except when raised by others, so as to render the whole limb straight, and thus call the spiral locking of the knee joint, described by Mr Goodsir, into action, immediate collapse resulted on any attempt to rise, and numerous falls attended any ambitious efforts.

For a fortnight or three weeks more, things seemed to remain in *statu quo;* till, about the middle of September, or three months from the date of the attack, recovery commenced, and was as rapid as the downward progress had been slow.

First the legs and then the arms gradually gained strength and steadiness, and by the end of another month recovery may have been said to be complete.

Before leaving the symptoms one must be noticed to which attaches a special physiological interest. During the month in which the paralysis of the lower limbs was most complete, a very annoying accompaniment was a peculiar heat and redness of the feet and ancles, with slight swelling; it seemed to arise from a fulness of every bloodvessel down to the remotest capillary; was quite different in its appearance from the ordinary œdema of weakness, and doubtless arose from the paralysis not being confined to the ordinary motor and sensory nerves, but attacking also the vaso-motor system. It was much mitigated and relieved by the internal use of atropia in tolerably large doses.

The treatment was constitutional and local.

1. *Constitutional:* Generous diet and a liberal—at first very liberal—use of wine, and latterly of bitter ale. Various remedies were

tried, chiefly of a tonic character; various preparations of iron; phosphate of zinc in large doses; a diligent use of Parrish's mineral phosphates; chlorate of potash; but except in so far as by their tonic properties they may have improved the general health, not the slightest advantage to the paralytic symptoms seemed to be gained by their use or lost by their omission.

Tincture of nux vomica, and strychnia in the form of pill, were both taken with very great regularity till one quarter of a grain daily was reached, and a slight tendency was noticed to a sudden snap of the jaws after a yawn, yet no appreciable benefit was noticed. It is possible, however, that had the strychnia not been persevered with the paralysis might have been worse.

Whether merely coincident or consequent it is difficult to say, but certainly recovery coincided very closely with the consumption, which was rapid, of a cask of Alsopp's bitter beer.

2. *Local Treatment: Friction*, with a stimulating liniment, was at times contra-indicated by the redness and heat, at other times possible, but apparently of no great advantage; *warm baths simpliciter* seemed only to make the weakness of the limbs more felt by their enervating effect. But *warm baths* persisted in for three or four minutes, as hot as possible, and followed instantaneously by cold douche on spine and limbs, seemed to have the very best result, both at the time and permanently. Such baths repeated every other night had a very marked effect in aiding recovery.

The morning cold bath, with prolonged cold douche on the limbs, though both pleasant and cleanly, could not be said to have any marked effect either for good or evil.

This communication, like many others on this subject which the Society has formerly heard, owes anything of value it possesses to Dr Begbie (senior), under whose most kind and skilful treatment the patient was from the beginning.

ARTICLE VI.—*Case of Foreign Body in the Œsophagus of an Idiot.*
By J. CRICHTON BROWNE, M.D.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 18*th January* 1865.)

A. C., æt. 25, a congenital idiot, subject to epilepsy, microcephalic (head measuring 19½ inches in greatest circumference), of scrofulous habit, low type of organization, when at dinner, upon the 5th December, having completed his own allowance of mince-meat, picked up some fragments of food from the plate of a companion, and pushed them into his mouth before the attendant could interfere. In about a minute afterwards he was noticed to be choking and gasping for breath, the face at the same time having become of a livid colour. The attendant, who was beside him, pushed his finger into his mouth, which was held wide open, and extracted from the pharynx a piece of irregularly shaped bone, measuring rather more than an inch in length. After this the patient exhibited no signs of uneasiness. He walked in the airing court and in his ward, seemed to be in his usual health and spirits when seen by the medical officers in the course of the afternoon, and swallowed without difficulty at tea-time, eating about 6 oz. of bread with butter. Nothing particular was observed in his condition when he was undressed and put to bed, and he was sleeping tranquilly when visited by the night-watch. The next morning, however, before getting out of bed, he was noticed to be unwell. There was a peculiar rigidity of the limbs, and an apparent loss of power over them. He was immediately examined by one of the medical officers, but no particular ailment was discovered. He could answer no questions that were put to him, but manifested no signs of suffering in any special locality. The throat, so far as could be ascertained by external inspection and by the finger introduced into it, was in a normal condition. There was no shrinking from pressure, no difficulty of breathing or in swallowing. The presence of the finger in the pharynx produced no irritation. He partook freely of breakfast on the morning in question, and seemed to improve in the course of the forenoon. It was thought that an epileptic paroxysm might be approaching, and a gentle aperient was administered. After dinner, which he did not take with his usual voracity, he appeared languid and drowsy, and accordingly he was put early to bed, where he at once fell into a placid sleep. In the evening the head and ears were hot and red, the general surface warm, pulse 90. Early in the morning of the 7th, a gurgling noise was heard to accompany respiration, while clear, glairy mucus was seen to flow from the mouth. At breakfast-time, swallowing was very imperfectly performed, all efforts at it being followed by regurgitation of the food and temporary suffocation. The mucous secretion seemed to accumulate very rapidly in the pharynx, and to be the chief impediment to deglutition, for whenever it had been cleared out by expectoration, swallowing was achieved with greater facility. The greatest difficulty was now experienced in examining, for a determined resistance was offered to all attempts to open his mouth. The teeth being perfect (a rare circumstance in an idiot), and the jaws with their muscles powerful, it was only after much trouble that a spoon was cautiously insinuated into the mouth, and a gag slipped in upon it. The fauces palate, uvula, and pharynx were then found to be coated with tenacious mucus, but of normal colour; and the finger passed over the posterior part of the tongue in front of the epiglottis discovered no peculiarity. A probang was then introduced, and passed readily and smoothly down the œsophagus to its full extent. On being used a second time, and passed a little to the right of the median line, it was for a minute arrested in its course, but on being pressed more to the left it again moved easily onwards. Neither the probang nor the finger occasioned any retching or apparent inconvenience. The chest as examined by percussion and auscultation was healthy, but there was large crepitation in the trachea and larynx. An ounce of mustard in a pint of tepid water was administered, but without effect. The fauces and soft palate were tickled with feathers, but no reflex action was induced. He was ordered a mixture containing squill and ammonia every four hours, with strong beef-tea, arrowroot, and new milk, as often as he could be persuaded to take them. In the evening the secretion of viscid watery mucus had increased in quantity. It accumulated in the mouth and pharynx to such a degree that the breathing was sonorous and laboured, and swallowing could not be effected until it had been in part removed. When cleared out by a feather, however, relief was obtained, and about half a pint of milk was swallowed without apparent discomfort. There was no tumidity of the throat, nor tenderness on pressure. There was no stridulous respiration, nor croupal cough; in fact, up to this time there was no cough whatever. The mucous secretion was expectorated in the ordinary process of expiration, and in occasional forcible efforts. The pulse was weak—120; the general surface was warm, but not highly feverish; the bowels had been freely opened; the water was passed in bed. The throat was now washed out with a solution of nitrate of silver ℈i. to ℥i., and marked benefit seemed to result for a time from this application. The secretions were less copiously poured out, deglutition was readily performed, and some sleep was obtained. Brandy was ordered, ℥ss. every four hours; iodide of potassium was added to the mixture so that gr. ii. should be taken every four hours also, and directions were given for the inhalation of steam, the head of the patient and a vessel containing hot water being enveloped in a blanket.

At 2 A.M. on the morning of the 8th, nitrate of silver was again applied to the pharynx, which had been previously cleared out with a feather. At this, as at all other times, the mouth had to be forcibly opened and gagged. At 11 A.M., along with the viscid mucus, patches of false membrane of various sizes, some of them being distinct circular moulds of portions of the trachea, were expectorated. These continued to appear throughout the day, were of considerable thickness and tenacity, of a yellowish grey or white colour, and muco-purulent constitution. Swallowing very difficult. Rhonchus (sibilant) heard in the chest upon both sides; respirations 38 per minute, pulse 90; surface warm; tongue moist, but covered with a thin yellowish fur. The patient constantly rolled upon his face, as this attitude seemed to facilitate the discharge of the morbid products from the mouth. Mustard was applied to the throat and chest; he was propped up by pillows with the face looking downwards, and was again made to inhale the steam of hot water; the brandy and the mixtures were continued, and nutrient enemata were ordered. In the evening there was a feverish exacerbation; subcrepitant rale was heard in the chest, and there was slight harsh cough at very wide intervals. Expectoration of mucus and shreds of membrane still continued; sinapism to be repeated and to be followed by hot fomentations, mixtures, brandy, and enemata as before. Passed a restless night, during which the throat had to be artificially cleared out at least once every hour; some urine was evacuated at 10 P.M., none having been previously passed for twelve hours. On the morning of the 9th, there was a diminution in the amount of the mucous secretions; the lips and nails assumed a blue appearance, the countenance became dusky, the pulse wiry, but very frequent. In the evening the extremities became cold, and the face livid, while gradually increasing drowsiness set in. Brandy was ordered every hour; swallowing was more easily managed. On the morning of the 10th, the secretions were altogether arrested. The patient gradually sank, and died at ten minutes to twelve.

Post-mortem examination.—Forty-eight hours after death. Body well nourished; livid in dependent parts; rigor mortis distinctly present. Dimensions of skull, after removal of scalp: antero-posterior, from nasal to occipital spine, 10 inches; lateral, between attachments of auricular cartilages, 10¼ inches; circumference, 18¼ inches. Skull of normal thickness; dura mater firmly adherent to skull, especially along median line, and posteriorly also much thickened; arachnoid thickened; vessels of brain generally engorged with dark-coloured blood; weight of brain with cerebellum, pons, and medulla, 35½ oz.; grey matter pale; medullary substance of a glistening appearance and unnaturally firm consistence; puncta sanguinea, very abundant on section of brain; ventricles empty; no trace of septum lucidum. Optic thalami, corpora striata, etc., normal; cerebellum normal; pons varolii, small; marked softening of the anterior pyramids of the medulla oblongata; decided congestion and redness of the floor of the fourth ventricle, extending to the point of the calamus scriptorius. Pericardium contained about ℥ii. of straw-coloured fluid; heart healthy; valves competent; dark purple clots in cavities of the right side, and straw-coloured, fatty looking clots in those of the left; both lungs much engorged; bronchi, down to remote ramifications, congested and containing muco-purulent fluid; small cicatrix at apex of right lung; old adhesions of upper lobe; hepatization of anterior border of middle lobe; old pleuritic membrane on posterior surface of lower lobe. Trachea contained a little muco-purulent fluid. An irregular, somewhat triangular-shaped piece of bone was found lodged in the pharynx; it was the top of the transverse process of the vertebræ of a sheep, and was neatly packed, on the right side, immediately above the cricoid cartilage, resting on the inner side on the arytenoid cartilages and crico-arytenoid muscles, on the outer side on the wing of the thyroid cartilage, the palato-pharyngeal and inferior constrictor. There was a wound in the mucous membrane of the posterior wall of the pharynx, where in its contractions it had pressed upon the free end of the bone. Behind the right of the pharynx there was a great deal of very offensive, dirty-greenish pus, which spread beneath the mucous membrane, around the constrictors, the stylo and palato pharyngeus muscles and other structures. It was found on the outer side of the space between the thyroid and cricoid cartilages, and was braced down the right side of the trachea, beneath the cervical fascia and sterno-thyroid muscle, to the attachments of the lobe of the thyroid. It penetrated for about a quarter of an inch between the thyroid gland and the trachea. It was also found along the bodies of the cervical vertebræ, the longus colli and rectus capitis anticus of the left side, and the posterior aspect of the œsophagus. It was on both sides of the pharyngeal aponeurosis; following the vertebral column it extended down to the body of the seventh dorsal vertebra, and there abruptly terminated. Other organs of the body of healthy appearance.

The preparation shows the fragment of bone in the position in which it was found, accurately fitting into the situation which it had taken up.

Remarks.—There are several points in this case worthy of observation. In the first place, I may allude to the obstacles which beset every attempt at diagnosis, and which are of such frequent occurrence in lunatic asylum practice. No information was to be gained from the patient, as he was incapable of forming a sentence, and could only articulate a few simple words. There was thus no opportunity for taking advantage of those valuable indications which the feelings of the sufferer afford; while the light to be gathered from physical examination of the body was only procured with much difficulty. The inspection of the throat was resisted to the utmost; so that the mouth had to be forcibly opened and gagged whenever it was thought desirable to examine the pharynx, or to make applications to it. In the second place, I may notice the anomalies which characterized the progress of the symptoms, which probably sprung out of the primary defects in the nervous system, and which set all ordinary theories at defiance. A foreign body of considerable size, with sharp edges and of irregular shape, lay wedged in the pharynx for about eighteen hours without producing the slightest inconvenience, and for about forty hours before there was any appearance of the dysphagia, anxiety, or suffocative attacks, which are the ordinary concomitants of such a condition. Indeed, up to the period of death, the direct effects of the foreign body were of a very trifling description. The difficulty of swallowing, which was subsequently manifested, appeared to depend almost entirely upon the accumulations of mucus which were constantly taking place in the pharynx, for it was invariably mitigated when these were cleared out. At no stage of the malady was there any cough worth speaking of, any spasmodic action of the muscles of the pharynx or œsophagus, or any rigidity of the sterno-cleido-mastoid or other muscles of the neck. The muscular contraction which was seen on the morning of the 6th, and which was probably due to a reflex irritation, was most marked in the limbs, and was only of short duration. It is obvious that there must have been impaired sensibility of the nerves contributing to the pharyngeal plexus, or defective power in the centres through which they communicate with the motor filaments, or in these motor filaments themselves, for all reflex phenomena were but imperfectly exhibited. The most determined titillation of the fauces, soft palate, and adjacent parts, by a finger or by feathers, failed to induce any responsive action; neither retching nor vomiting could thus be brought on. The gastric branches of the pneumogastric also shared in this singular torpidity, for a powerful emetic and distention of the stomach by warm water did not secure evacuation of its contents. It is thus rendered probable that the anæsthesia had a centric origin. But the remarkable circumstance is, that even when the symptoms of nausea and reflex movements could not be produced by direct irritation, deglutition was still very tolerably performed. It should be mentioned that it was at all times slow and laborious in this patient, and that some attacks of choking, consequent probably upon dysphagia paralytica, had led to his being fed upon mincemeat. During his last illness he swallowed for the first two days with average facility, and after that with an increased difficulty, which I have attributed to the quantities of mucus which collected at the top of the œsophagus. This secretion of mucus, with saliva in unusual amount, is characteristic of affections of these parts, occurring in persons labouring under nervous diseases. It is constantly seen in dysphagia spastica, and in the present instance it was thought to be indicative of pharyngeal catarrh, arising from the irritation of the piece of bone which had been removed. But this catarrh was of an asthenic type, for it was not only characterized by pellicular exudations, but by extension into the air passages, and by the supervention of suppuration in the cellular tissue connecting the pharynx to adjoining structures. The laryngitis and pharyngitis which were lighted up, were marked by the formation of membraniform concretions, which cannot, however, have been accompanied by any great tumefaction of the subjacent tissues, as there was but little interference with the respiratory process. The appearance of such products in a person of twenty-five years of age was a noteworthy circumstance, and pointed to certain constitutional proclivities, which were even more clearly indicated by the bronchitis which immediately followed, and which eventuated in death. This at once assumed the asthenic form, which by far most frequently occurs in large towns among the very young or very aged who are of cachectic habit and exhausted powers. In this tendency towards the infantile type it resembled all the affections to which idiots are liable. Even the best developed members of this class, who are of manly stature and mature years, are the merest children in their relations to disease. Their ailments seem to keep parallel with their mental capacity, and we must refer to West rather than to Watson for their pathological history.

Another interesting circumstance in this case was the rapid occurrence of diffuse suppuration in the pharynx, extending downwards into the thorax, instead of the limited abscess which is ordinarily the result of lesions in this locality. An imperfect development of the connective tissue may have favoured the diffusion. The suppuration itself was of a low and imperfect type, tending immediately to disintegration and decay. So offensive was the odour of the parts that it penetrated, and so green and aged was the aspect of the pus, that it was at first supposed that it must have been formed for some time, and that the piece of bone must have been lodged in the pharynx long anterior to the 5th instant. This, however, was negatived by the discovery that the fragment found after death corresponded to that removed during life, and that both belonged to the cervical vertebra of a sheep, a neck of mutton having been a portion of the dinner served up to the ward on the day in question.

Only one more point claims attention, and that is the state of the medulla as revealed after death, which, taken in connexion with the chronic epilepsy of the patient, the symptoms of the disease from which he died, and the origin of the glosso-pharyngeal, pneumogastric, and accessory nerves, affords some ground for speculation.

Part Second.

REVIEWS.

Practical Dietary for Families, Schools, and the Labouring Classes. By Edward Smith, M.D., L.L.B., F.R.S., etc. London: Walton & Maberly: 1864.

The subject of dieteties has made great progress of late years. By means of chemical analysis and experimental physiology we have obtained an accurate knowledge of many of the changes which go on in the human body, and of the composition and nutritive value of most articles of food. This knowledge has, however, been mainly confined to scientific men and members of the medical profession, and even they have not, so much perhaps as they might have done, applied their information to a practical account. Of late years the subject has certainly been brought increasingly before the public; and the extraordinary interest displayed in the recent onslaught against Obesity, shows how much interest is taken in subjects of this kind. It is highly characteristic that this interest was aroused by the publication of a pamphlet by a non-professional man. There was nothing new in the statements made by Mr Banting, nothing that was not previously well known to physiologists; but they were brought forward in a striking and practical manner, and his readers were speedily numbered by thousands. There is, of course, considerable danger in publications of this kind. Mr Banting found a particular system of dietary useful in his own case, and he recommended its indiscriminate employment by all persons labouring under obesity, not being aware that it was certain there would be circumstances in individual cases to render its application injudicious or injurious. Still, this episode has shown how willing the public is to be taught, and has proved how desirable it is that its teachers should be thoroughly informed men.

The work before us is just such a one as we could have desired to see; its character is admirably described in the preface. "It is essentially practical and popular in its aim, and therefore contains directions rather than arguments; but at the same time it is based upon the most advanced state of the science, and, except in the exclusion of many technical terms, is scientific as well as popular." No one was better qualified then Dr Edward Smith for the composition of such a work. His scientific attainments, his intimate acquaintance with the subject, and his logical yet practical turn of mind, constitute him an eminently safe guide in anything connected with dietetics.

The book is divided into two parts. The first part, entitled "Foods," treats of the elements of food which the body requires, and of the qualities of the various kinds of food. It contains minute and valuable information as to the value of all the ordinary kinds of food, and forms the basis on which the second part, headed "Dietary," is constructed. It is to this second and purely practical part that we would briefly direct the attention of our readers.

The first division of the second part treats of the Dietary of Families, and is divided into sections which contain rules for insuring a proper supply of food at the different periods of life. The dietetics of infancy are first treated of. For the first few months the mother's milk is undoubtedly the most proper food, but it frequently happens that the supply or the quality is deficient, and supplementary nourishment must be provided from some other source. Dr Smith maintains that this should consist exclusively of milk, that no bread, flour, or similar substance should be given, on the ground that milk contains all the requisite elements of nutrition, and that the absence of a nitrogenous principle in the juices of the stomach and bowels of the infant either entirely prevents the transformation of starchy food or very greatly lessens it. The following sentences expose an error which we believe is not unfrequently committed:—

"Again, some feed infants on cream and water, in the belief that they are thus offering a food richer than milk, but in so doing they make a grievous error, and the child rapidly pines away. The explanation is, that the cream is composed entirely of fat, except the small quantity of milk which accompanies cream when skimmed off the milk; and fat by itself is totally inadequate to sustain life. It has been already shown that milk contains casein, or cheese, in large quantity, with sugar and numerous important salts besides the fat, and all these are necessary to nutrition and growth."

During childhood milk should still form the principal article of diet, but farinaceous articles must now be added to it in suitable proportion. The question as to the age at which animal food should be given has occasioned a good deal of discussion; for our own part we believe that it is generally given too soon, and that children thrive better and have much clearer complexions when meat is very sparingly given during the first five or six years. Dr Smith makes the following remarks on this subject:—

> "There can be no doubt that a child which has been well supplied with milk and farinaceous food properly cooked, has no need of meat up to four, six, or eight years of age, since in the milk he has received all the necessary elements of meat, and in a form more readily capable of appropriation by the system than in that of meat; but on the other hand, if the milk have not been sufficient in quantity and quality, the defect may be in some part supplied by the use of meat; and if, with an abundant supply of milk, a small quantity of meat have been given, it is probable that no injury has been inflicted. When a child has been fed on the plan laid down in this work, the use of meat may be regarded quite as a luxury—to be exhibited first as gravy with the vegetables, and afterwards as solid meat cut into very small portions and eaten in a small quantity. Hence, at three or four years of age, the dinner should supply a little potato with any reasonable quantity of meat gravy, if the latter have not been too highly seasoned and salted, in addition to the milk pudding. After that period and up to the limit above mentioned, the use of solid meat should be in small quantities, well divided, deliberately masticated, and eaten at first occasionally and then regularly. The meat should be well cooked and eaten hot, and the usual variety of white and red meats may be given.
>
> "The points on which I am desirous to lay stress, and which must guide the mother, are these:—1*st*, With an abundant diet of milk, eggs, and farinaceous food, meat is not necessary. 2*d*, With a spare diet of milk, meat should be given. 3*d*, With milk pudding as a principal part of the dinner, the meat should be eaten after the pudding. 4*th*, Variations which are slight in degree are often well borne; and, 5*th*, With abundance of the food named, and with the eager appetite of childhood, the use of savoury food is unnecessary, and the kinds of food in use should be few. With a pampered appetite the right kind of food is discarded, and an insufficient amount of nutriment is obtained. With a wisely trained appetite from infancy, plain foods are eaten and abundant nourishment received."

Dr Smith strongly deprecates the use of tea and coffee in childhood, on the ground that being nervous stimulants they must at this period of life be not only unnecessary but injurious.

Regarding the dietary recommended by Dr Smith in youth, in middle life, and in advanced age, we have nothing special to remark. In addition to the general rules laid down, special directions are given to those who are in peculiar circumstances; to those who are of feeble constitution; to those who make much exertion, and so forth. We must not omit to mention two sections addressed respectively to those who wish to *increase*, and to those who desire to *decrease* in weight. We have only space to quote the dietary which Dr Smith recommends in the case of the corpulent who desire to diminish in bulk.

"For breakfast at 8 to 9 A.M., and tea at 7 to 8 P.M., three-quarters to 1 pint of tea, with 1 oz. of milk, and one-third oz. of sugar, 2 oz. of bread, eaten fresh or toasted, or 2 oz. of biscuit, one-third oz. of butter, and 2 to 4 oz. of gluten bread, toasted, and eaten when quite hot. Also one or two eggs, taken boiled or poached, or 2 oz. of any kind of lean cooked meat, eaten hot or cold.

"For dinner, 6 oz. of soup made from the shin of beef and vegetables, but with all the fat skimmed off; bread, moist or toasted, 1 oz.; cooked meat, hot or cold, 4 oz.; green vegetables, 6 oz., or potatoes, 4 oz. On alternate days an omelette of one or two eggs, when not eaten at both the other meals, or 4 oz. of any of the puddings already mentioned, or 2 to 3 oz. of cooked macaroni. Half a pint of water or *very* weak table beer, or 4 oz. of light wines may be drank. When the dinner is eaten at 5 o'clock, or at a later hour, this plan must be modified. The tea will then consist only of half a pint of tea prepared as above mentioned, and without the other foods; and there will be a lunch at 1 to 2 o'clock, of 2 oz. of cooked meat, or 1 oz. of cheese, with 2 oz. of bread eaten when moist or toasted, with a small glass of water or *very* weak table beer; or it may consist of a glass of light wine with a biscuit.

"It is evident that with persons differing extremely in the amount of food which they eat daily, in the exertion which they make, in the age at which they have arrived, and in their views and desires upon the question of food, no fixed dietary can be adapted to all persons; and whilst many combinations of food could be made of equal value, no scheme can be otherwise than general in the quantity of any particular food which it may afford. In the foregoing it is assumed that there will be suitable interchanges of the food, so that, for example, eggs will not be taken at all the meals, and that within the limits prescribed the palate will be consulted. If the craving for food should in any person be considerable with this dietary, the quantity of gluten bread, green vegetables, and meat, may be increased.

"If we assume that in this dietary there will be consumed daily 3 oz. of milk, 5 oz. of bread, 4 to 6 oz. of gluten bread, 6 oz. of cooked meat, two medium sized eggs, the materials of the soup, 1 oz. of butter and suet, 1 oz. of sugar in puddings and tea, 6 oz. of green vegetables, or 4 oz. of potatoes, and 1½ oz. of flour or some equivalent farinaceous food, 7 to 8 oz. of carbon will be eaten daily. This will not be a deficient dietary for an idle man, or for an old man, or for one who has been habitually a small eater; but it will be deficient in the contrary conditions, and therefore it should never be reduced except under proper medical advice.

"With this regimen there should be regulated walking in the open air, during two to four hours daily, as the degree of obesity will admit of. An occasional Turkish bath should be taken, and the skin should be well wiped with cold salt and water twice daily. The clothing, both by day and night, and the temperature of the rooms, should be only moderate."

In the chapter headed "Dietary in Schools," Dr Smith very properly insists on the extreme importance of a due supply of food to boys and girls, and points out how through ignorance or economy a deficient supply is too often afforded.

Perhaps the most important portion of Dr Smith's book is that which treats of the dietary of the labouring classes. Among no portion of the community does so much ignorance prevail regarding the value of different articles of food, and among no class is greater relative extravagance practised. Dr Smith treats first of the present mode of living; and his own investigation made by direction of government, and published in the reports of the Medical Officer of the Privy Council, enable him to speak from an extensive personal knowledge. He then describes what he considers the most suitable dietary, and gives much valuable information regarding the nutritive value of various kinds of food in reference to the price at which they can be obtained. He also gives various specimens of dietaries with their respective costs, and it is really wonderful to find how, by proper management, a nutritious diet can be obtained at a very low price. This part of the work he concludes as follows:—

> "Such, then, are abundant and cheap dietaries for our working classes, at a cost within the reach of all in England who obtain regular employment, or who are not oppressed with a large family wholly dependent upon the head. There are, however, multitudes of persons both in England, in our sister island, and on the Continent, who from these and other causes do not obtain so much income as would enable them continually to purchase this quantity of food, and to such it is of the greatest moment that they should restrict themselves to the cheapest food, viz., Indian corn, peas, bread, butter-milk, and skimmed milk. I need not refer at length to the nutriment to be obtained from these foods, as that has already been stated in Chapter III., but a reference to the following table will show that two pounds of Indian meal made into stirabout will afford more than the required nutriment at a cost of 2d. per day; and it cannot be doubted that this, with 1 pint of butter-milk, costing ½d., or of skimmed milk, costing ¼d. to ½d., would, if the appetite for it did not fail, sufficiently nourish the system. Again, in the case of those who grow a large quantity of potatoes at a merely nominal cost, there can be no doubt that they may make them a principal article of food, and, taken in sufficient quantity with butter-milk or skimmed milk, would maintain health at a cost much below that of the dietaries which I have devised."

Dr Smith has, we consider, done a national service by the publication of this book. It should be in the hands of every medical man; and we know of no work calculated to be of greater use to those who are desirous of improving the condition of the lower classes.

On the Diagnosis and Treatment of Cancer and the Tumours analogous to it. By MAURICE HENRY COLLIS, M.B. Univ. Dublin, F.R.C.S.I. London: Churchills: 1864.

WE have much pleasure in bringing under the notice of our readers the work of which the title forms the heading of this article. It is a true book; the result of much thinking, and of the exact observation of a few cases, rather than of extensive reading or the accumulation of elaborate statistics.

Thoroughly conversant with the pathological literature of the present day, and imbued with its style of thought and method of research, the author is evidently no mere pupil or retainer of one or other school, but an independent thinker; a free lance, ready to do battle on one side or the other indifferently, or, if he sees need, to fight for his own hand.

In the very first sentence he sounds the key-note which is faithfully maintained, that a knowledge of pathology must be the basis of all true diagnosis. To a knowledge of the pathology of cancer and the allied tumours the microscope is essential; but the use of the microscope must be guided by common sense. On this Mr Collis' remarks are excellent. He is not one of those who believe that the presence or absence of a specific cell in a given tumour can convey much useful information: such pretensions, when pitted against the instinct and the educated fingers of operating surgeons, almost brought the microscope into disrepute; but he insists, as most of us do now, on the extreme value of what the microscope tells as to a tumour's life, the manner in which its cells are arranged, the speed with which they grow, the relative proportion of cells and fibres, the amount of organization and strength of the fibres, the position of the tumour in relation to the neighbouring tissues, the whole subject of infiltration, and the changes, degeneration, or development which the cells themselves undergo.

But while pathology is thus all-important for increasing our knowledge of disease as a whole, and also in individual cases for aiding our prognosis, we require to be able to recognise the cancer or other tumour while still *in situ* and out of the reach of the microscope, if we wish to treat our patients rightly. In this direction, also, Mr Collis' treatise is practical and successful. The cases given are not very numerous, but the clinical history of each is careful and graphic, the really important facts thoroughly brought out, and the cases are, so far as this has been possible, followed to their termination.

Pathology and clinical history are the two chief lines in which our researches on cancer and all tumours are to be directed; and as all clinical description must depend on the nomenclature, and the nomenclature in its turn depends chiefly on the pathology and the real or supposed relations of different groups to each other, we must here notice one or two slight changes which Mr Collis introduces into his nomenclature and arrangement.

Under Cancer he includes acute or encephaloid, chronic or scirrhus, with sub-varieties of each. Then, under the head Cancroid, he places fibro-plastic, including myeloid; and fibroid, including colloid; and then makes two separate groups for fibrous tumours and epithelioma.

In this latter separation of epithelioma from cancerous tumours we cannot agree with Mr Collis, and think his arguments require reconsideration. For, whether looked at pathologically or clinically, we hold there are no differences between epitheliomata and other cancers that cannot be perfectly accounted for by the position of the former being superficial, their having free access to the surface, hence ulcerating early, and in their being easily removed.

Its characters, in the tongue of rapid growth and infiltration, are surely sufficient to win for epithelioma the bad eminence of being quite as bad a cancer as any of its neighbours, granted only a sufficiently central and sufficiently vascular organ for it to attack.

The brief analysis of the contents of some of the more important chapters, at least, is necessitated, if we wish to give a fair idea of the scope as well as the value of this work.

The first chapter is devoted to a brief comprehensive resumé (illustrated by diagrams) of the various forms of cell growth, starting from what Mr Collis calls the lymph cell, as a standard of comparison. In its breadth of thought, it reminds us of Mr Simon's most masterly and suggestive paper on Inflammation, in Holmes' System of Surgery. The following sentence on the pus cell agrees with Mr Simon's views as to the "sterile finality" of pus:—

> "Pus affords another example of the low vitality of the lymph cell. Pus cells are only lymph cells which have perished, and which, in parting with most of their nitrogenous element, exhibit a superabundance of oily granules in their interior. This is the true reading of the multiple nucleus of the pus cells; it is not, as Virchow and Lionel Beale would have it, a spontaneous and vital division of the nucleus, preparatory to a multiplication of the cell by division; it is only a step towards the disintegration of the cell, and an evidence of the loss of its life in its very centre and most vital part."

The second chapter treats of the clinical history of tumours: their arrangement, physical characters, numbers, etc.

In the third chapter, Mr Collis considers encephaloid cancer which he regards as acute or typical cancer. It contains an excellent description and explanation of the different forms of death by exhaustion, and death from hæmorrhage, from the so-called fungus hæmatodes. Several illustrative cases are given with accurate and minute histories.

The relation of encephaloid to the period of growth or development in organs or bones is well brought out; while the greater prevalence of encephaloid of the breast in France and Ireland, than in England, is noticed, but hardly explained.

Chapters IV. to VIII., inclusive, are devoted to scirrhus, its varieties and treatment. The description of atrophic scirrhus (that form of cancer which reduces the size of the organ below its natural dimensions) is excellent, and the advice, not to interfere, a sound one, the natural life of the tumour being remarkably chronic, sometimes lasting for nearly thirty years.

Lardaceous scirrhus is the name given to that variety in which fat is also deposited in considerable quantity along with the cancer, and in which the skin assumes its peculiar brawny hardness and roughness, as if seen through a magnifying glass.

On the treatment of scirrhus, Mr Collis is full of detail, specially on the remedies for reducing the inflamed state in which the tumour so frequently is when first seen by the surgeon. For this purpose he recommends free leeching, to be followed by the persistent local use of bladders of pounded ice, the internal use of bromides and iodides, weak evaporating lotions, pressure by strapping, various plasters, etc. On the subject of removal his remarks are very sound and sensible; his conclusions are, that it is justifiable only when it can be complete; that the knife is preferable to all caustics; and that, in cases where the knife is inadmissible, only the most powerful caustics are allowable. Among these he specially refers to Mr Syme's plan by strong sulphuric acid and sawdust.

Chapters IX. to XIII., inclusive, are devoted to the group of cancroids, including fibro-plastic, myeloid, recurring fibroid, fibro-nucleated, fibrinous or hæmorrhagic, and colloid tumours. The chapter on the fibrinous or hæmorrhagic tumours, is an endeavour, and not (we think) a very successful one, to group under one head, and explain by one pathology, a number of those curious and puzzling cases the result of injury, of very rapid, sometimes instantaneously sudden, origin, and which on section resemble more or less coagulated blood. Of the groups here described, one must have very closely resembled a tumour of the testicle lately exhibited by Professor Spence to the Medico-Chirurgical Society of Edinburgh; another was, apparently, an encephaloid of the ham; and a third, described at p. 202, was almost certainly a ruptured artery.

Chapter XIV., on fibrous tumours, takes those of the uterus, prostate, and polypi of the nares and pharynx, as its text. The chief pathological peculiarity of this group is, that it is possible to destroy them and prevent their return, even though they are not absolutely extirpated.

Chapter XV., on epithelioma, is a long one, giving a full description of its microscopic characters, and the clinical history of these tumours or ulcers in their most frequent situations, besides going into considerable detail on the subject of plastic operations for the removal of the lower lip.

Chapters XVI. and XVII., on cysts and adenoid tumours, are introduced, not with a view of going deeply into the origin, pathology, or treatment of these growths, but only because they may often be confused with, or may degenerate into cancer.

Chapter XVIII., on melanosis, as an accident of many tumours, especially of cancers, is chiefly remarkable as giving an account of a most interesting case of multiple melanotic tumours over the trunk and extremities.

Mr Collis has given the profession a work which will be found as interesting as it is instructive. His style is pleasant and lively. He has evidently a thorough interest, not only in his profession, but in every individual patient he describes. There are few references, no long-winded notes.

The book is charmingly printed, with some most exquisite lithographs, some coloured, and, as usual, over-coloured, but some of the uncoloured ones are as near perfection as possible.

Every surgeon will find it a valuable addition to his library.

1. *The Use of the Laryngoscope in Diseases of the Throat, with an Appendix on Rhinoscopy.* By MORELL MACKENZIE, M.D. London: Hardwicke: 1865.
2. *The Laryngoscope.* By GEORGE JOHNSON, M.D., M.R.C.P. London: Hardwicke: 1864.

IN the first of the above works the physician will find all that it is requisite he should know regarding the laryngoscopic and rhinoscopic arts. It is a thoroughly practical book, and to all who desire to become acquainted with these new methods of exploring and of treating throat affections, it must prove of great value. Its author is already well known to the profession as an earnest and successful investigator of these forms of disease. In the first chapter, Dr Mackenzie gives an interesting history of the invention, detailing the prominent features in the methods of viewing the larynx proposed by Levret, Bozzini, Senn, Babington, Bennati, Baumes, Liston, Warden, Avery, Garcia, Türck, and Czermak. The second chapter describes the instruments now most in use, and the third treats of the theory and practice of the operation itself. The directions for using the instrument are so plain and intelligible, that a perfect novice can require no further instruction. In Chap. IV., a minute account of the appearance of the healthy larynx is well illustrated with woodcuts. Chap. V. treats of some accessories of laryngoscopy, such as auto- and recipro-laryngoscopy. We agree with Dr Mackenzie when he expresses his belief that "a thoroughly effective instrument for drawing forward the epiglottis would be, without exception, the most useful addition to the art of laryngoscopy." In our own comparatively limited experience, we have found more difficulty from a pendulous position of the epiglottis than from any other condition. The epiglottic pincette invented and described by the author looks as if it would go far to meet this difficulty. The remaining chapters of the book are devoted to treatment, or rather to the means of addressing curative measures to the diseased parts. The various modes of applying solutions, powders, solid caustics, escharotics, and galvanism, are clearly described and illustrated by cases. The different operative proceedings applicable to the larynx are next explained. The extirpation of warty growths by means of forceps of the author's own invention, is specially illustrated by six cases in which Dr Mackenzie successfully removed such growths. In an Appendix the art of Rhinoscopy is described.

We have much satisfaction in recommending this book to the profession. It is just what such a treatise ought to be,—brief, clear, and systematic; whilst every page exhibits the results of the author's matured experience and reflection.

Dr George Johnson's little work consists of two lectures delivered before the Royal College of Physicians of London. They were originally published in the *Lancet*, and are now reprinted, with some inconsiderable alterations, within the limits of a small pamphlet. It contains much valuable information, and will be found a trustworthy guide by the student.

The Practical and Descriptive Anatomy of the Human Body. By THOMAS LEDWICH and EDWARD LEDWICH. Second Edition, Revised and Enlarged by EDWARD LEDWICH, F.R.C.S.I., Surgeon to Mercer's Hospital. Dublin: Fannin: 1864.

THIS is the second edition of a work, originally published in 1852, revised and enlarged by the survivor of the two brothers by whom the first was written. After a separate existence of thirteen years, it is almost too late to ask, Was this work needed at all? By whom was it required? Had it a purpose to serve? Has it served it?

Before endeavouring to answer these questions, we have a word to say on its arrangement. It is eminently a systematic work in its arrangement, conventional rather than scientific in its simplicity; for under six systems is grouped the whole range of anatomical nomenclature. This involves, or at least has resulted in, some rather incongruous juxtapositions and divorces. Thus, under the head "Osseous System" we find the teeth, *nares*, and the Schneiderian membrane. Under "Muscular System," we have the eyelids, eyelashes, thyroid gland, *posterior nares*, larynx, generative organs, mammary glands, and milk. The cavities and viscera, under the head of "Splanchnologia," have a large share of patronage, but do not include the bladder or uterus, both of which are under muscles. Neurologia includes brain, nerves, skin, eye, and ear; while Angiologia, or the Vascular System, concludes the work, with an account of arteries, veins, and lymphatics; the heart, however, having been previously treated of under Splanchnologia.

Judging from the arrangement alone, it might naturally be supposed that the work was intended for study at home of these various systems in their completeness, each as a separate whole, and to supplement the more regional and fragmentary views which the student gets in the study of relative anatomy in the dissecting-room. Yet this appears to be not at least the sole intention of the work; for in the preface we are told that the author will consider "my time and labour well expended should I succeed in winning him (the student) back to the subject;" and in the body of the book directions are everywhere given for the actual dissection of the part. If it is intended that the student should be *won* back to the subject by the anticipated delight of having such a work as this to guide his hands, we fear the expectation will be disappointed; but it is less unlikely that the anxious inquirer may be *driven* to the subject by the utter impossibility of making anything, of relational anatomy at least, by the aid of this manual.

Returning to the question of the purpose of this work, and the manner in which it has been fulfilled, we find from the preface, and also from internal evidence, that a "systematic description has been attempted," combining at the same time an account of the relative anatomy of parts and organs, with a description of their structure and function. The question then arises, How will this work bear comparison with anatomical books already published? As a systematic work, it will not bear comparison with the Descriptive Anatomy of Quain and Sharpey; while we miss entirely the admirable section on general anatomy prefixed to the latter work. As a manual of dissection, again, we should infinitely prefer the good old Dublin Dissector.

It may be said that the work under review possesses the advantage of being much smaller than the others, being contained in one volume, and doubtless cheaper. It contains a carefully written description of the various systems, but labours under the very great disadvantage of having no plates whatever to refer to, and, worse than this, the reader cannot even use the *part* directly as a reference, at least in an intelligible manner, for the very obvious reason that the description has to follow the separate vessel or nerve through the limb, without more than a passing reference to its relations.

Perhaps the saphenous opening, cribriform fascia, etc., is the most difficult region in the body to describe accurately and intelligibly. Turning to it, we find, at p. 371, a very painstaking and elaborate description, but one which, unaided by plates, almost presupposes the existence of considerable anatomical knowledge.

We once knew a very idle student, who, being occasionally attacked by fits of contrition, yet unable to conquer his hatred of the dissecting-room, used to endeavour to master his "Ellis" by committing it to memory *verbatim*. For this purpose Ledwich's Manual would be shorter than Quain's, and perhaps easier in the graceful turns of its sentences than Ellis's rugged and involved style. But here and there dissectors would have their revenge. In the description of the joints, for example, our industrious and retentive friend would first, perhaps, find a little difficulty in recognising the knee-joint under the name tibio-femoral articulation, and would then have to commit to memory the fact that "it belongs to the class of angular ginglymus," and that "its media of connexion are twelve in number." Seriously, however, the really careful and accurate descriptions of the several joints, bones, and muscles, in which the real strength of the work lies, are deformed here and there by useless pedantry of expression, which, however suitable in a big German folio, is out of place in a student's manual.

Although Mr Ledwich's work cannot, from its comparative brevity, and from the almost entire absence of the details of structural anatomy, be considered a complete systematic book, it only requires, with the author's clearness and powers of description, a different arrangement to make it a most excellent "Dissector."

Part Third.

PERISCOPE.

PHYSIOLOGY.

ON THE PHYSIOLOGY OF THE BRAIN AND NERVOUS SYSTEM.

BY DR BROWN-SEQUARD.

(*A Lecture delivered before the College of Physicians of Ireland, on February* 3, 1865.)

THIS lecture, I am afraid, will have many faults. Laying aside the principal one, of which I do not mean to speak, it has to treat of so many topics, that the variety will itself be a great obstacle to the comprehension of many of the views which will be put forward. I shall, however, endeavour, while putting forward as many facts as possible, to condense them, to accommodate them one with another, and establish some tie, some union, between them, so that they may, as far as practicable, form one connected and consistent whole. That whole will have for its principal object to show the immense importance—to the physiologist, of the study of morbid cases in the human species—and to the practitioner, of the study of physiology, and especially of that branch of physiology which is founded upon experiments on animals. Indeed, a complete revolution has been made in the practice of medicine in this century from the study of effects observed upon animals, and if, together with the light thereby thrown on morbid cases in our own species, we make an appeal not only to physiology—and experimental physiology particularly—but if we also try to throw the light of normal anatomy, and especially the anatomy of the nervous centres, and of the nerves at the base of the brain, we find that to understand the symptoms of a large class of pathological cases in our own species is almost as easy as to read the alphabet. Indeed, a great many of the most complicated, the most obscure, and the most unintelligible cases of nervous complaints are as easily understood as the simplest case of bronchitis or any slight inflammation, if we have the light which experimental physiology and the anatomy of the base of the brain now give us.

Suppose, for instance, a patient comes to us with paralysis—a perfectly and absolutely complete paralysis of one-half of the body, from the neck downwards. Suppose that, in addition to this paralysis of motion (say in the right half of the body), he has also on the same side extreme hyperæsthesia, or increase of sensibility in all those parts which are struck with paralysis of motion. Let us suppose that we find not only extreme sensibility to touch, which we may measure with the compasses, but also an extreme sensibility to tickling, which is a sensibility quite distinct from the other. Suppose we find, besides all this, that the sensibility to a prick or a pinch—in fact, the sensibility to painful impressions—likewise the sensibility to changes of temperature, to cold and heat, is also immensely increased. Thus, you have these four kinds of sensibility, which, I repeat, are absolutely distinct one from the other, all considerably increased in that limb apparently dead, as it has not the least power of motion. Suppose besides, that in the same limb we find the temperature considerably increased, and that the circulation there is more energetic, not more rapid of course, because it is the same heart that propels the blood there as elsewhere, but fuller, there is decidedly more blood there than elsewhere in the body.

All these symptoms, mark, are observable on *one* side of the body—the right; if we now contrast them with what we observe in the limbs on the opposite side, they acquire still greater interest. If we examine the *left* side of the body, we find an absolutely reverse condition. We find that all those various species of sensations of which I have spoken are lost, completely and absolutely lost, upon the left side. We find there is also on the left side—in opposition to what exists on the right—a complete power of movement, not the least diminution of the power of the will. Suppose, going further, we examine into the condition of the fifth kind of sensibility existing in the limbs (admitting that there are only five species of sensibility), if we examine into the sensibility that exists in the muscles, which help us to direct our movements, we find the muscular sense remains perfect in those limbs which have lost the other kinds of sensibility. There is on the *left* side, therefore, a complete anæsthesia of the *four first* kinds of sensibility of which I have spoken, combined with a *persistence of the muscular sense* and *persistence of voluntary motion;* while on the *right* side the condition is exactly the reverse. But what about the degree of heat in the limbs on the left side, and what about the circulation? On the left side there is not only a much *inferior* degree of heat than on the right side, but there is also an actual diminution of heat, if you compare those parts with what they were in their normal condition; in other words, there is an *absolute* diminution of heat on the left side,—not simply a relative diminution; so that while on the *right* side the temperature of the body has increased, on the *left* it has diminished, and similarly with regard to the circulation.

These features are striking enough, yet there are many others in the same individual upon which I cannot now dwell, but which are fully as interesting and equally difficult to understand by a man who is not *au courant* with the present state of physiological science.

In the *face* on the side of the injury (admitting that an injury is the cause of these symptoms), there is an increased heat, an increased sensibility, a contraction of the pupil, and a degree of occlusion of the eyelids, so that the two eyes, if you look at them at the same time while open, are quite different one from the other; the eye on the side of the hyperæsthesia and of the increased heat is smaller, and the opening of the eyelids smaller than on the other side.

All these facts we produce in animals very easily, and it has been my good fortune (in some respects) to find many such cases in our own species; one of the most striking of them was seen with me by my dear and talented friend Dr Robert Macdonnell, in which the symptoms upon which I have been dwelling were as marked as possible.

Now, what was the injury which produced all these remarkable effects? It was simply a division of the spinal cord, not simply of the half of that organ in the neck, but of the entire half—*i.e.*, the posterior column, the lateral column, the anterior column, and the grey matter of one side, had been divided completely. Owing to that injury all those symptoms existed.

Now, I put it to you, if any physician at the beginning of this century, not knowing the present state of our knowledge in physiology, no matter how learned and how able in other respects, had such a living problem been presented to him, he would most certainly have been at a loss to understand the case. Nay more, he would not *see* the case as I have described it; he would not recognise the existence of those symptoms; he would probably commit the error committed by the great French surgeon who had had such a case, but who never discovered that the sensibility was lost on the side still under the power of the will until the nurse told him of it.

From these facts you may see the importance of a thorough knowledge of physiology. The physiologist can have no difficulty in understanding such a case; for when he knows that the spinal cord is the organ conveying the orders of the will to the muscles, that the nervous fibres serving for voluntary movement pass from each side of the spinal cord, so that the fibres which serve for the movements of limbs on the *right* side pass to the *right* side of the cord, and those serving for movement of limbs on the *left* side pass to the *left* side of the cord, it is quite evident that such a division of the cord as I have described will produce loss of motion on one side, and not on the other. On the other hand, the sensitive nerve fibres, which serve to the four first kinds of sensibilities I speak of, pass into the spinal cord in such a manner as to go to the *other side* of that organ, so that the nerve fibres of sensibility in my *right arm and right leg* pass into the *left side* of the spinal column, and *vice versa*, and thus a division of the cord produces loss of sensibility on the opposite side of the body to that of the injury.

Equally clear is the explanation as regards the increase of heat in the limbs on the side of the injury. The nerves of bloodvessels pass into the spinal cord on the side corresponding to that of the limbs into which they go, just as with the nerves of voluntary movement; so that a division of the spinal cord in or below the neck on the *right* side produces paralysis of the nerves of bloodvessels in the right side of the body, in consequence of which paralysis there is an increase of the impulses of the heart everywhere on that side, causing a greater efflux of blood and increased heat, and (as a consequence of the increased heat) in a measure also the increased sensibility of which I have spoken, the hyperæsthesia of the four kinds of sensation.

I cannot dwell further on this class of cases; enough has been said to show how much light physiology can throw on symptoms which certainly would have been most obscure (to say the least) to even the most eminent men of the beginning of this century, who did not know the physiological facts which have been since discovered.

I shall now produce another case.

Let us suppose a man has sustained an injury, not of the spinal column, but of the medulla oblongata above the decussation of the anterior pyramids,—not such an injury as would destroy life at once, not such a tumour or morbid alteration as would prove immediately fatal; but suppose an injury of sufficient extent to produce decided symptoms, though not enough to cause immediate death.

Now in this case you would have all the features which I have described in the former case, but with this difference, that as the anterior pyramids decussate below the injury, an injury on the right side would strike the fibres of voluntary motion belonging to that side going up to make their decussation before they passed from the right side into the left side, and it would also strike after their decussation those fibres of voluntary movement that have come from the left side. Thus, therefore, there would be in this case paralysis of movement on *both* sides of the body. As regards the state of the circulation and heat, and as regards the hyperæsthesia and anæsthesia, everything would be similar in this case to the former.

Let us now suppose the injury is a little higher up, and we shall find other striking differences.

Suppose a patient comes to you with paralysis of the external rectus of the eye—suppose on the right side—the *face* is paralyzed on the same side; there is also anæsthesia of the *face* on the right side; but in the left side it is not the face but the *body* that is affected with paralysis, both of sensibility and motion.

Here, then, is a case absolutely distinct from both the others—a case in which there is loss of motion and loss of sensibility in the *face* on the right side, and in the *body* at the opposite side. I cannot dwell at length on the other features of the case, but I must not pass from it without noticing a most striking feature.

You will often find in similar cases that the tongue is perfectly free,—there is no loss of movement at all in that organ. You will find the facial paralysis is just the same kind of paralysis that exists when the facial nerve outside the cranium has been injured, *i.e.*, that the muscles which give expression to the face and also the orbit of the eye are paralyzed. This case, therefore, is quite distinct from cases of hemiplegia. As you are aware, in paralysis due to disease of the brain you will find the face paralyzed on the same side as that in which the body is paralyzed, the orbicularis is not paralyzed, while the tongue is almost always somewhat paralyzed. In the class of cases I am now submitting to your notice the distinction is characteristic and striking. The grimaces which the patient makes, owing to the paralysis of the face, take place on the side of the paralysis, instead of on the other side, because the paralyzed side of the face is the opposite to that which is usually paralyzed. Besides all this, you will find that the sense of *taste* is altered on the side on which the face is anæsthetic. You will find further that the patient is in a state of considerable emotion; he will shed tears easily, he will gape frequently, and while gaping there frequently will be a sudden jerk of the paralyzed limbs. There is also generally considerable giddiness and tendency to vomit. I mean now only to put forward the principal symptoms.

Now, I ask, what is the explanation of this case? Do you think the most eminent man of the beginning of this century, not knowing the science of physiology and anatomy as we now know it, could have understood this case? Certainly not. Therefore, as you may perceive, physiology and anatomy are immense helps in the diagnosis of disease.

Now, the set of symptoms I have last described belong to a case of disease of the pons varolii, striking at the same time the roots of the trigeminal and of the facial nerves before they have made their decussation, which is at the lower part of the pons varolii, and striking also the sixth pair of nerves before it has made its decussation, producing paralysis on the side of the injury, just as much as if the cause existed in the nerve itself.

You must not, however, think that an alteration of the pons varolii will in all cases produce all these effects. If the alteration takes place a little higher up than the middle of the organ striking at the place where the facial nerve and trigeminal nerve cross, you will have these effects. The two sides of the face will be paralyzed, both in sensibility and motion; the two sides, as regards the action of the external recti of the eye, and also the sense of taste, will be paralyzed, while the paralysis in the body will be only on one side. It is quite sufficient, in order to understand this case, to keep in view what the nerves do when they reach the pons varolii. When the injury strikes the pons varolii *above* the decussation, you have, as regards the portions of the face and body which are paralyzed, the same effects as are observable in most cases of brain disease—that is, the paralysis of the face will be on the same side with the paralysis of the body; if, on the other hand, the injury strikes the pons varolii below the decussation, the contrary effects are perceived.

The question, however, remains, how can you know when the disease is in the pons varolii, if you lose that peculiar symptom of the sides as regards paralysis of the face and body? In the first place, if the injury is one producing anæsthesia, you will find in the beginning of the affection, if the injury is in the pons varolii, a most important symptom—viz., a great and extreme coldness of that side of the body which is about to become paralyzed; in fact, just the reverse of what will occur when the paralysis is complete. I remember on one occasion my friend Dr Bright (who, like almost all great men, is extremely modest) did me the great honour of asking my advice upon a case which had come before him at the College of Surgeons. The symptoms in that case were —besides the extreme coldness already mentioned in one-half of the body—tingling in the fingers, very slight ptosis of the external rectus, some jerks in the muscles of the face, on the opposite side to that of the injury—a symptom you do not find when the injury is higher than the pons varolii; also some sensations of tickling in the face—another symptom you do not find in cases where the disease is higher up than the pons; in fact, there were symptoms which, to one who was then more of a physiologist than a practitioner, as it is since that time I have seen more patients, were new in the human species; but I had no hesitation, simply from the teachings of physiology, in stating it was that kind of case which I have just pointed out; and so it proved, as it gradually and successively presented all the symptoms I have mentioned of disease in the base of the brain. Not having had the advantage of making an autopsy of the case, you may, perhaps, think me very presumptuous in holding that I had made a certain diagnosis while knowing nothing but the symptoms; but really with this class of cases doubt is impossible when the symptoms are combined, forming a group so defined and distinct that there is an absolute certainty even during life as to the cause. It is not so when the disease goes higher up in the brain. We are then at a loss, and it is extremely difficult to say whether there is organic disease, or merely a temporary disorder of the circulation; but if the symptoms I have pointed out are, all of them, present, the case is absolutely free from doubt or uncertainty.

I pass on to another kind of hemiplegia. There is one kind of hemiplegia absolutely distinct from all these kinds. Suppose a patient comes to you with some slight stiffness and tendency to throw his limbs on one side; there is not a great paralysis, but rather a decided weakness, and hardly any loss of sensibility at one side of the body. He complains also of noises in the ear on the same side, of feeling extremely giddy, and having a tendency sometimes to revolve and turn round upon himself. He sometimes reels as if he were intoxicated; he very frequently cannot walk straight forward; sometimes he has also very great hyperæsthesia to sounds; he has also at times a sudden tendency to fall down; it seems to him that he cannot keep up, and that he must fall, also that if he takes hold of something he will keep up.

This kind of case is, indeed, the most instructive of all kinds of hemiplegia. I have now collected more than twenty-two such cases—not all seen by me, but a large proportion of them. According to the autopsy made in a number of these cases, they are simply cases of reflex paralysis; they are not paralysis owing to the destruction, the alteration, or the section, in fact, to the interruption of the conductors of voluntary motion. They are absolutely distinct from the paralysis which is due to the fact that the conductor between the organ of the will and the muscle is interrupted—they are produced by quite another mechanism. In this class of paralysis there is disease, either of the lower part of the brain, at the base of which lies the fifth pair of nerves, or near the place of entrance of the auditory nerve. There is in this case not a destruction but a pressure (and not a very considerable pressure) on the cerebelli, a small part of the pons varolii, and the medulla oblongata. Place a tumour there, which has encroached slightly and gradually on the neighbouring parts to those I have named, and these symptoms will appear.

But, let the injury go further, then the paralysis on one side of the body—viz., the paralysis on the side where the injury existed—will disappear; yet the injury to the base of the brain is greater now than it was in the former case; but from the moment that a real disorganization has taken place in the base of the brain the symptoms which existed at first disappear, and the paralysis passes from the right side where it existed at first to the left side, the tumour still continuing at the right side.

I regret I cannot, owing to the limited time at my command, explain the cause of this more at length, but I will endeavour to do so in a few words.

In the first case the same condition exists as where an injury exists on a nerve anywhere in the body, producing paralysis. Acting upon the brain it produces an alteration of some kind which the microscope does not detect, and by a reflex action produces paralysis. But why is it that in the second case we find the paralysis disappears at that side? The explanation is, that the part which in the first case was irritated has now been destroyed, and hence there is no more irritation, and paralysis consequently ceases on that side, but it goes to the other side of the body; because in the pons varolii and medulla oblongata there are conductors of voluntary motion above their decussation passing to go up to the brain. Hence, if an injury exists, such as to destroy some of these conductors, paralysis will occur on the other side; and it disappears on the first side, owing to the fact that the part has been destroyed and there is no longer a pressure and irritation as at first.

I intended bringing forward a great many other types of hemiplegia to show what physiology can do to explain these cases; but I am compelled to be brief. I, however, must mention one other kind of hemiplegia—that due to hæmorrhage in the cerebellum. In this case there are features which have, most of them, been found in experiments on animals, and which, if rightly read, will lead to accurate diagnosis. One is, there will be vomiting—this is a constant symptom of hæmorrhage into the cerebellum. There will also be hyperæsthesia in some parts of the body—not the whole, nor even the half, but in some parts. There will also be amaurosis, not from pressure on the tuberculi quadrigemini, but due to reflex action, as the disease in most cases does not press upon the tuberculi quadrigemini. That this is so, appears still more certain if we take into account what occurs in many of these cases of amaurosis; we may have amaurosis of the left eye alone, of the right eye alone, or of both eyes; still more, we may have amaurosis passing from one eye to the other alternately, showing, in fact, that there is no persistency or uniformity of action in the production of amaurosis in these cases.

There is another kind of hemiplegia as to which I must say a few words, it is that kind which is due to a lesion of the anterior lobes of the brain. Phrenologists, we know, have regarded the anterior lobes of the brain as organs of speech, but there are many cases—Dr Stokes mentioned to me a very striking one a few days ago—in which a destruction of these lobes took place without any loss of speech. But the question (and it is an interesting one) arises, what creates the loss of speech when such loss exists? As regards that, I shall have in a moment or two to point out how great a variety of symptoms may be produced by a lesion of almost any part whatever of the brain. This loss of speech I hold to be a mere reflex phenomenon; and of this we have a proof in the fact, that it will vary very much, even in the same patient, according to circumstances which physiology has as yet been unable to detect, but certainly with the lesion still continuing. The facts at all events prove that destruction of the anterior nerve is not the direct cause of loss of speech—it is one of the instances of reflex phenomena. It is worthy of remark that the loss of speech is usually unaccompanied by any difficulty of movement in the tongue; there is perfect freedom of motion in the tongue, and the defect of speech arises from the patient being unable to express his thoughts not only by speech, but even by signs or writing; it is a paralysis of the "organ of expression of thoughts." The patient may, notwithstanding, remain very intelligent. I some time since met a case—it was that of a clergyman—who was a remarkable instance of this. He had not absolutely lost his speech, for he pronounced words very distinctly, but they were words possessing no meaning whatever. He was likewise unable to write or even to express his thoughts by signs. When asked, for example, to express "yes" by lifting one finger, and "no" by two fingers, he was unable to do it, although he appeared extremely intelligent.

I pass on to notice another group of facts, showing the importance of a knowledge of physiology in the diagnosis of disease. There is one form of disease to the discovery of which I have been led by experiments on animals, and which I must mention. Patients may come to you complaining of pain in the back, of a pricking sensation in both arms, with some degree of itching, burning, or some strange sensation of cold and heat alternately in the skin of both their limbs. You may find some strange forms of skin affection different from those which you have usually to deal with when they are not due to nervous disease. You may find also some degree of weakness in the two upper limbs, jerkings in these limbs, sometimes also a great stiffness in some of the vessels, and they are tender under pressure. If you do not pay attention to the state of the spine—if you do not know the exact physiological meaning of all these symptoms—you will perhaps be led to suppose that there is some local affection—rheumatism, if you like—of the arms. You, perhaps, will think it very strange that heat and cold can coexist in the same organs. You are surprised you can find no description of such a disease in books. Yet the explanation is most simple: it depends altogether on an inflammation of the nerves at their exit from the spine in the lower part of the cervical region. There is in the spine sometimes local meningitis. The whole thing arises from irritation of motor and sensitive nerves, and especially irritation in the nerves of blood-vessels—this it is which produces all these symptoms. If, then, applying your physiological knowledge, you arrive at the true character of the injury, you may, by a certain mode of treatment, which presents no great difficulty and causes no great pain, cure, or at all events greatly mitigate the disease. I have met many cases of the kind, and with the exception of one, which I saw in London in conjunction with Dr Adams, and which terminated in death, all the cases have been either cured completely or more or less ameliorated. The treatment consists in the most active blistering of the spine in the region of the injury, also in the application of dry cupping all along the lines of blister. Injections of narcotics have also been resorted to, but the principal treatment consists of the application of blisters to the spine. Internally I have sometimes employed iodide of potassium, but what share it had in the cure I do not know.

I intended to bring forward other cases of reflex paralysis of the lower limbs and from inflammatory softening of the spinal cord, but time does not allow of it. I will simply say, that physiology has demonstrated these most important facts—that the spinal cord in its central part, which is decidedly insensible in its normal state, will become exquisitely sensitive under the influence of inflammation; and when sensitive, it will give rise to all those strange sensations complained of by patients attacked with myelitis or great congestion in the grey matter. When there is considerable congestion of the grey matter, or, still more, when there is inflammation, we have these symptoms, which are also the effects of irritation of the motor nerves—viz., jerks, tremblings, convulsions, contractions in the muscles, etc. All these symptoms are due to a special change in the condition of the spinal cord; they cannot be produced without congestion or inflammation; they are essential to these two diseases, and if you do not find at least a part of them you may be convinced that the spinal column is free from congestion, and free still more from myelitis.

I proceed to notice two kinds of cases of fracture of the spine absolutely distinct one from the other, though the injury in both is in the neck.

Two patients are brought to you, having sustained a fracture in the cervical region. One of them is almost pulseless, extremely cold, covered with a clammy perspiration, his limbs lie loose and dead, there is no contraction, no rigidity, his breathing faint. If you bleed him, you will find the venous blood red like arterial blood, flows out, not with a great impulse, for the impulse of the heart is extremely weak and almost in a state of syncope; but still the blood does not flow in the same way as venous blood—it has an impulse.

Now, examine the other patient. Here the symptoms are directly contrary to the former. The limbs are stiff and rigid; the pulse extremely high; the heart's action excited; the heat of the body not only in the extremities higher than is usual, but absolutely higher than the temperature of the blood in health in man.

Now, what is the explanation of these two cases? It is found by experiments performed on animals. In the first of the two cases there is irritation—perhaps extremely slight, the slightest prick will be sufficient—on the spinal cord. The effect is, stoppage of the heart's action, so that it beats with less force and rapidity, and, as consequences of this condition of the heart, all the other symptoms above described ensue. In the other patient, on the contrary, the spinal cord has been cut across, and the patient is in a much worse state, in reality, than the other; still he seems to be far more alive than the other. He seems to have the power of reaction which we wish to find in patients; yet the danger of his position is far greater—in fact, he is sure to die; while the other, by means of an operation (which was performed to-day upon a patient in this city), may survive.

I proceed to make some remarks on the production of symptoms of brain disease. As you well know, our view of the production of symptoms of brain disease—disease in the brain proper—is, that a disease there produces paralysis by striking the organs of the will, and that there is a paralysis of the will, at least for that part of the body which is paralyzed—that, if other symptoms occur, such, for instance, as any form of insensibility, or any form of convulsions, the connexion of the part is altered or perturbated in some way, and owing to that alteration, this or that form of insensibility or convulsions will occur. All the symptoms of brain disease, at least all of them that I know of, either alone or united one with another, or grouped just as you may fancy to group them, may be due to simple reflex action. I have not time now to demonstrate this, but I shall demonstrate, I hope fully, that they are *not* owing to loss of function or pressure upon neighbouring parts.

You are all familiar with the great variety of symptoms presented in brain disease. Take, for instance, facial paralysis in cases of disease of the brain. Facial paralysis, as you know, does not exist in the orbicularis, but in the other muscles of the face. Now, if you say that in cases of disease of the anterior lobe of the brain (as is certainly the case) there is facial paralysis, because the nerve fibres of the facial nerve go to that part—well, I am perfectly willing to admit it; but let us take a case of injury to some other part of the brain—take a case of injury in the posterior lobe, and here too we find facial paralysis. How will you explain this? Do the nerve fibres of the facial nerve go to the anterior lobe in one and to the posterior in another individual? Certainly not. The result of such a hypothesis would be, that there is absolutely no part of the brain which would not be the spot to which the nerve fibres of the facial nerve go. If you imagine such a thing possible, I wish you would reconcile the facts with what anatomy teaches. Anatomy teaches that the facial nerve goes to a certain part of the pons varolii, so that besides (excuse the word) the absurdity of supposing that the facial nerve extends to every part of the brain, and each part containing all those fibres—besides this absurdity, there is the anatomical impossibility which we see when we examine the condition of the facial nerve. It is absolutely impossible, in fact, that such a hypothesis can be correct. Again, take another instance; the tongue, as you well know, is more or less paralyzed in most of these cases of disease of the cerebellum: there is some difficulty in drawing it out in a straight line, also some slight impediment in the speech owing to the paralysis of some of the fibres of the ninth pair of nerves. How can you reconcile the existence of that paralysis in most cases with the fact that we do not see the fibres of the ninth pair of nerves—the hypoglossal—going up higher than the medulla oblongata? Here is a patient whose pons varolii is completely injured—mark that the pons varolii must be the place of passage—if there is any such passage—of the fibres of the hypoglossal pair going up to the brain. If, therefore, one-half of the pons varolii is congested by disease there must be a paralysis of the tongue, yet there is no paralysis of the tongue in that case, so that both anatomy and this physiological fact prove that the hypoglossal nerves do not go to the back of the brain. How, then, is it that disease in *any* part of the brain may (as we know it does by experience) produce paralysis of the hypoglossal pair? I leave it to you to decide. To me it seems quite clear that to hold that when an injury to a part of the cerebellum causes paralysis of the ninth, or any other nerve, it is because the paralyzed nerve goes to that part of the brain, is decidedly wrong, and that there is no way of explaining the phenomena without admitting that irritation starting from any part whatever of the body may cause paralysis of any other part, *e.g.*, that irritation of any part of the bowels might produce paralysis of the tongue. If you examine a number of cases of disease of the brain, especially of the active form, such as cases of tumours producing irritation, and especially tumours in the membranes of the brain, you will find that, for a tumour in one and the same part of the brain, there is no symptom produced in some cases, while in others you will find any symptom whatever. I do not think you could put your finger on any form of nervous complaint that you will not find to exist in some one or other of the cases of injury in any part of the lobe of the brain. Still more, with the same persistent disease you may have during the course of the life of the patient, whether he is to recover or die, you may have a great change. The patient may be paralyzed to-day, another day he may not; you may, in fact, have every variety of phenomena, or no phenomena at all,—all arising from one and the same cause, and in the same individual, so that unless you suppose that each individual part of the brain possesses every function whatever, and has the effect of reacting on every part of the body in a direct way,—unless you prove that each part of the brain is connected with the fibres of the whole body, you cannot understand these facts; and mark, in some cases of injury to the cerebellum, there are no phenomena at all, so that while, on the one hand, you must suppose that there is no part of the brain which does not contain all the nerves of the body, you are, on the other hand, forced to conclude, that there is no part of any nerve of the body going to the cerebellum. Such a hypothesis is obviously impossible. You are aware that the explanation I have ventured to offer of these phenomena is that they come under the class of those produced by reflex action. I shall present a few other instances of a similar kind; and first, a few words as to syncope, as induced by a blow on the stomach, which is nothing but syncope by reflex action. In these cases experiments have shown that the syncope is produced by reflex action through the abdominal sympathetic ganglion acting through the par vagum, medulla oblongata, and spinal cord. I have often and often tried the experiment by crushing the ganglion of the sympathetic in the abdomen. In such cases there was sometimes a sudden arrest of the heart's action, in other cases only a temporary diminution in the beating of the heart, in other cases, again, there was hardly any effect produced. Again, in those animals in which I observed the effect to be produced, I waited till recovery was established, and I then divided the ganglion and crushed the par vagum; no effect was then produced on the heart's action, clearly showing that the transmission takes place through the par vagum.

In those cases in which the heart is stopped, whether from the cause above assigned or any other,—I mean cases of syncope, I do not mean death, when of course the stoppage of the heart's action is a definitive one,—but those conditions which are on the verge of death, and which lead to death if nothing is done to relieve it,—in those conditions there very frequently are means of restoring life. I have ascertained in animals very frequently that though the heart is then quite stopped, we can, by simply pressing on the sternum, and by giving a hard push to the heart, make it beat. It will not beat long if the cause of the syncope is a powerful one; but beat it will, and if you continue the cause of irritation it will continue to beat, and in that way you may often revive the patient. But this is not all. If you add to that cause of revival another which is most powerful, and which is directly the reverse of what John Hunter did upon himself when he found he was in a state of syncope one day at college,—if, instead of breathing as quickly as you can, you stop the patient's breathing altogether, just as if you were trying to kill him by suffocation, you revive him, by producing a state of asphyxia, the patient is saved, he will have a struggle, and will come out of it very quickly. Nothing, indeed, is more powerful to make the heart beat than an accumulation of carbonic acid in the blood. Whether I have been right or wrong in maintaining the principle, that the normal and abnormal beatings of the heart when very tumultuous depend chiefly upon an accumulation of carbonic acid in the blood, —whether I am right in this respect or not, there is no question that if you produce partial suffocation in these cases, you make the heart beat again, and beat with force. I should add that I have not the merit of having discovered this fact, as I find that in an old book, published some two hundred or three hundred years ago, an English surgeon has mentioned this fact as very important. He, however, does not say on what he grounded his view. There are some other features about syncope of great importance. If there is little blood circulating you may in a moment throw something like one or even two pounds of blood into the heart by simply pressing on the four main arteries of the body. If you press those four arteries you prevent circulation going on in them, and at once an immense quantity of blood returns from the venous system to the trunk, and there is an immediate revival.

A few words now upon asphyxia. There are experiments which show, as clearly as possible, that if you take two animals, one of them having had its temperature very much diminished, the other at a normal temperature, dip them both into water at the same time, the one having its temperature very low will survive the other twice, three times, and sometimes even five times; the duration of life under water being extended sometimes to twelve or fifteen minutes. The greater the previous diminution of temperature the longer the duration of life. There is another fact which is a very interesting one. It is well known that persons who have fallen into very cold water have in many cases been drawn out and revived after a number of minutes' immersion. Now, in experiments performed upon animals by applying galvanism to the part, so as to stop the heart's action, which is just the effect a fall into water will produce, we find life will last much longer, the animal will be able to survive a much longer stoppage of the heart's action from having had an attack of syncope just before the asphyxia. This case, then, is exactly the reverse of the former. In one, syncope was cured by asphyxia, in the other, asphyxia is less mortal, because syncope previously existed.

In cases of death by asphyxia, if the temperature is low, there is one fact very similar to what we see in cases of sudden emotion producing arrest of the heart's action when the individual falls into cold water, and that is, that in the two cases—diminution of temperature, or dipping into water—the heart beats very much slower. Any patient attacked with asphyxia, whose temperature is much diminished, has a slow beating of the heart, and whose blood is red, is not exactly an asphyxiated patient; there is a mixture of syncope and asphyxia, and the patient has much more chance to recover. If you try to raise the temperature of such a patient you run the risk of killing him.

I shall now say a word upon poisoning. Poisoning is often the cause of death by producing such diminution of temperature as is incompatible with life. Take, for instance, two animals which have been poisoned with the same quantity of opium. Supposing the temperature to be cold in the room, lay them both on a table, one covered carefully with warm clothes, the other exposed to the cold, you find, *cæteris paribus*, that the one which is kept warm will survive, while the other will die. This fact we find with almost every poison of an organic nature, that there is considerable diminution of temperature produced, if not *per se* sufficient to cause death, enough, at any rate, to add a powerful cause to the other causes existing. Now this diminution of temperature is a feature which we can fight against, and it is therefore of the utmost importance in cases of poisoning to use every means to keep up the temperature of the body.

I am now obliged to stop this exposition. My object was to show that by the knowledge derived from experiments on animals, from pure physiology, as well as from the knowledge derived from other researches than those made on animals, the knowledge we derive from microscopic anatomy, and even from simple descriptive anatomy, as regards the base of the brain especially,—from all these sources, combined with the study of pathological cases at the bedside, —from all these facts we can indeed advance with great rapidity, and we are enabled to form a sure diagnosis in many otherwise obscure cases. I may illustrate the importance of the knowledge of physiology, especially of the advances made in the science of late years, by the successful practice of many men in this city and in England; I shall not attempt to mention names, as no doubt the names of the men to whom I allude are present to your memory. To follow in the footsteps of these men, doing what they have done, and perhaps giving more time than they have given to the study of physiology, will give to you the greatest help both in your diagnosis and prognosis, and what will perhaps prove a still greater comfort to you, your conscience will always be at ease when you meet a complicated case. I hope that, as I have been speaking to many men of eminence,—men much more advanced than I am in life, in knowledge, and in practice,—I hope what I have said will not hurt their feelings, and that they will find an apology for me in the fact that, as I have been a physiologist before becoming a practitioner, I have been able perhaps to find more than most of you the advantage of that science. I hope therefore there will be no offence in my conclusion.—*Dublin Medical Press.*

SURGERY.

EXCISION OF THE TONGUE. BY PROFESSOR SYME.

Some years ago I endeavoured on two occasions to afford relief from disease of the tongue, otherwise incurable, by cutting out the entire organ; but, as both cases terminated unfavourably, I felt no desire to repeat the experiment, and have repeatedly declined doing so under circumstances of a very urgent character.

In the early part of November last, Mr W——, aged fifty-two, from Manchester, applied to me on account of a very formidable morbid condition, affecting his tongue. From its point to the root it was swollen and indurated, the surface being of a brown colour and roughly tuberculated, so as to resemble the back of a toad. It was also nearly quite immovable, and, from completely filling the mouth, not only prevented articulation, but rendered deglutition impossible with respect to solids and extremely difficult in regard to fluids. From the same state of matters, there was a most offensive fetor through mucus secreted by the unhealthy surface not being permitted to escape.

The patient informed me in writing that he had suffered from uneasiness in his tongue for many years, but that neither articulation nor deglutition was seriously affected until 1862, since which time he had been under medical treatment in London as well as Manchester without experiencing any benefit. As palliation seemed all that could be expected, I offered some suggestions with this view, and advised that no time should be lost in returning home. But soon after his arrival there I began to receive from the patient very painful letters, reporting aggravation of the symptoms, especially in regard to deglutition, so that death from starvation seemed imminent, and urgently desiring some means of relief. To these appeals I replied that the only effectual remedy was removal of the tongue, and that this could not be done without very serious danger to life, so that the operation promised nothing more than a chance of escape. This slight encouragement brought the patient back, and he arrived here on the 27th of December.

Being thus as it were compelled to make another trial of excision, I carefully considered all the circumstances concerned that might tend to interfere with its successful performance. Of these the one which most prominently presented itself was the prevention of voluntary deglutition that must result from depriving the os hyoides of the power by which it is drawn forwards. In the common cases of cut-throat, where a large tranverse wound is made into the pharynx, although the suicide rarely accomplishes his object in the first instance, he still more rarely escapes the fatal effect of pulmonary inflammation induced by irritation propagated from the larynx; and I did not forget that both the patients on whom I had performed the operation in question died from purulent effusion into the lungs. Instead, therefore, of cutting through all the muscles of the os hyoides, as had been done in the former cases, I resolved to retain the mylo-hyodei and genio-hyodei entire, and divide merely the attachments of the genio-hyoglossi. I also thought it would be better to perform the operation without chloroform, since the patient, instead of lying horizontally, might thus be seated on a chair, so as to let the blood run out of his mouth and not pass backwards into the pharynx.

The operation was performed on the 29th, with the assistance of Mr Annandale, Dr Sewell, and Mr Cheyne, to the first of whom I am especially indebted for his able co-operation. Having extracted one of the front incisors, I cut through the middle of the lip and continued the incision down to the os hyoides, then sawed through the jaw in the same line, and insinuating my finger under the tongue as a guide to the knife, divided the mucous lining of the mouth, together with the attachment of the genio-hyoglossi. While the two halves of the bone were held apart I dissected backwards and cut through the hyoglossi along with the mucous membrane covering them, so as to allow the tongue to be pulled forward and bring into view the situation of the lingual arteries, which were cut and tied, first on one side and then on the other. The process might now have been at once completed had I not feared that the epiglottis might be implicated in the disease, which extended beyond the reach of my finger, and thus suffer injury from the knife if used without a guide. I therefore cut away about two-thirds of the tongue, and then, being able to reach the os hyoides with my finger, retained it there while the remaining attachments were divided by the knife in my other hand close to the bone. Some small arterial branches having been tied, the edges of the wound were brought together and retained by silver sutures, except at the lowest part, where the ligatures were allowed to maintain a drain for the discharge of fluids from the cavity.

Next day I visted the patient, and finding him in all respects comfortable, inquired if he could swallow. In reply he pointed to a drinking-cup containing milk, and intimated that he wished it to be filled; then, placing the spout between his lips, while his head was bent backwards, he drank the whole without any cough or sputtering. Having seen this, I felt assured that the result would be satisfactory, and was not disappointed, as everything went on well afterwards. The only inconvenience experienced was from the edges of the jaw being occasionally displaced; but this was easily remedied by an ingenious contrivance of Mr Wilson, the dentist, who, finding that a silver cap enclosing the teeth, was not sufficient for the purpose, fashioned a shield of gutta-percha, embracing the chin on each side, and secured to the metal plate by a wire.

Under an ample supply of nourishment by milk, soup, and soft solid food, there was a rapid return of strength, so that an improvement in this respect was almost daily observable, and before the end of three weeks the patient declared that he had never felt better in his life. He returned to Manchester on the 23d of January.

Excision of the tongue has thus afforded complete relief in a case of the most formidable and distressing disease. How far the relief thus obtained may prove permanent, and how far it may admit of being extended to cases of a similar kind, are questions that can be determined only by experience. But the frequency of malignant growth affecting the tongue in an otherwise sound state of the system urgently requires the truth to be ascertained in regard to the value of a remedial measure; and if the operation is now, as I trust it has been, freed from the chief danger attending its performance, facts sufficient for the purpose will probably erelong be accumulated.—*Lancet.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLIV.—MEETING IV.

1*st February* 1865.—Professor Douglas Maclagan, *President* of the Society, in the Chair.

I. SPECIMENS OF TUMOURS.

Professor Spence exhibited three tumours which presented special points of interest.

The *first* specimen was a tumour of the testicle. The disease occurred in a sailor in one of Her Majesty's ships. The patient, about the end of July last, while engaged aloft in shifting topsails, received a blow on the left testicle, which caused sharp pain at the time, accompanied with a sensation of faintness. This soon passed off, and he experienced no farther inconvenience for several days, when, after having been much exposed to cold in the performance of his duties, the pain returned, accompanied with swelling. He was sent to sick-quarters at Berwick-on-Tweed, and it was ascertained that a hydrocele had formed. It was tapped, and a good deal of clear serum was evacuated; but a hard swelling about the size of a walnut remained at the lower part of the testicle. The operation relieved him for a time, but the swelling soon returned, and a fortnight afterwards it was again tapped. On this occasion the fluid contained a considerable admixture of blood; the tumour formerly recognised was now about the size of an egg. The patient returned to his duty, and three weeks afterwards, whilst aiding in the rescue of the crews of three vessels wrecked during the October gales, he was not only exposed to a thorough drenching and extreme fatigue, but received a blow on the scrotum from part of the rocket apparatus. A few days afterwards, the swelling increased; he was again tapped, but on this occasion only a little blood came away. The operation gave no relief; on the contrary, the symptoms were aggravated, and the swelling became larger and more tense. The patient was then sent to Edinburgh, and came under Mr Spence's care on the 1st of December last. At this time a large swelling, ten inches in length and seventeen in circumference, was found to occupy the scrotum; there was a feeling of fluctuation at top and bottom, but the tumour had a doughy sensation in the intermediate part. The characters of the swelling resembled in some respects those of a hydrocele, but from the opacity of the lower part when examined by light, and from the other circumstances of the case, it was considered that it might be a hæmatocele, or more probably a tumour of the testicle. Accordingly, in operating, Mr Spence was prepared either to treat it as a hæmatocele or to remove the testicle. A long incision was made over the tumour where fluctuation was most distinct. The sac was punctured, and a quantity of clear serous fluid was evacuated. On introducing a finger into the opening, the tumour was distinctly felt to be of the gland itself, and the testicle was accordingly removed. After removal the tumour was put in spirits to be hardened, and a section had been only made to-day. The appearance was peculiar, resembling more than anything else an effusion of blood into the body of the testicle. Still, Mr Spence looked upon it as a medullary growth; what would formerly have been called a fungus hæmatodes of the testicle. A microscopic examination had not yet been made. The wound healed rapidly, and everything went on well till ten days ago, when the patient began to have cough and pain in the chest, accompanied with bloody expectoration. On examination, there was found to be crepitation over the lower part of the left side of the chest; the expectoration, however, was not of a pneumonic character. The patient had been transferred to a medical ward, and was now under Dr Haldane's care.

The *second* tumour was of a peculiar character. It was removed from a man about sixty years of age, who came in from the country on account of the occurrence of hæmorrhage from the growth. For many years the patient had had a small tumour on the front of the forearm about two inches below the bend of the arm. It was evidently connected with the skin, and appeared to have originally been a nævus. The bleeding, which was both venous and arterial, took place from a small ulcerated opening. The tumour was at once removed, when there was a good illustration of the fact, that the vessels going to such growths are not enlarged, but that they become dilated within the tumour; for although the hæmorrhage from the growth had been severe, there was little or no bleeding from the vessels cut across in the operation. The tumour, it would be seen, was solid below, the erectile tissue being confined to one part.

The *third* specimen exemplified the bad effects of deferring amputation in cases of malignant disease. About two years ago, a boy was brought to Professor Spence with medullary disease of the arm. It appeared to have begun in the soft parts, but at that time had involved the bone. Amputation at the shoulder-joint was recommended, but the father of the patient would not hear of it, and the boy was removed. About the beginning of this session he was brought back in consequence of bleeding having occurred from an ulcerated surface, and the father urgently requested that the disease might be removed. Amputation at the shoulder-joint afforded the only chance of prolonging the boy's life, but the ultimate result was hopeless. It was found that the tumour passed pretty deeply into the axilla; as much as possible of the muscular tissues were removed, in particular nearly all the deltoid. There was not that line of definition which could have been desired. For a fortnight after the operation the patient went on well; then rigors occurred; the flaps opened; ulceration extended, and the axillary artery was opened into; this, however, had been anticipated, and the vessel was secured higher up. The rigors continued, and the boy died of pyæmia in the fourth week after the amputation. If the operation had been performed earlier, although the disease might probably have recurred elsewhere, it would doubtless have prolonged the boy's life, and rendered it more comfortable.

II. WOUND OF THE BRACHIAL ARTERY.

Professor Spence showed an arm which he had removed at the shoulder-joint on account of a severe railway injury, and which illustrated a lesion of not very frequent occurrence, although the wonder was that it did not occur oftener. It was that the brachial artery was wounded at a point corresponding to the fracture of the humerus, and that the circulation below the part had been interfered with. When admitted, the lower part of the limb was cold, and as there was only one course to pursue, amputation was performed at the shoulder-joint about a week ago. Though much of the muscular tissue had been removed, some sloughing had occurred. Although the patient was not in a very favourable condition, he was perhaps as well as could be expected under the circumstances, as, in addition to the local injuries, there was always great shock after railway accidents. The constitutional irritation had probably been occasioned by the sloughing.

III. CASE OF IDIOPATHIC PULMONARY GANGRENE.

Dr Haldane showed a specimen of gangrene of the lung, the result of pneumonia. The patient, from whose body the preparation was obtained, a shoemaker, 50 years of age, of somewhat intemperate habits and broken-down constitution, was admitted into the Royal Infirmary, under Dr Haldane's care, on the 11th of January last. He had turned ill on the 7th, and on admission complained of pain and oppression in the chest, and presented the appearance of considerable prostration. His respirations were 40 in the minute; pulse above 100, rather small and soft; he had much cough, with pretty copious, tenacious, rusty expectoration. Percussion over upper half of right front impaired, with fine crepitation and increased vocal resonance. The left lung seemed quite healthy. Only a trace of chlorides was found in the urine. A large linseed-meal poultice was directed to be kept constantly applied over the right side; he was ordered a mixture containing aqua acetatis ammoniæ, spirit of nitrous ether, and a little tincture of hyoscyamus. Diet to consist of milk, beef-tea, and six ounces of wine. Next morning he was weaker than on the previous day; he had suffered from severe diarrhœa during the night, bowels having been opened ten or twelve times. For this chalk-mixture with electuary of catechu was prescribed. Dulness on percussion over right front was better marked than on admission, and in the dull space bronchial breathing and bronchophony were audible. On the morning of the 13th there had been no return of diarrhœa, but he had been delirious during the night, and at visit he was still very restless and incoherent. Pulse weak; expectoration copious, less tenacious, but of the colour of prune-juice. Shallow tympanitic percussion was well marked under the right clavicle, with hoarse blowing breathing and occasional moist sounds. He was ordered some brandy in addition to the wine. From this time till his death, which occurred on the 21st of January, the following changes in the physical signs took place:—Shallow tympanitic percussion was recognisable over the upper half of the right front; in this situation harsh tracheal breathing, with occasional gurgling, was audible. Bronchophony was so loud as to be almost painful; but there was no approach to pectoriloquy. The sputa changed from the colour of prune-juice to ash-gray, dirty green, and finally to a greenish yellow, the odour becoming fetid, and latterly was characteristically gangrenous. There never was any great degree of fetor of the breath. The patient grew gradually weaker, and died, as already stated, on the 21st of January. At no time was there any hæmoptysis. The body was examined on the 23d. The right lung did not collapse when the chest was opened; it felt generally solid. In removing it the pleura gave way at two or three points over the upper lobe, and from these openings drops of fetid pus escaped. No air had escaped from the chest when it was opened, and the right lung quite filled that side of the thorax. On section, numerous gangrenous cavities were found in the upper lobe of this lung. Those in the apex, and for two inches below it, were quite superficial, were of small size, but communicated freely with one another, and occupied nearly the whole thickness of the lung. Below this part was a single cavity about the size of a walnut situated about three quarters of an inch from the surface of the lung. The remainder of this lobe was in the condition of red passing into gray hepatization. The rest of this lung was in the state of solid œdema. In the lower lobe of the lung were a few small purulent deposits. They were situated immediately below the pleura, were generally about the size of peas, and their contents were not fetid. The left lung was free from adhesions, and perfectly healthy. All other organs were natural. In reference to this case, Dr Haldane remarked on the comparative rarity of idiopathic gangrene. The patient whose case had been narrated presented, however, the combination of circumstances most favourable to such an occurrence. He was of broken-down constitution, of intemperate habits, and had worked in a very badly-aired and unwholesome apartment. He had been under Dr Haldane's care during part of December on account of dyspepsia, and had been dismissed somewhat relieved at the end of the year. He had then been exposed to cold and wet, and contracted his fatal illness. The prognosis from the first was unfavourable: the previous habits of the patient, his weak state on admission, the locality of the disease, the diarrhœa, the early occurrence of delirium, and the prune-juice colour of the sputa, were all circumstances calculated to inspire the greatest anxiety. Accordingly, when lecturing clinically on the case on the 13th of January, Dr Haldane had expressed the most gloomy opinion regarding it. The diagnosis of gangrene was arrived at without difficulty. Shallow tympanitic percussion succeeding to dulness appeared to indicate superficial excavation; while the odour of the breath, but still more of the expectoration, following the signs of pneumonia, indicated that the excavations were of a gangrenous character. From merely looking at the lung after its removal from the body, the idea that the gangrene was of secondary or metastatic origin might have been suggested. This opinion, however, was not borne out by the history of the case, and it was farther negatived by the absence of any source of purulent infection, and by the perfectly healthy condition of the left lung. The case must therefore be regarded as one of idiopathic gangrene.

IV. CASES OF PNEUMONIA.

Dr Haldane observed, that while the patient whose history had just been recorded was under treatment, other three cases of pneumonia had been admitted under his care, and he might be permitted to lay a very brief notice of these cases before the Society.

I. John B., a groom, 19 years of age, had rigors on the 7th of January, followed by some pain and oppression of chest, cough, and difficulty of breathing. When admitted on the 11th January his countenance expressed considerable anxiety; he had much short cough, with rather scanty, tenacious, rustcoloured expectoration. There was comparative dulness on percussion over the lower two-thirds of the left back; in the upper part of this region fine crepitation was audible, while over the base of the lung there was bronchial breathing. Next day he appeared somewhat relieved; he had sweated moderately, and stated that he had not so much uneasiness in the chest. Physical signs unchanged. Early in the morning of the 13th he had a rather smart attack of diarrhœa. This had ceased at the time of visit, but he was then found sweating profusely. Accordingly the chest was not examined; but all the general symptoms were much ameliorated. He made a most satisfactory recovery, and was dismissed, by his own desire, on the 27th of January. His chest was examined on the 26th; the dulness over the left side had completely disappeared; healthy vesicular breathing was everywhere audible; the only morbid sound audible was an occasional subcrepitant rale.

II. W. S., apprentice to a sweep, 16 years of age, was admitted to the Surgical Hospital on the 11th January, on account of a congenital cleft in the palate; as he was evidently suffering from a severe chest affection he was sent up to the medical house, and came under Dr Haldane's care. When seen on the 12th he was in a state of considerable depression; respirations 40 in a minute; a good deal of cough, with scanty rusty expectoration; face pale and puffy; pulse 120, weak. His urine was tested, and found free from albumen; but contained only a trace of chlorides. His skin was exceedingly dry and harsh; over the back and shoulders it was of a greyish black colour, absolutely engrained with dirt, so as to resemble the state of the surface in ichthyosis—(he afterwards stated that he did not remember having ever had his shoulders or back washed, and that he had never in summer bathed in the sea). Percussion was dull over the lower two-thirds of the left back, and in this situation only bronchial breathing was audible. On the following day (the 13th) dulness was as before; but in addition to bronchial breathing a little coarse crepitation was audible with the expiration. From this time he progressed favourably, bronchial breathing was gradually replaced by coarse crepitation, which in its turn disappeared, so that when examined yesterday (31st January) it would have been impossible to determine by the physical signs which lung had been affected. He had been going about the ward for the last eight days, and was only retained till he could be re-admitted into the Surgical Hospital.

III. J. G., a labourer, 23 years of age, stated that he had been ill for five or six days before his admission; that his illness had commenced with rigors, followed by sharp pain in the chest, and cough. When admitted on the 11th January he complained of cutting pain in the left side, much increased on full breath. There was much short cough, with a little rusty expectoration. There was dulness from a little below the middle of the scapula to the base of the left lung. Inferiorly the dulness was absolute; on passing upwards it gradually diminished, and shaded off into healthy resonance. At the base of the lung distant bronchial breathing was audible; superiorly there was fine crepitation with the inspiration. On the 12th and 13th he sweated much, with great relief to all the symptoms, and on the morning of the 13th there was a copious deposit of lithates in the urine. He made an excellent recovery, and was dismissed, at his own request, on the 25th January. His chest was examined the day before, and, with the exception of slight comparative dulness quite at the base of the left lung, everything was quite natural.

In reference to these cases Dr Haldane remarked that they were good examples of pneumonia, proceeding uninterruptedly to a favourable termination. In the second case only had there been any cause for anxiety. The patient's pallor and puffy appearance suggested the idea of Bright's disease; and although there was no albumen in the urine, he was evidently not in a satisfactory state. Next day, while the general symptoms were much the same as on admission, returning crepitation indicated that resolution was beginning In this case Dr Haldane thought it probable that the dry and unperspirable condition of the skin, induced by a total absence of cleanliness, had exerted an unfavourable influence. In all these cases the treatment was essentially the same, consisting in the continuous application of large hot poultices to the chest;

the use of a mixture containing aqua acetatis ammoniæ and nitrous ether, to which, if there was much irritation, a little tincture of hyoscyamus was added; and a nourishing non-stimulating diet. In the second case a little wine was at first prescribed, but it was stopt as soon as resolution commenced. Dr Haldane attached great importance to the poultices, which, to be efficacious, must be kept continuously applied. They relieved the pain, and were very powerful diaphoretic agents. In none of these cases had antimony been administered. Without denying that there were occasionally cases where it might be useful, Dr Haldane, as a rule, avoided it, as he thought that its good effects could generally be obtained by other means, without the risk of the depression which antimony often induced.

Before concluding, Dr Haldane might give a short account of another case of pneumonia which had been under his care during the month of December. The patient, a sailor, 24 years of age, was admitted on the 28th December 1864. He was brought from the Sailors' Home at Leith, and was stated to have been ill about eighteen days, but could give no account of himself. His face was somewhat livid; the extremities were cold; difficulty of breathing was very great; respirations 60 in the minute. He could scarcely speak owing to the difficulty of breathing, and at each respiration his nostrils dilated widely. Pulse 120, moderately full, but soft. From his weak state the examination of his chest was necessarily incomplete. There was puerile breathing anteriorly, while posteriorly there was dulness on percussion and bronchial breathing over the lower three-fourths of the left lung; over the right back percussion was impaired, and coarse crepitation both with inspiration and expiration was audible. As the dyspnœa tended to prove speedily fatal, Dr Haldane determined on bleeding the patient; accordingly, a vein at the bend of the arm was opened, and about ten ounces of blood were drawn. A little wine was given before the operation. The relief was immediate; the patient's countenance became less anxious and less livid; he expressed himself as much relieved; the respirations fell from 60 to 40 in the minute. A large poultice was directed to be applied to the chest; a mild diuretic and diaphoretic mixture was prescribed; beef-tea and wine were ordered to be freely administered. The blood drawn became, on standing, intensely buffed and cupped; but the proportion of serum was large. When seen in the evening the patient was found to have passed a somewhat restless afternoon. He had slept at intervals, but had raved a good deal. Dyspnœa was greater than immediately after the bleeding, though less than immediately before it. Respirations 50 in the minute. Pulse 130. There was little change till immediately before his death, which took place at midnight, apparently by a mixture of asthenia and asphyxia. On examining the body about half a pint of turbid fluid was found in the left pleura, that membrane over the greater part of the lung was coated with soft recent lymph. The lower two-thirds of the lung were in the state of grey hepatization passing into purulent infiltration. The upper third was congested. Nearly the lower half of the right lung was in a state of solid œdema. The pericardium contained six or eight ounces of turbid serum; the whole surface of the membrane was coated with soft recent lymph. All other organs were natural. Dr Haldane remarked that this was a melancholy example of a case of severe inflammation allowed to run its course uninfluenced by treatment, and probably aggravated by neglect. When first seen the patient was in a nearly hopeless condition. The duration of the disease, the character of the general symptoms combined with the physical signs, indicated the existence of very extensive pulmonary disease. The only treatment which seemed to have any chance of success was to relieve the urgent dyspnœa, and, at the same time, to keep up the strength of the patient. To fulfil the former indication, blood was drawn; and the relief afforded far more than counterbalanced the weakening effect of withdrawing a few ounces of blood. The relief, however, was only temporary, or rather the case had advanced too far to admit of any but a fatal result, which was not long delayed.

In reference to the above cases (including that of gangrene of the lung) Dr Haldane remarked that they afforded a good illustration of the fallacy of an indiscriminate application of the statistical method to the determination of the value of different methods of treatment. The extent of disease and its character varied so much in different cases; there were such differences in constitutional power; so much depended on the favourable or unfavourable nature of external circumstances that it was really impossible to draw any definite conclusions from a number of cases massed together. It was only by a clinical record of cases of disease that our therapeutical knowledge could be advanced. The above constituted the last five cases of pneumonia treated by Dr Haldane; of these, two had died; but would it be fair to conclude that his mortality was 40 per cent.? Certainly not; for the two fatal cases were manifestly exceptional. The constitutional power of the first patient was so low, that, taken in connexion with the severity of the disease, probably no method of treatment could have prevented the fatal result; while the last patient was not seen until the period when treatment would have had a chance of being successful had gone by. On the other hand, the three patients who recovered had presented some exceptionally favourable features; the patients were young, of good constitutional power, had been well nourished, and were seen early in the disease. And so it must be in all series of cases; in some, circumstances would be exceptionally favourable; in others, the reverse; and it was impossible with such conditions to strike an average and prove by statistics that a particular plan of treatment was best suited for a particular disease.

V. DISEASE OF THE AORTA FATAL BY ANGINA.

Dr Haldane showed the heart and aorta of a man who had died from angina. The patient, a bookbinder, thirty years of age, of small size, and rather effeminate appearance, was admitted into the Royal Infirmary under Dr Haldane's care on the 21st of last December. For the two previous years he had been out of health, suffering from pain in the chest, with occasional palpitations, and during that period had but rarely been able to work. He had never had a distinct attack of rheumatism, but had occasionally complained of pains in the joints. On admission, he complained of pain, chiefly referred to the epigastrium darting upwards to the clavicles and extending to the shoulders. When examined there was no extension of the cardiac dulness; a loud diastolic murmur was audible at the base. While in the hospital this murmur diminished considerably in intensity, and evidently followed the second sound, which was distinctly audible. Heart's sounds at the apex were unaccompanied by murmur, but were feeble. The pulse was weak and did not present the characters usually met with in aortic regurgitation. The stomach was found somewhat distended, and he complained of dyspeptic symptoms. He was ordered tincture of the muriate of iron, with an occasional sedative and antispasmodic. Very shortly after admission he began to be affected with paroxysms resembling angina, characterized by severe pain and accompanied by difficulty of breathing and sense of great prostration. These attacks occurred almost daily, and lasted from a few minutes to half an hour. On the evening of the 27th of January, while conversing with his wife, a paroxysm came on, and he died before the arrival of the house physician, who had been immediately summoned. The attack had lasted from three to five minutes. The body was examined on the 29th. The heart, of natural size, contained a moderate quantity of blood on each side. The aorta was not dilated. On opening the heart the orifices were of natural size, and the valves were healthy, with the exception of the aortic, which were slightly incompetent. These valves (aortic) were all a little thicker and more opaque than natural, but were of natural size, with the exception of the left, which was a little shortened and must have been the cause of the incompetency. On laying open the aorta, its inner surface was found very irregular owing to the presence of very numerous, somewhat prominent, patches of semi-transparent thickening, so that scarcely any part of it retained its normal appearance. This condition began at the very commencement of the vessel, and extended through the greater part of its thoracic portion. In consequence of this diseased state, the origin of both coronary arteries was contracted, that of the right being barely the tenth of an inch in diameter, that of the left being less than half that size. Immediately beyond their origin these vessels acquired their natural calibre, and their coats were healthy. The origin of the left carotid and subclavian, and of several of the intercostal arteries, was contracted from the same cause. The left ventricle of the heart was very slightly dilated, and its wall a little thinned. Examined microscopically, the muscular tissue of the heart was not distinctly fatty, but the striæ of the muscular tissue were less sharply marked, and the fibres were more granular than natural. Dr Haldane remarked that this case presented several points of interest. He regarded it as essentially one of chronic aortitis, where, had the patient lived, calcification would have occurred, or fatty degeneration and softening would have taken place in the interior of the prominent patches, and the so-called atheromatous ulcers would have been formed. In general, this process was not characterized by any special symptoms, although it seemed probable that in some cases a certain amount of pain might accompany it. The cause of the peculiar symptoms in the present case was, in Dr Haldane's opinion, the great contraction of the origin of the coronary arteries. In the healthy state the left coronary was slightly larger than the right (according to Bizot, in the proportion of about five to four), but in this case, while the vessel on the right side was smaller than natural, that on the left side had not more than the half of the diameter of the right; and in reference to this it was interesting to remark that the left ventricle had become decidedly atrophied. The vessels immediately beyond their origin dilated to their normal size; but, of course, their contracted mouths must have limited very much the supply of blood to the substance of the heart. This, no doubt, was the cause of the angina,—a cause which Dr Haldane did not remember to have seen previously in operation in such a marked manner. Contraction of the coronary arteries was a frequent cause of angina, but this was usually due to their calcareous degeneration; in this case, however, the contraction was due, not to disease of the vessels themselves, but was secondary to the changes in the aorta. The opacity of the aortic valves was no doubt due to a process similar to that which had affected the arteries, and might be referred to a very chronic endocarditis. During the life of the patient Dr Haldane had believed that there was fatty degeneration of the heart, and iron had been prescribed with a view to promoting the general nutrition. It appeared, however, on microscopic examination, that the degeneration was in its very earliest stage, so that the symptoms of angina could not be referred to this cause.

VI. POISONOUS CHEESE.

Dr Cairns showed a specimen of poisonous American cheese which had produced alarming symptoms in at least twelve cases. The symptoms came on about three hours after the cheese had been eaten, and consisted in severe pain in the stomach, in some cases cramp, violent vomiting of a greenish fluid, soreness of the throat, and a cold clammy condition of the skin. All the patients recovered, and in every case recovery was preceded by profuse perspiration. The cheese appeared quite fresh, it had no peculiar odour, or anything to indicate its poisonous properties.

VII. INSTRUMENT FOR THE PERFORMANCE OF TRANSFUSION.

Dr Handyside exhibited an instrument calculated to facilitate the performance of transfusion. He had performed the operation three times, and happily in two cases successfully. In the two former of these cases, while using the ordinary apparatus, he had experienced some difficulty in preventing the blood to be injected from coagulating; the greatest celerity being required in taking the blood into the syringe and transfusing it into the vein. No doubt, when there were plenty of assistants, the difficulty could be got over, but where there were not enough it was by no means easy to introduce the tube and retain it within the vein. He had found on two occasions that the edge of the vein was apt to become inverted. Accordingly, last time he had used a lancet in the form of a cutting director, somewhat rounded at the point and grooved. With the left hand this instrument was introduced for about a quarter of an inch into the vein, and with the right the nozzle of the syringe was passed along the groove into the vein; the instrument was then withdrawn. He had found that this facilitated the operation very materially. He had employed it last Sunday in the case of a patient of Dr Alexander Wood and Professor Simpson, where bleeding had been going on from two till six o'clock. Transfusion was performed at eight o'clock; the instrument and nozzle were introduced very readily, and sufficient blood was injected to restore the action of the heart. For some hours previously the pulse had been imperceptible, and the heart's sounds inaudible. Dr Handyside believed, that while ordinary means were sufficient when there was plenty of assistance, the instrument shown would be found useful when the operator had to depend exclusively upon himself.

VIII. ON SOME OF THE LAWS OF THE PRODUCTION OF TWINS.

Dr Matthews Duncan read a paper on this subject, which will be found at page 767 of this number of the Journal.

IX. ON THE CHARACTERS OF THE EXPECTORATION IN TWO CASES OF FETID BRONCHITIS AND GANGRENE OF THE LUNG.

Dr Arthur Gamgee read a paper on this subject, which will be found at page 807 of the present number of this Journal.

Dr Haldane had listened with much pleasure to this paper, which he looked upon as very valuable. Dr Gamgee had established the fact that butyric acid was present in the sputa in many cases, and that it was not the cause of the fetid odour of the expectoration. Dr Haldane agreed with Dr Gamgee, that the gangrene of the lung in the second of the cases detailed had probably been occasioned by the obstruction by a clot of a branch of the pulmonary artery.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXIV.—MEETING III.

11*th January* 1865.—Dr Graham Weir in the Chair.

I. CASE OF HYDATIDS.

The *Secretary*, in the absence of *Dr Bryce* of Dalkeith, showed a very minute specimen of hydatids. When fresh it was much the shape and size of an ordinary gooseberry, and when opened into and examined microscopically it presented the perfect grape-like forms of the hydatid growth. This was the smallest specimen of that nature which had been shown before this Society.

Dr Graham Weir remarked that this specimen was extremely interesting on account of its minute size. It also illustrated how early an ovum may be lost by this degeneration. He had no doubt such specimens of hydatids occurred oftener, but were lost in the clots and discharged.

II. NOTES ON SMALLPOX AND INOCULATION IN OLD CALABAR.

Dr Hewan read the following paper on this subject:—Native name *Isotti*, or *Mfat Itu*. A very uncommon disease, entirely epidemic. It had not occurred for upwards of eighteen years before the present attack in 1864. Its approach causes terrible panic and alarm. On the appearance of the first symptoms, the individual is hurried out of the town, and lodged in some remote and isolated place, generally in the midst of an extensive bush. If it be a free man, or one having slaves, he gets all the attention and care that, with the great dread of contagion, they are able to bestow. If a slave, his condition is altogether different, and hence he is sometimes utterly neglected, and left to die or live as the case may be. In case of death the body is no buried, for fear, they say, of infecting the ground, and so perpetuating the disease, but it is placed on a platform raised a foot or so above ground, and there left to its fate.

They have *no means of cure.* The disease is left to run its course. It is alleged, however, by an old chief of influence, who remembers, when a young man, to have seen the epidemic of smallpox, that there was a man in the country at that time who cured persons afflicted with the disease by a plant which grows in the country. The man would never tell his secret to any one, and it died with him.

In order, however, to modify the virulence of its attack, whenever the disease has fairly made its appearance in the town or district, inoculation is had recourse to, and is pretty extensively practised. The seats selected for inoculation are various: at one time on the foot, about half-an-inch in a line above the bifurcation of the first and second toes, on the back of the wrist, or between the eyes. Eight days after the operation the disease begins to affect the system. First the throat becomes affected with a feeling of constriction, and dysphagia, accompanied by pains in the head, neck, loins, and joints, and general febrile symptoms. Four days after these premonitory symptoms the pustules begin to appear, beginning on the breast, then on the face. One case which I saw had many on the breast, and only one on the face. The severity of the fever lasts for eight or nine days, but the patient continues more or less under the disease for a month. The most of the cases that I was privileged to see had recovered from the disease, and had characteristic marks on their faces and breasts, but, with one or two exceptions, *no pitting.* To ensure this they cover the parts during convalescence with burnt clay mixed up into a paste with water. The marks which the pustules leave are for the most part blacker (and this upon the negro) than the ordinary healthy skin.

The first case that attracted my attention was that of a woman with round black spots scattered over her face and breast; darker than the general integument. On asking her the cause, she said it was the effects of smallpox that she had had after inoculation. There was no pitting to be seen. Another case presented itself, namely, that of a young man with several marks on his face. His whole skin was very black. I saw no pits, and, in order to test the accuracy of his statement, and also to gather from him more information than otherwise he was disposed to give, for it is such a horrid disease, they don't like to speak of it, I expressed to him my doubts of his having had true smallpox, as there was no pitting, which is such a common result. He then looked up and showed me the free edge of the right *ala* of the nose where there had been a pustule, and which, owing to the difficulty of the paste sticking on there, had left a pit.

Inoculation is performed by mixing the matter with oil—palm oil—before it is inserted.

Inoculation is pretty extensively practised. Some deaths do occur, but this is comparatively rare. More deaths take place when it occurs idiopathically; and here, in cases of recovery, the marks resulting from the pustules are more numerous.

As a charm against the disease scarlet cords with white feathers stuck in are hung around the neck, and the several roads to the towns are guarded by festoons of fetishes having scarlet fruits — an amomum — interspersed through them. This reminds me that it is alleged of one John of Gaddesden that, for the cure of his patients, he placed them in a room whose walls and curtains were made red for the purpose.

There are other methods employed to ward off the disease, which are purely superstitious, and have no bearing whatever on its treatment. For example, the quiet observed during the day, and then the sudden shout and noise at dusk, which is kept up pretty much all night by those appointed to watch the town, believing the disease to be a monster, or, to use the words of scripture, a "pestilence that walketh in darkness." And next, the killing of the first one attacked, by way of a sacrifice, I presume, to the presiding genius of the town, who permitted the disease to begin its ravages. This custom, however, occurs in one family only.

III. ABORTION-POWDERS.

Dr Hewan also exhibited a specimen of the Amomum used by the natives in Old Calabar to produce abortion. It was powdered and introduced into the vagina, and being of the pepper tribe it no doubt acted as an irritant.

SESSION XXIV.—MEETING IV.

25th January 1865.—Dr PATTISON, *Vice-President*, in the Chair.

I. FIBROID TUMOURS AND POLYPUS OF THE UTERUS.

The *Secretary* showed a specimen of the above, which illustrated all the varieties of fibroid tumours. There was a polypoid tumour within the uterus, an intramural, and several subperitoneal tumours, some of which had become pediculated. The following are the notes of the case supplied by Dr Tuke of the Royal Asylum, Morningside, to whose kindness the Society was indebted for the preparation.

"C. E., the patient from whom this preparation was obtained, was admitted twenty years ago into the Royal Edinburgh Asylum. She was then in a state of the most violent and dangerous mania; so much so as to be an object of dread to the attendants. The mania under which she laboured was of a peculiarly animal type; she uttered sounds more like the howls of a wild beast than the voice of a human being. When put to bed she rapidly destroyed all the clothes, and arranged them in a sort of nest, in which she sat naked and gibbered, presenting one of the most humiliating spectacles possible to conceive. These attacks recurred about every two years, and lasted for from six to twelve months. During the interval she was profoundly demented. *In* one of these attacks, and *from* its effects, she died, worn out and exhausted. When excited she was in the habit of gnawing the woodwork of her room, on one occasion eating through two shutters, both of which were two inches thick. The room in which she died is now in the state in which she left it, and the amount of destruction she contrived to effect is extraordinary, considering the weak state of her system.

"In June 1852, an entry appears in her case (she was then fifty-two years of age), in which it is remarked that the catamenia were very profuse, and that she had attacks of vomiting. In October of the same year appears the following entry:—'The profuse menstruation continued, and the attacks of vomiting. The abdomen attained a large size, and felt hard and knotty to the hand. She was treated with enemata and purgatives, which were followed by evacuations of hardened feculent matter. The swelling subsided in a great measure, and left only a hard tumour of the size of the fist in the situation of the uterus, apparently of a fibroid nature.'

"The tumour increased slowly but gradually, until two or more distinct portions could be detected—the menstruation ceased at the age of fifty-four. Since she came under my observation (two years ago), she never experienced any uneasiness from the tumour; in fact, I was not aware of its existence until I examined her, amongst other patients, when making a careful investigation into the bodily health of all the old inmates, with the view of discovering diseases which are liable to lie latent in the insane to an extent hardly to be credited by any but those accustomed to their treatment."

Dr Alex. R. Simpson remarked that the case was interesting, as showing the different forms and relations which fibroid tumours were wont to assume in connexion with the uterus. There were a great number of subperitoneal tumours, which here, as in so many other cases, were the largest in the group, and presented patches of commencing calcareous degeneration. The intramural nodules were here mostly small, and not in any instance surrounded with very large vessels. But the most important of the group was the large submucous tumour, of the size of a lemon, which had come to project in a polypoid form into the uterine cavity. It was probably when this polypus first began to protrude towards the interior of the uterus that the patient had begun to suffer from menorrhagia thirteen years ago. For there were two stages at which patients were specially liable to hæmorrhage in connexion with fibroid polypi of the uterus; viz., 1*st*, when the uterine mucous membrane was first pushed out by it, when it was usually thickened and vascular; and, 2*d*, after the body had become fairly polypoid, and when, from the effort of the uterus to expel it through the os, or from any other cause, the mucous membrane on its surface got abraded and ulcerated. But the beautiful preparation which Dr Tuke had sent them illustrated further the extent such tumours may attain without causing any inconvenience. It was explained in this case by the circumstance that none of the tumours which had attained a large size had been retained in the pelvis. It was where tumours got wedged within that cavity that they caused any inconvenience from pressure on the neighbouring parts, and it was often sufficient to push up the tumour above the brim to afford relief.

Dr Moir stated that the remark of Dr Simpson was often borne out in practice. Sometimes, however, very small tumours gave rise to great uneasiness. He had one case where there were great suffering and hæmorrhage every catamenial period, and a small fibroid tumour was all that could be detected. He would ask the members of the Society what was their experience regarding the bromide of potassium in fibroid tumours? He had tried it, but must confess he had not the same reliance on its power of causing absorption of these bodies that many other physicians had.

Dr James Young stated that he had attended a patient with a fibroid tumour, who had taken the bromide for some time, and he thought the tumour was now less.

Dr Cairns had seen a case where a tumour, the size of an apple, had entirely disappeared under that remedy.

Dr Keiller remarked that he had very frequently prescribed the bromide of potassium in these cases. One effect that he had noticed was that, if the remedy was long and largely given, the patient generally got thinner, and the tumour thus might appear less than it formerly was. The alterative effect of the remedy was often useful. He had a case recently where a patient suffered great pain and inconvenience from a tumour, but experienced considerable relief from the use of the bromide. It was surprising to what a size these growths may attain before the patient is aware of their presence. One lady he attended died without being made aware that she had a tumour of a very large size; it being subperitoneal, the discharge was comparatively slight. He thought that the nearer the tumour lay to the mucous membrane the more the liability to hæmorrhage.

Dr Charles Bell remarked that these tumours often underwent a change when the patient ceased to menstruate. He had a patient who suffered much from hæmorrhage, and attacks of peritonitis; but since the change of life these had ceased, and the patient now enjoyed good health, and was not inconvenienced from the presence of the tumour.

Dr Moir had a case where there was a tumour the size of the fist in the interior of the uterus. It had a distinct neck, but it could not be induced to pass through the os. He had frequently dilated with tents, but they threatened to set up inflammatory symptoms, and had to be discontinued. The patient suffered much from attacks of hæmorrhage.

Dr Alex. R. Simpson said that Dr Baker Brown, in such a case, incised the os, and found that it generally stopped the hæmorrhage. With regard to the use of bromide of potassium, he must say that he could entertain no doubt as to the remedial powers of that drug in many cases of uterine fibroids. He had just a few days previously, for example, seen a case where only a small body like a pea was to be felt per vaginam on the back wall of the uterus, which would probably have escaped his observation had it not been that the patient had been under treatment for some time for a tumour, which, when she first came to consult him had been considerably larger than a walnut, and gave rise to some distress from its pressure on the rectum. It was a distinct case of a fibroid tumour melting away under the use of bromide of potassium; and he had seen several such. He had remarked before that the inconvenience caused by tumours depended greatly on their position. But tumours, as had been remarked, which generally gave no trouble, are sometimes apt to create much disturbance at each catamenial period. He had had one patient for some time under his care, who very frequently, during her menstrual periods, required the use of the catheter, from the swelling that on these occasions was apt to arise in connexion with a large fibroid tumour which was immovably fixed in the pelvis. He would ask Dr Keiller, what were his present views upon the diagnostic value of his vaginal stethoscope in these cases?

Dr Keiller, in reply, stated that at one time he had thought that by means of the stethoscope he was able to diagnose at an early period the existence of pregnancy, by hearing the uterine souffle through the vagina, and that the bruit of a fibroid tumour could not thus be detected. But he had lately modified his views on the subject; for in several cases he had heard the bruit of a fibroid through the vagina. He thought it greatly depended on the position of the tumour. Still he believed that the stethoscope thus applied was of great service in detecting the existence of pregnancy in many cases, at a period before the mammary signs were developed, or where these are equivocal. There was another diagnostic aid in such cases, which removed in a measure the difficulty,—in pregnancy you had no menses, in fibroid tumours there was frequently menorrhagia. He had been surprised to see that Dr Routh of London had lately proposed the use of the stethoscope in this manner as something new; but, as the members were very well aware, he (Dr K.) had brought it forward a number of years ago, when he communicated several illustrative cases, and exhibited various forms of stethoscope which he had used *for the special purpose of diagnosing early pregnancy per vaginam.* The minutes and reports of the Society would clear away any possible doubt on this subject, and amply enough prove that *the direct application of the stethoscope to the uterus*, in cases of disease, as well of suspected pregnancy, was long ago here recommended.

Dr Alex. R. Simpson was glad Dr Keiller had made these statements, for he thought Dr Keiller had often done himself great injustice from not publishing his inventions and observations. In Germany, seven years ago, Professor Hecker, now of Munich (if he rightly remembered), had remarked at one of the friendly discussions which in that genial country was wont to follow the more formal meeting of the Obstetrical Society of Berlin, as well as of other learned bodies, to one of the gentlemen present, that if they could apply a stethoscope immediately to the uterus, they might be able to diagnose pregnancy at a very early period. They were surprised and delighted to be told that in Scotland Dr Keiller had already invented a stethoscope for this purpose, and had used it for some time with most satisfactory results. Another point which he would like to hear the opinion of the Society on was the diagnosis between ovarian and fibroid tumours. Dr M'Clintock of Dublin speaks, in his book on Diseases of Women, of a bruit being common to pregnancy, fibroids, and ovarian tumours. Now, his (Dr A. R. S.'s) experience led him to believe that the presence of a bruit in an ovarian tumour was such a rare phenomenon as to constitute an almost patholognomic sign of distinction between it and the uterine fibroid. In the examination of a case of supposed fibroid tumours of the uterus, he had been in the habit of believing that we had not arrived at all the obtainable facts of the case, until by means of the stethoscope the presence or absence of the bruit had been determined, and in cases of fibroid of any considerable size the bruit was almost invariably audible. When in England, a few days before, seeing a lady labouring under puerperal fever, the nurse in attendance had asked him to examine a uterine tumour under which she herself had long laboured, and for which she had consulted a number of medical men in Birmingham and London. Some of them had correctly enough diagnosed the case to be one of uterine fibroids; but what astonished him, in the history of the case, was, that none of them had ever auscultated the tumour, although there was a very distinct and unmistakable bruit present. He would ask whether any of the members ever heard a bruit in a case of an ovarian tumour?

Dr Moir never heard a bruit in such a case.

Dr Keiller concurred in Dr Moir's opinion. Ovarian tumours have no sounds like fibroid uteri or pregnancy. There was one case, however, in which he (Dr K.) had performed ovariotomy, where a sound was heard low down in the left side. Dr Trail of Arbroath and others had examined the case, and at first there was some doubt as to whether it was an ovarian case. In removing the tumour, which was of large size, they found extensive adhesions, with a large vascular surface connecting the tumour with the abdominal wall, over the spot where the sound was heard. This was the only case where he had heard any sound with an ovarian tumour, the bruit here being external to, although connected with the ovary.

CORRESPONDENCE.

SYPHILIZATION.

THE following Correspondence has been published in the *British Medical Journal* in reference to a paper on Syphilization, by Dr David Simpson, which appeared in the December number of this Journal.

LETTER FROM HENRY LEE, ESQ.

SIR,—In your Journal of this date, under the head of Progress of Medical Science, you furnish an abstract of some cases of supposed very severe constitutional syphilis, which yielded at once to syphilization.

One of these patients was advised, in 1861, to go to Christiania, and to place himself under the care of Professor Boeck. Six years had elapsed since he contracted a chancre. He was suffering from an ulcer over each tibia and one over the left clavicle. Perforation of the palate had taken place, and some pieces of bone had come away from the interior of the nose; the frontal bone was swelled; and he was very much reduced in strength. He underwent a course of syphilization, and was "completely cured" in three months.

Now, sir, curiously enough, I have had under my care during the last eighteen months a gentleman who has suffered from disease of the bones of the legs, from enlargement of the left clavicle, and from whose nose some pieces of bone had come away. The frontal and parietal bones in this gentleman were enlarged; he had been sent from Edinburgh to Professor Boeck, and had undergone some 160 inoculations.

It is most unlikely that two cases could agree in so many particulars; and I, therefore, conclude that my patient is the same as the one you mention as having been completely cured.

I took great pains to investigate my patient's case; and, to his surprise, gave him my decided opinion that he had never suffered from syphilis at all. One day, after leaving my house, he met a relative somewhat older than himself; and he mentioned to him, that it appeared a very curious thing that, after going over to Norway to be syphilized, he should now be told that he had not had syphilis at all. His relative replied by asking him if he knew what his father had died of. He did not; and was then informed for the first time that his father had died of "softening of the bones." My patient returned at once to furnish me with this information; which afforded to him, as it did to me, a satisfactory explanation of his symptoms.

The absence of any eruption in this case, and the interval of some years, between the appearance of the supposed primary and secondary affections would of themselves be sufficient to lead to the inference, that the patient's symptoms were not syphilitic; and I can only conclude that, although his journey to Norway had undoubtedly done him much good for a time, he had very unnecessarily undergone some 160 inoculations. One more inference may, I think, be deduced from this case; namely, that the matter employed in these inoculations was pus from suppurating or soft sores, and not the secretion from indurated or infecting sores.

Had the latter been employed, my patient would probably have returned from Norway with a disease which he had not when he left England. I am, etc.,

Savile Row, 28th January 1865. HENRY LEE.

LETTER FROM PROFESSOR J. Y. SIMPSON, M.D.

SIR,—My son, whose report of two cases of tertiary syphilis cured by syphilization was quoted by you in the Journal for 28th January (p. 89), is abroad. In his stead, therefore, allow me a word or two in answer to Mr Henry Lee's criticism on one of these cases, contained in the last number of your Journal (4th February, p. 128).

1. Mr Lee (who never saw the gentleman I allude to, before his cure by syphilization) states it as his "*decided* opinion that he had never suffered from syphilis at all." All the medical men who saw him when he was ill, entertained exactly the opposite opinion; and surely they had infinitely better means of forming a correct judgment. Dr Rynd of Dublin, who attended the patient during the existence of the primary disease, declared the sore to be syphilitic; and Dr Guthrie, Professor Miller, and Professor Boeck, who had him successively under their care after the tertiary symptoms broke out, never, I believe, had a single doubt as to the syphilitic character of his malady. I never myself saw a more distinct and marked case of tertiary syphilis.

2. Mr Lee argues that the absence of any recollected eruption in this case, and the lapse of some years between the primary and the tertiary affections, "lead to the inference that the patient's symptoms were not syphilitic." This argument seems to me inconceivably strange, coming from a gentleman in Mr Lee's high position. Mr Lee himself states, in his work on *Syphilis* (2d edition, p. 268), "that there is no well-marked natural division between the secondary and tertiary symptoms," and, "in fact, the so-called tertiary symptoms sometimes appear before the secondary." Mr Acton (*Practical Treatise*, 2d edition, p. 558) lays it down that occasionally no secondary symptoms whatever appear after the primary sore; but, after "a considerable lapse of time," in consequence of some exciting cause, as cold, etc., "tertiary symptoms all at once declare themselves." In his elaborate and masterly treatise on the *Pathology and Treatment of Venereal Diseases*, Dr Bumstead of New York observes: "We meet with some instances in which syphilis appears to skip over its secondary, and manifest itself *only* in its primary and tertiary forms." (P. 652.) One of the greatest living authorities in surgery, Professor Gross of Philadelphia, remarks in regard to the phenomena of tertiary syphilis, that the average period of their evolution ranges "from six to eighteen months, although in very many instances they do not occur until a number of years after the appearance of chancre, or chancre and bubo. Thus I have repeatedly seen tertiary symptoms manifest themselves for the first time from twelve to eighteen years after the primary disease, the poison having lain all this time like a hidden spark in the economy." (*System of Surgery*, vol. i. p. 486.)

3. Evidently Mr Lee is anxious to reject this case as a case of tertiary syphilis, merely—it appears to me—because it was completely cured by syphilization. And, indeed, the recovery was one of the most wonderful that I ever witnessed. The patient left this country for Christiania in the most wrecked and wretched state of health, so helpless and hopeless that most of his friends were in despair of the possibility of his recovery. He came back

in three months well and feeling well; and one of his first visits was to the racket court. But Mr Lee maintains that the disease of which this patient was cured was not syphilis, but "Softening of the Bones." That syphilization should cure syphilis is, according to some minds, a great paradox—though not in reality a greater than the cure of it by mercury, iodine, or sarsaparilla. Professor Boeck has now used syphilization successfully in hundreds of cases, and he looks upon the cure of syphilis by syphilization as one of the most certain and "mathematical" facts in the range of practical medicine. I have the pleasure of knowing Dr Boeck personally, and I know how entirely and implicitly his facts and observations and statements can be trusted to. But I opine that, if Mr Lee's allegations be true, Mr Lee has made—perhaps unwittingly—a greater discovery in surgical therapeutics than the cure of syphilis by syphilization; for, according to his letter, he believes that inoculations from suppurating or soft sores cured in this case what was formerly, I believe, deemed incurable—namely, "Softening of the Bones." If the cure of syphilis by syphilization is a marvel, should not the cure of "Softening of the Bones" by syphilization be regarded in surgical therapeutics as a still more marvellous marvel?

4. The "Softening of the Bones" which, according to Mr Lee's pathological views, was the malady under which the patient was brought to the gates of death, was marked, as he himself states, by disease of the bones of the legs, enlargement of the left clavicle (there being carious ulcers in each of these three parts); pieces of bone were discharged from the interior of the nose, and the frontal and parietal bones were enlarged. What species of "Softening of the Bones" is, may I ask, indicated by this combination? Is there any new osseous disease, or "Softening of the Bones," of which these are the symptoms? Is there any old disease of the bones, *except* syphilitic disease of them, which ever leads to such a combination of them as the above? I am, etc.,

Edinburgh, 6th February 1865. J. Y. SIMPSON.

P.S.—I shall forward a copy of Mr Lee's letter to Professor Boeck, who, I have no doubt, will answer it better than I can. Allow me to add, that I happened to see in the country lately the gentleman whose case is the other instance of syphilization reported by my son. This gentleman is in the enjoyment of the very best of health, and spoke, to my astonishment, quite distinctly. He showed me that the secret consisted in his having a flexible caoutchouc palate to replace the havoc made in his mouth, etc., by the syphilitic ulceration. He assured me that, since leaving Christiania, he has not suffered in any degree from the old syphilitic enemy that had formerly nearly destroyed him before he was cured by syphilization. J. Y. S.

LETTER FROM HENRY LEE, ESQ.

SIR,—Professor Simpson has, in a very friendly manner, furnished me with the means of identifying the case he mentions in your impression of last week as the same as the one to which I had previously referred.

It is now admitted that that patient had no constitutional symptoms for six years after the appearance of the supposed primary affection; and that he had at no time any eruption upon the skin. Professor Simpson has attempted to show that the case might nevertheless be one of syphilis, by quoting passages from Dr Bumstead and Professor Gross, to the effect that sometimes "tertiary symptoms manifest themselves for the first time from twelve to eighteen years after the primary disease;" and, in the clever way in which Professor Simpson has put it, an ordinary reader might be led to suppose that the authors mentioned believed that constitutional symptoms, in real cases of syphilis, might not appear until after that lapse of time. A fallacy underlies Professor Simpson's whole argument.

It is true that Dr Bumstead says we meet with some "instances in which syphilis appears to skip over its secondary, and manifest itself only in its primary and tertiary forms." But he also states that "the general symptoms of syphilis, in the absence of specific treatment, *always* appear within six months, and generally within three months, after infection."

Dr Gross says very much the same thing; and Ricord has printed his opinion upon the subject in very large letters; namely, that in the natural development of the disease, "IL NE PASSE JAMAIS SIX MOIS SANS QU'IL SURVIENNE DES MANIFESTATIONS DE L'INTOXICATION SYPHILITIQUE." (*Lettres sur la Syphilis*, p. 300.) I must add, that the opinion here expressed agrees with my own experience.

It is then evident that, when authors mention the tertiary symptoms as appearing after the lapse of some years, the "tertiary" are put in opposition to the "secondary" symptoms, which have previously appeared at their natural time. Such a case I well recollect, where an eruption appeared after the lapse of twenty years; but in this, as in other similar cases, the secondary symptoms had first manifested themselves at their usual time.

There are other reasons, such as the absence of any history of an indurated chancre and of any chronic indolent enlargement of the glands, which induce me to believe that the disease under which the patient in question was suffering was not syphilitic. I was of that opinion when the patient was first sent to me for a written opinion on his case; I was of the same opinion when I read the case reported in the *British Medical Journal;* and I am of the same opinion now that I have read Professor Simpson's comments upon it.

If, on the contrary, Professor Simpson has indeed made out that consitutional symptoms may appear for the first time after a lapse of six years, it is he who has made a great "discovery." For myself, I must disclaim having maintained the "marvellous marvel," that softening of the bones could be cured by syphilization, especially in the instance before us; seeing that it was the father who had the "softening of the bones," and the son who was (as it is termed) syphilized.

In the third paragraph of his letter, Professor Simpson implies that I am anxious to reject the case in question only because it was completely cured (?) by syphilization. Whatever opinion I may now entertain with regard to the virtues of syphilization, I certainly had no prejudice against it originally, and have published a plate in my work on *Syphilis*, illustrating the appearances of the inoculations in a case in which I had tried it.

My friend Dr Marston informs me that he has also lately carried it to the extent of some forty to sixty inoculations, but with no benefit to the disease for which it was tried, and with a certain amount of damage to the patient's constitution.

When these experiments are tried on those who are already syphilitic, and who are provided for in military hospitals, no great harm can probably follow. But, holding the views I do, your readers will not be surprised if I should exert what little influence I may have to prevent the young men of our country from leaving their homes to be syphilized with matter taken indiscriminately from hard and soft sores, where there is any chance of their having a disease communicated to them which they had not before.—I am, etc.,

9 Savile Row, 12th February 1865. HENRY LEE.

UNIVERSITY OF EDINBURGH.

AT a meeting of the University Court, held on Wednesday the 8th February, Dr William Seller, F.R.S., F.R.C.P., was appointed one of the additional Examiners in Medicine in place of Dr Begbie resigned.

PUBLICATIONS RECEIVED.

Benoist,—Essai sur la Médication Isolante. Par le Docteur Ferdinand Benoist. Poitiers, 1864.

Blumenbach and Hunter,—Anthropological Treatises of. London, 1865.

Bonnafont,—Des Maladies de l'Oreille. Par le Docteur J. P. Bonnafont. Paris, 1860.

Clarke,—Manual of the Practice of Surgery. By W. Fairlie Clarke, F.R.C.S., etc. London, 1865.

Dufour, etc.,—Le Congrès de Genève. By MM. Dufour, Moynier, et Lehmann. Geneva, 1864.

Hamilton,—Military Surgery and Hygiene. By F. H. Hamilton, M.D., etc. New York, 1865.

Médicine et de Chirurgie pratiques, Nouveau Dictionnaire de. Vol. II. Paris, 1865.

Ringer,—On the Temperature of the Body as a Means of Diagnosis in Phthisis and Tuberculosis. By Sydney Ringer, M.D., etc. London, 1865.

Speech, The Irrationale of. By a Minute Philosopher. London, 1864.

Squire,—Photographs (coloured from life) of the Diseases of the Skin. By A. B. Squire, M.B. London, 1865.

Topinard,—De l'Alaxie Locomotrice. Par le Docteur Paul Topinard. Paris, 1864.

Wilson,—On Food, as a means of Prevention of Disease. By Erasmus Wilson, F.R.S. London, 1865.

PERIODICALS RECEIVED.

Berliner klinische Wochenschrift,—No. 53, 1864; Nos. 6, 7, 8, 1865.

Births, Deaths, and Marriages, Monthly Return of, for January 1865; and Quarterly Return, Dec. 1864; with Supplements.

British Medical Journal,—Feb. 4 to 25, 1865.

Bulletin Générale de Thérapeutique,—Dec. 15. Paris, 1864.

Canstatt's Jahresbericht, Vol. 5 for 1863. Wurzburg, 1864.

Dublin Medical Press,—Feb. 1 to 22, 1865.

Gazette des Hôpitaux,—Nos. 11 to 22, 1865.

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Gazette Médicale de Paris,—Nos. 5, 6, 7.

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Journal für Kinderkrankheiten,—Nos. 11 and 12. Erlangen, 1864.

Medical Times and Gazette.—Feb. 4 to 25, 1865.

Revue de Thérapeutique Medico-Chirurgicale,—Feb. 1, 15. Paris, 1865.

Virchow's Archiv,—Vol. 31, Nos. 3 and 4. Berlin, 1864.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Civil Incapacity.* By DOUGLAS MACLAGAN, M.D., Professor of Medical Jurisprudence in the University of Edinburgh.

(*Read before the Royal College of Surgeons, 24th February* 1865.)

THE principle which lies at the foundation of all the legal proceedings in reference to cases of civil incapacity is this,—that the Sovereign, as the political parent and guardian of the kingdom, has the protection of all his or her subjects, and of their lands and goods, and is bound in a more particular manner to take charge of all those who, by reason of imbecility or want of understanding, are incapable of taking charge of themselves. Whether this be a royal prerogative, or a royal duty, or a royal trust committed to the Crown by Act of Parliament, for the benefit of the subject, we need not stop to inquire. What we have to inquire into here is this,—Who are the persons thus pointed at as being more specially the objects of royal guardianship? What is the machinery by which the sovereign brings these weak ones under the royal cognizance and care? Does this machinery work well, and smoothly; or if not, in what way may repairs be executed, so as to combine, as far as possible, power with safety, and easy action with certainty of result? In plain terms,—What constitutes that mental imperfection which renders a person incapable of taking charge of himself and his affairs? What are the legal proceedings in reference to these cases of civil incapacity? May these legal proceedings in any way be improved?

We have here a subject for which I need offer no apology, because (I trust not personally, but indirectly) it interests us all as citizens; whilst to a meeting consisting partly of doctors and partly of lawyers, it has a professional interest,—the medical element preponderating in the first of these inquiries, the legal in the second and third. I beg at the outset to say, that I do not expect to bring before you anything that is new. The subject has, in some of its bearings, been discussed in this place, and elsewhere, both by jurists and by physicians abler than myself. It is its importance, not its novelty, which justifies me in now bringing it before you. Confessedly it involves some questions of delicacy and difficulty; and I therefore hope that I may be permitted, standing before such a tribunal as the present, to put in delicacy and difficulty as pleas in mitigation of criticism. What I say here may readily be deemed and proved to be wrong; but if, by bringing up the question, I lead wiser heads than my own to consider it for practical purposes, I shall gladly undergo the operation of refutation, if the result shall be to elicit some authoritative exposition of what is more correct.

Let us proceed, then, to consider the more medical part of the question,—What constitutes that mental imperfection which renders a person incapable of exercising his right as a citizen, to take charge of himself and his affairs?

The oldest technical expression which we find used for designating such individuals is the well-known one *non compos mentis*, which occurs in an Act of Parliament passed so far back as the reign of Edward II. But though this phrase is found in Terence and Cicero, and has, one way or another, in Acts of Parliament, legal treatises, and medical books, been in use in this country for five centuries, it is not yet quite agreed upon, by lawyers and doctors, what is meant by *non compos mentis*, and that which is held to be its English equivalent, "*of unsound mind.*"

Legal writers inform us that Lord Coke made four classes of persons *non compotes mentis.*—"1*st*, Idiot, or fool natural, who from his nativity, by a perpetual infirmity, is *non compos mentis.* 2*d*, A person who was of good and sound memory, and by sickness, grief, or other accident, wholly loses his memory and understanding. 3*d*, A lunatic, *lunaticus*, who sometimes has his understanding and sometimes not, *qui gaudet lucidis intervallis*, and therefore he is called *non compos mentis*, so long as he hath not understanding. And, 4*th*, A person who, by his own vicious act, for a time deprives himself of his memory and understanding, as he that is drunken; but such a person has no privilege by this voluntarily contracted madness."

Let us see how far lawyers and medical men are agreed upon the characters of these classes, and how they view them in relation to the phrase *non compos mentis.*

I presume that, taking the word in its ordinary acceptation, there are no two persons in this assembly who have any doubt, that an individual belonging to the first of these classes, viz., one in a state of idiocy, is incapable of managing his own affairs, and is *non compos mentis.* But what constitutes idiocy? As medical men, we understand by it a congenital deficiency, which, in its more exquisite forms, is denoted by characters so obvious as to render the diagnosis of it matter of no difficulty. We can hardly pass one in the street, though we see him for the first time, without recognising the idiot. The small head, the vacant countenance, the squinting or ill-balanced eyes, the thick lips, the gaping mouth with the saliva running from it, the unsteady gait, connected, it may be, with unequally developed limbs, the ill-controlled voluntary muscles interfering with the power of directing the body to any fixed object,—all, or most of these combined, readily point out, even to the unprofessional eye, the helpless, hopeless, slavering idiot. If we attempt to hold any intercourse with such a being, the first thing we see is the imperfection of speech, or, it may be, the complete absence of this faculty,—all that is spoken being, at the most, a few words, uttered without a trace of any corresponding expression of countenance; or perhaps not even thus much, but only some cries or muttered sounds. If we inquire into his habits, we learn that he evinces no idea of even the simplest proprieties. If we attempt to get a view of his intellect, we fail to perceive it at all. We speak to him, but there is no response, no emotion manifested in the countenance, none of that instinctive shrinking from us if we are a stranger, nor that recognition of us by sign or gesture if we have previously been in contact with him, which a dog or a cat will manifest. It is zoologically a human being, but in reality a creature below the brutes. I am, however, sure that all of you will say, that though, undoubtedly, such idiots as this exist, the portrait is drawn in lines far too strong to represent the majority of idiots whom we meet with in the course of our lives. It is seldom that things are quite so bad as this. Though generally the countenance betrays the degradation of the mind, there is fair controlling power of locomotion, and a certain amount of faculty of speech, though it may not extend beyond the power of pronouncing single words or short very simple sentences; there is enough of intellect to enable him to distinguish a kind from an angry word or gesture in others, and to act in obedience to it; and there is enough of memory to enable him to bear in mind that he is to avoid the grosser indelicacies in attending to the wants of nature, provided he has been drilled into this by those who have had charge of him. Perhaps there is enough of memory to enable him to repeat a few short sentences, and to attach, or act as if he attached, some meaning to them; but beyond this he does not go, and we hesitate not to pronounce him a hopeless idiot still. Here, however, we get a glimpse, in its most rudimentary form, of that which, according to its absence or presence, has been by many authorities taken as the best test of the existence or non-existence of idiocy—the capability of receiving instruction; of which I shall say no more than that, in reference to our subject, it is perhaps the most important test which we can apply to determine in what category, as regards mental weakness, we are to place any given individual.

But it is not my object to describe idiocy. We have looked at it only in its more complete forms, and these are practically the least interesting, because they present little or no difficulty. Gradually, as we follow out our survey of those who, in general terms, may be called idiotic, we see less and less distinctly the more debased and repulsive features, and we see more and more of the intellectual manifesting itself, until, from the absolute idiot, with the utter destitution of anything like reason, at whom we have been pointing, we rise first to the "imbecile," in whom a certain capacity for instruction is recognised, and for whom the humane spirit of the age is now providing more systematic means for his upbringing and education, if we may use such a word in reference to him, and then through every grade of weakness, up to him who barely falls, if he falls at all, within the circle of our remarks, who goes about in the world, and may be met with in society, where he is spoken of, more or less feelingly or unfeelingly, as a "weak brother."

Now, if this be the case, it is obvious that failure can be the only result of any attempt by us, as medical psychologists, to lay down fixed characters by which we may classify persons of weak mind, so as to be able to hand them over, as it were, duly labelled, to be disposed of by the tribunals of the country according to rules applicable to each variety. We can hardly be stupid enough to mistake the absolute idiot, and we are generally ready enough to point out the weak brother; but between these we can hardly draw distinctions, except as to degree. It is quite true that to a certain extent they differ in their special manifestations of character. Whilst in all of them the reasoning power is more or less defective, there may be very various conditions of the moral sentiments which may be considerably, though unequally developed.

"Among the moral sentiments," says Dr Rae, "it is not uncommon to find self-esteem, love of approbation, religious veneration, and benevolence, bearing a prominent part, if not constituting the entire character, and thus producing a slight approximation to humanity. Rush speaks of one who was remarkable for kindness and affection and spent his life in acts of benevolence, though he showed no one mark of reason. Dr Combe saw two, who, though differing very much in other respects, agreed in evincing a strong predilection for religious worship, and for listening to sermons and prayers. Some can recollect names, numbers, or historical facts; some are capable of repeating what they have frequently heard; others are able to sing a few airs, and even to play on musical instruments. Gall saw one at Hamburgh, sixteen years old, who learned names, dates, numbers, history, and repeated them all mechanically, but was destitute of all power of combining and comparing his ideas, and was incapable of being engaged in any employment. Various propensities, such as the sexual feelings, cunning, and destructiveness, they often manifest in an inordinate degree of vigour and activity." To quote again from the same author,—"In short, there is even more diversity in the characters of the idiot and imbecile than in those of the sound; and this truth must not be forgotten, if we would avoid the flagrant error of regultaing judicial decisions by rules, which, though perfectly correct in regard to one case or set of cases, may be wholly incorrect in regard to others."

But, in fact, these varieties in kind are only as it were accidents of the individual cases. In their essential character, especially as regards civil capacity, they differ only in degree.

Let us, before quitting the medical view of the subject, glance for a moment at the pathology, so to speak, of imbecility. I am not going to discuss the organic defects with which imbecility is more or less connected; but shall confine what little I have to say to the mental manifestations, and this chiefly with reference to the slighter cases, which are the really important ones in regard to civil incapacity.

If we consider what I have quoted from Dr Rae, and it is entirely consonant with daily observation, it appears that it is one part of the mind only which is essentially the seat of the defect. It is true that in the more aggravated cases we have the whole mind defective, or, it may be, absent altogether; but when we look at the milder forms, we see that the essential defect lies in the intellectual powers; for the active powers, the appetites and desires, and the affections and moral sentiments, may not only not be absent, but some of them may be predominant, and yet the individual be, in the strictest sense, imbecile. Thus, to say nothing of the sexual appetite, so strong in many imbeciles, we have in one case, in marked activity, the desire of esteem; in another, the benevolent or malevolent affections; in a third, self-love, in the shape of vanity; in a fourth, the moral faculty manifesting itself, by a perception of right and wrong, religious veneration, etc.; and yet, along with all these, that deficiency of intellectual power which stamps each case as imbecile.

And when we look at what is wrong with the intellectual powers, the first thing we notice is that there is no distortion of them, none of the delusions which characterize the insane. In the imbecile it is weakness, not perversion of the intellectual powers. No doubt many of the intellectual faculties, especially the perceptive, may be in good preservation, but that which is essentially amiss is the judgment and power of reasoning. It is this which makes the imbecile what he is, both in the eye of the psychologist and of the jurist, both as an object of scientific study and as an object of judicial protection. He does not reason erroneously and form false judgments from the data before him; this implies mental activity. He may attempt to form a judgment, but the task is too much for him; he either forms no judgment at all, or he gets only half way in the process. From his defective power of comparing he leaves out of view one or more data essential to the mental process, and unable to carry his ideas forward so as to judge of consequences, he takes things as he meets them, acts upon them in reference to the present only, and then becomes the victim either of some stronger mind which can form the judgment, can foresee the consequences, and means to turn them to its own advantage, or he becomes the slave of his own animal appetites, and gives a loose rein to them because he cannot form a judgment as to the consequences of their indulgence. In this way, then, he becomes a proper subject for the protecting power of his Sovereign, and thus, as being incapable of managing his own affairs or himself, he requires to be put to a greater or less extent under the care and authority of stronger minds.

Coke's second class need not occupy us long, they differ from the former only in respect of the mental deficiency not being congenital; and though they come not unfrequently before tribunals in reference to the question of putting them under guardianship during their lives, they are more commonly the subjects of litigation after their deaths, from disputes as to their wills, and as to whether or not they were of disposing mind, a subject on which we do not enter here. Occasional remarkable examples of idiocy occurring from severe mental shock are recorded in books, and in them the amentia is generally so complete as to leave no doubt as to their state. Occasionally, also, this fatuity supervenes upon natural disease: it is occasionally the result of poisons, being, for example, one of the ordinary terminations of the effects of the metal on persons working in mercury mines; it occurs, too, as a consequence of attacks of insanity, and more frequently still, it is senile dementia, the result of organic changes in the encephalon. Few of these cases give rise to any difficulty as regards their being subjects for legal protection, and any remarks applicable to congenital imbecility apply equally to them.

With Coke's third class—those who have the mind not weakened but perverted—we need not deal. A person certified as insane is assuredly *non compos* and incapable. I believe, however, that few of my medical brethren will be willing to attach the same significance which he does to lucid intervals. It is pretty obvious from the importance attached to this point in the descriptions of lunacy by older writers that they often assumed lucid intervals to exist where they did not, and that what were called lucid intervals were only remissions, the patient being held to be insane only when in the paroxysm of mania. That lucid intervals, in the proper sense of the word, *i.e.*, a temporary cessation of the morbid state of mind, do occur, there can be little doubt, but the results of litigation in recent times show, that juries very rarely adopt the view of the reality of lucid intervals, either in regard to civil or criminal questions.

By his fourth class it is obvious that Lord Coke meant merely persons labouring under the temporary effect of alcohol, and it amounts practically to this,—that a drunk man is a fit subject for some sort of protection—that a deed, such as a conveyance of property, would be void, if executed by a person who was so utterly drunk as not to know what he was doing, whilst he would have no privilege of exemption from the consequences of any criminal act done by him when in this state. These are no doubt very important questions; but in relation to civil incapacity the mere consideration of the effects of drunkenness existing at the moment not only does not exhaust the subject, but leaves untouched what is really the important point,—the effect of habitual drunkenness on the capacity of an individual to manage himself and his own affairs. Of this more hereafter.

Looking, then, with a medical eye at the classification of persons whom Lord Coke includes under the common head of *non compotes mentis*, we see at once that it is utterly defective. In the first and second classes he leaves no middle ground between utter idiocy and sanity,—he does not include the very class of persons whose cases are by far the most important,—those where there is some difficulty in saying whether they can or cannot be left to manage their own affairs. In his third and fourth classes he includes only those who are actually maniacal or drunk at a given moment, and leaves no room for considering the effect on their civil capacity, of the state of their minds in the intervals between the paroxysms of insane or drunken delirium. We must admit that all whom he does classify are properly styled *non compotes;* but we must insist that a great many more shall be put into this category. Let us see how far these defects are supplied by the lawyers of the present day.

There seem to be three legal terms in use in modern times to express diseased mental conditions,—*idiot* (of which *fatuous* is an equivalent), *lunatic*, and *of unsound mind.* They are, in fact, those which occur in the rubric of the existing Lunacy Acts, to be used by medical men in filling up certificates of insanity, and they are those which are employed to designate the mental states which infer civil incapacity. It is far from easy to expiscate from the mass of contradictory legal writings and contradictory legal decisions what these terms really mean. I ought, perhaps, to modify this statement so far as to say that there is not so much difficulty as to the two first, which are those designated by Professor Bell, in his "Principles," as inferring *total* incapacity; but the third seems to have been a bone of contention to all who have handled it.

I think that there can be little doubt that, by all modern lawyers, the word *idiot* means total, unremitting, permanent, generally congenital deprivation of reason. "He is without understanding from nativity," says Mr Shelford. "The character here is that of total incapacity, without interval and without cure," says Professor Bell. No doubt much discussion is to be found in books as to the attempts which have been made by some writers to define this total incapacity, such as that "he cannot number or count twenty pence, nor tell who was his father or mother, nor how old he is," etc.; but these so-called tests are neglected in practice, and very properly so, because they are not characters or definitions, but only examples of what may occur in idiocy; and in any disputed case it must be left to the jury to decide, upon the whole evidence before them, whether the party in question is or is not an idiot. But in order to decide this in the affirmative, what they must find is, according to the exposition of the Scotch brieve of idiocy given by Erskine, "that he is entirely deprived of the faculty of reason." In all the cases which have recently occurred in this country the jury have been kept strictly to this, and as we have seen that persons totally deprived of reason form a very small part of those who are idiotic, in the ordinary sense of the term, the practical result is that to find a person an idiot, in any case where the question is really worth trying, is virtually impossible.

The word "lunatic," in modern law, has been used in more than one sense. It has in comparatively recent acts of Parliament been employed as including "any idiot or person of unsound mind, or incapable of managing his affairs." Its general signification in the eye of the law, however, is that of a person who has delusions (subject, of course to inevitable strife as to what is a delusion), whilst to the medical mind it seems to be an old-fashioned, very absurd term, implying an exploded theory as to the influence of the moon on paroxysms of madness. In general it may be held to be equivalent to what in common language is understood by insane, *i.e.*, the mental powers not defective but perverted. The technical form "brieve of furiosity," under which, in Scotland, persons in this state are cognosced, is unhappy, as it seems to imply raving or violent mania, and, as everybody knows, this state is not necessary to make a person truly insane and truly incapable of managing himself or his affairs. It is not common to have brieves of furiosity before our courts, and it is well that there are in Scotland other ways of taking the management of property out of the hands of the incapacitated, for the cases in which it is really important to do this are not those which would comply readily with the brieve of furiosity. The fact is, that in a great proportion of the cases dealt with by the Court of Session as incapable of managing their affairs, there is no delusion, properly so called, to be detected at all, and many a "prodigus" would be left to manage his affairs, to the ruin of himself and his family, if this could be prevented only by cognoscing him under a "brieve of furiosity." Viewed, therefore, in reference to civil incapacity, the terms idiot and lunatic, and the legal proceedings founded on them, are far from satisfactory, though it must be admitted that there is not much practical difficulty in knowing what these terms imply.

It is very different, however, with respect to the more modern legal term—"of unsound mind"—which is the form of expression under which, in England, the great majority of cases are tried, where it is sought to take from a person the management of his affairs. If the terms *idiot* and *lunatic* are hampered by restrictions which render it difficult to apply them to the cases most needing the royal guardianship, this is certainly not the case with respect to "unsoundness of mind;" we would therefore hail it as the great practical solution of all difficulties, if we really knew its meaning as well as we do that of the terms "idiot" and "lunatic." But there have been so many explanations of it by great law authorities, both in books and in courts, by learned writers and learned lords, that we are almost driven to take refuge under the dictum of a lord who cannot even in courtesy be styled learned, and declare the term "of unsound mind" to be "one of those things which no fellow can understand."

One would suppose that this being a very general and comprehensive term, was intended to be used to include every form of mental disorder, and I presume that many of my brethren hold that it is for this comprehensive purpose, that it is placed in the rubric of the form of certificate, to be granted for the purpose of removing an insane person to an asylum. I always use it on the understanding that it will cover any form which mental disease will assume, and this is undoubtedly its proper medical signification, though it is by no means the universal interpretation put upon it by legal authorities.

In older statutes and writings it seems to have been used in this general sense, and it is only in comparatively recent times that a special signification has been given to it. Lord Chancellor Hardwicke maintained that the term implied not mere weakness of understanding, but a total deprivation of sense. Lord Chancellor Eldon, again, gave to it the signification which it is generally supposed to have in reference to questions of civil incapacity; he says, "that the words of unsound mind import that the party is in some such state as is contradistinguished from idiocy and from lunacy, and yet such as makes him a proper subject of a commission to inquire of idiocy and lunacy." If we turn to writers on the subject we do not find any greater accordance. Mr Shelford (On the Law of Lunacy) says, "a *sound mind* is one wholly free from delusion, all the intellectual faculties existing in a certain degree of vigour and harmony; the propensities, affections, and passions being under the subordination of the judgment and the will, the former being the controlling power, with a just perception of the natural connexion or repugnancy of ideas. Weak minds, again, only differ from strong ones in the extent and power of their faculties; but unless they betray symptoms of a total loss of understanding, or of idiocy, or of delusion, they cannot properly be considered unsound. An *unsound mind*, on the contrary, is marked by delusion, mingles ideas of imagination with those of reality, those of reflection with those of sensation, and mistakes the one for the other. And such delusion is often accompanied with an apparent insensibility to, or perversion of, those feelings which are peculiarly characteristic of our nature. Some lunatics, for instance, are callous to a great sense of affection, decency, or honour; they hate without a cause those who were formerly most dear to them; others take delight in cruelty; many are more or less offended at not receiving that attention to which their delusions persuade them they are entitled."

Here, then, it is obvious that this standard author attaches to this disputed phrase chiefly the meaning of ordinary insanity characterized by delusions, change of character, etc.; and he also includes under it complete idiocy, but specifically excludes from the category of unsound minds those which are weak enough not to be able to manage their affairs properly, but are neither truly insane nor wholly idiotic. On the other hand, Mr Amos, in his Lectures, says, "that the legal acceptation of the term 'unsoundness' is not very easy to define, for it is neither lunacy, idiocy, imbecility, or incompetency to manage a person's own affairs." I venture humbly to suggest that the mind of one who is neither lunatic, nor idiotic, nor imbecile, nor incompetent to manage his own affairs, is pretty nearly a sound mind. Mr Amos says, in conclusion, "that it is a legal term, the definition of which has varied, and cannot even in the present day be stated with anything like scientific precision." This is not the most comforting or reassuring of conclusions when we consider that "unsoundness of mind" is the very thing which, in every case of interdiction in England, medical evidence must prove and juries must find. We fall back, therefore, upon the judgment of Lord Dundreary.

It is obvious that all this confusion arises from injudicious attempts at defining what cannot be defined, and in classifying cases which are rebellious to all classification. If we will only look at the subject with a medical eye, we see that we have two distinct categories in which all cases of mental imperfection may be placed,—those where the mind is weakened, those where it is perverted. If we have these, what use is there of this *tertium quid*, which may include both of these or none of these, which may mean both of them, or may mean something different from both, or may mean nothing at all, and of the meaning of which we know only this that we know nothing. But why have recourse to two categories at all? If we can only get leave to use the term "unsoundness of mind" in its proper common-sense general meaning, we need no other than it. Let us, in reference to all cases of civil incapacity, determine first of all that the mind is unsound, that is amiss in some way or another, and then let us state under what form of unsoundness, whether weakness or perversion, whether total or partial, the case before us comes; whether this weakness or perversion does really or does not render that particular individual incapable of managing himself or his affairs; to what extent he is so incapacitated, therefore to what extent he must be put under the charge of others; and thus restoring the term unsoundness of mind to its right place, we get rid of these profitless discussions as to what is the meaning of it, or of idiocy, or of lunacy. If we do this we may safely pack up in one box the dicta of Lords Hardwicke, Eldon, and Dundreary, shove them into the darkest cellar below the House of Lords, turn the key upon them to prevent them coming up stairs and, as Bartoline Saddletree says, "batherin' their lordships," and thus leave each case, as must practically occur anyhow, to be judged of on its own merits, according to the kind of evidence, general and medical, which is brought before the court.

The second part of our subject to which I proposed to direct your attention, is the machinery by which the royal guardianship is brought to bear upon those who are incapable of taking charge of themselves. On this point I must not expatiate: *first*, because to a large part of my audience it is much better known than to myself; and, *secondly*, because it is to be found, by those who wish to be informed about it, authoritatively given in the ordinary law books. I confine my remarks to the proceedings in Scotland, merely observing that those in England are more limited in their application and more expensive. It is perfectly clear that some reform is wanted in this latter respect, when we look at the records of recent English cases, such as that of Mrs Cumming in 1846. In this case the inquiry lasted sixteen days, and cost £5000. "In fact," says Dr Taylor, "a large portion of the lady's property was spent in determining, by a verdict, that she was insane; and there was an intention that the remainder should be expended in reversing the decision, when the unfortunate lady died."

The process in England may shortly be said to be this. A petition by any person who can instruct an interest in the party, is presented to the Lord Chancellor, who, in virtue of a special warrant issued to him by the Crown, acts here for the Sovereign. This petition must be supported by medical evidence, to the effect that the party is insane, whereupon the Chancellor directs a commission to issue to one or more persons—generally required to be learned in the law, directing the inquiry by commissioner and jury into the facts of the case. This is the well-known writ *de lunatico inquirendo*. The commission thus authorized directs a precept to the sheriff, commanding him to summon a jury, who, when they meet, hear testimony—on both sides, if desired—on the matter submitted to them, and, after being charged by the commissioner as to the law of the case, return a verdict as to whether or not, from unsound mind—from being *non compos*—the party is incapable of managing his estate.

This is essentially the proceeding followed in America, with this notable addition, that habitual drunkenness as well as lunacy may be inquired into as a cause of the incapacity.

In Scotland there are three processes by which a person incapable of taking charge of his affairs may be dealt with,—cognition, interdiction, and the appointment of a *curator bonis*.

The process of cognition is under a brieve of idiocy or furiosity, which is proceeded with in this wise. Any one who has an interest—relationship appears to be sufficient—takes out, or, as it is called, purchases from the Chancery, a brieve directed to the Sheriff of the county in which the supposed incapable resides, and this brieve must not only be submitted to the formal processes constituting publication, but must be served upon the person who is the subject of it. The brieve is in Latin, which, of course, is intelligible to the Sheriff, and is accompanied by a claim by the petitioner, happily in the vernacular, which is addressed to the "GOOD MEN OF INQUEST," setting forth what the petitioner means to prove. If the brieve be one of furiosity, it directs inquiry to be made "*se sit incompos mentis, prodigus et furiosus, viz., qui nec tempus, nec modum impensarum habet sed bona delaceranda profundit.*" A jury is then impannelled to try the case, taking evidence both medical and general as to the state of mind of the party, one essential element being, that he shall himself be produced before them, so that they may judge of his state for themselves. If they are dissatisfied with the evidence they dismiss the claim. If they are satisfied, they return, or, as the technical word is, retour the brieve to Chancery, and it then becomes a warrant for letters of curatory to issue, to the person entitled to take charge of the thus cognosced party. This person is the nearest agnate, or relative on the father's side, provided he be proved to be of lawful age, capable of managing his own affairs, and find security for his intromissions with the estate of his relative. These proceedings before the Sheriff are not necessarily final. The case may be appealed to the Supreme Court, and tried over again; but this does not concern us here. The result of the verdict, then, if affirmative of the claim, is to reduce the party cognosced to the condition of a pupil, who has no personal status, whose deeds are null, and for whom everything has to be done through the tutor-at-law.

The proceedings under brieve of idiocy, *mutatis mutandis*, are the same as under brieve of furiosity, the brieve directing inquiry to be made—"*si sit incompos mentis fatuus et naturaliter idiota, sic quod timetur de alienatione tam terrarum suarum quam aliarum rerum mobilium et immobilium.*"

Now, this form of procedure need not detain us long. It is practically a nonentity, except as regards the wasting of money upon it, because it must find the party totally an idiot, a state of matters which but rarely exists at all, and never in the very cases which alone are likely to come before a tribunal; or it must find furiosity, which certainly need not exist in a person who may be a most proper subject for cognition, so far as his property is concerned.

The other remedy provided by our Scotch law for cases of civil incapacity, is one which, from its simplicity and cheapness, has justly found much favour with English writers; this is the appointment of a *curator bonis.* This is done by the Court of Session, in the cases of those persons, who are not labouring under such an amount of mental incapacity, as would warrant their being cognosced as insane or idiotic. And it is to us medical men of much interest, as recognising this form of mental weakness, natural or adventitious facility of temper, which does not come under the unsatisfactory technical denominations of "idiot," "lunatic," or "of unsound mind," in its legal sense, whatever that may be. The proceedings in reference to *curator bonis* are by petition to the Court of Session, supported by a medical certificate, on soul and conscience, showing the weakened state of the party's mind. If unopposed, it is granted at once; if opposed, it is remitted to the Sheriff of the party's county, to examine witnesses and the party himself; and on the Sheriff's report, the Court decides whether or not it will appoint a *curator bonis*. The powers of this functionary, as the name implies, are limited to the management of the estate; he has no control over the person of his ward.

The third process in Scotch law, or interdiction, may take place either by the petition of the person himself, who feels that he is liable to be imposed on, or by petition of others; and it consists in the appointment of certain persons as interdictors, without whose consent he shall have no power of alienating heritage, "or others pertaining to himself," or of contracting debts.

Now, I think there can be no doubt that these two forms of proceeding, which are of comparatively easy attainment, are very valuable so far as they go; but it will be seen that they are of very restricted operation, being limited to the property of the interdicted party, and this only partially. It is not for me to say how far a party under curatory or interdiction may enter into contracts which would be found binding upon him, but I certainly see nothing in the law to prevent this, and there is nothing to hinder him from entering into the most solemn of all contracts—marriage—and still less is there anything to prevent him from living so as to ruin his health and shorten his life. Our law, therefore, is not free from the reproach often directed against it, that it cares more for a man's estate than for his body or his life. It is clear, therefore, that, for a large class of persons lying between those, who are idiotic or mad enough to be cognosced on the one hand, and those who are weak enough to be debarred from the complete control of their property on the other, there is no adequate provision. They are, no doubt, all cared for as regards their estates, but not as concerns their persons. Yet this is essential to a great many; for whilst for some a *curator bonis* or interdictors may be quite enough, because they are thereby protected from their only besetting danger, the designs of the crafty and greedy, there are many more who require to be protected against themselves, which no curator or interdictor can do.

If there is one illustration more than another fitted to show the necessity for some more careful guardianship of many people of unsound minds, it is to be found in the unhappy class of persons whose mental ailment manifests itself by habitual drunkenness. The American law recognises this as a ground for cognition, but British law takes no notice of it, except in the limited sense of Coke's fourth class, which applies merely to the actual drunken moment, and no further than in vitiating certain acts done in that interval.

I am not going to repeat here the proofs of the necessity for something being done for these wretched people, nor am I going over the arguments to prove, as has been done over and over again, that there is a distinct and easily-recognised difference, between drunkenness the vice and drunkenness the mentally morbid habit. I must be content to assume, that the habitual drunkenness of the cases to which I allude, is connected with mental imperfection. I think, however, that something may be done by a more careful consideration of the kind of mental imperfection on which it depends. It has been too much talked of as a form of insanity, and as most persons attach to that the idea of delusion, they are naturally startled at this term being applied to the habitual drunkard, seeing, as they do, that when he is for a short while kept from brandy, he is in all respects apparently as sane as any other man. True, it may be regarded as a recurrent form of moral insanity, which does not involve the idea of any intellectual delusion at all, and if it be insanity, it seems to fall more properly within this category. But we know that even among medical authorities the existence of a moral insanity unconnected with any intellectual disturbance, is not universally admitted; and we further know, that in law courts moral insanity is always looked on with much doubt and suspicion; and in a matter of this kind, it is of the last importance to remove any obstacle which the opinions—nay, if you will, the prejudices—of men's minds may throw in the way. But it appears to me that we may get rid of much of these difficulties by regarding the morbidly habitual drunkard (let us, for shortness, call him dipsomaniac, though I dislike the word) as one of those persons who labour under that weakness of mind which renders him incapable of taking charge of himself or his property. And it is not by making a forced interpretation of words that I wish to get at this; but I believe that, after all, this is the correct view of the case. In every thing the dipsomaniac comes nearer to the facile imbecile than to the insane. He has no delusions—neither has the imbecile; he does not rave, except when, from want of control over himself, he rushes into drink which makes him for a while furious; no more does the imbecile rave, except when, from want of control over himself, he rushes into fits of ungovernable passion or hideous obscenity. At his sober moments the dipsomaniac promises to the friend who remonstrates with him, that he will give up his drunkenness; but the moment that friend's back is turned, he rushes to the spirit foe as before. And so the imbecile gives ready obedience to the tutor or guardian who commands him; but when his eye is not upon him, plays his fantastic tricks, or works his silly mischief, or wastes all that belongs to him, as before. The imbecile spends his money, if he has any, on trifles and gewgaws, because his power of forming a judgment as to consequences, does not enable him to set the result of his prodigality before himself, so as to lead him to control his lavish propensity; and in like manner, the dipsomaniac, unable to form a controlling judgment as to the consequences of his propensity, cannot set before his mind in its true light his inevitable ruin in soul, body, and estate, so as to enable him to curb his reckless self-indulgence, and spends his money after a worse fashion. The imbecile is often cunning, so is the dipsomaniac; the imbecile will often lie, the dipsomaniac does so habitually, at least on the subject of his habits. The imbecile, when he has perhaps been conducting himself with comparative prudence for a while, will, on a small suggestion from some dangerous person, be led into acts which tend to his pecuniary ruin; and the dipsomaniac, who may for a while have been living temperately and abstemiously, is, by the smallest temptation, led into acts which tend to his corporeal as well as pecuniary ruin. I believe that if we would but look on dipsomania in this light we might devise a way of dealing effectually with it under a very simple improvement of the law; and, under submission to more competent authorities, I am not sure that—at least to the extent of providing curators—we might not deal with it under the law as it stands. Not that I think curatory an effectual remedy; but in many a case it might be better than nothing, and if it did not save the man, it might at least secure something out of the ruins of his estate.

I am well aware that it is the opinion of many, both legal and medical, whose opinions I am bound to respect, that dipsomania should be dealt with by a special legislation. I venture to doubt the necessity and policy of this; there is a great fear in many quarters, of any such legislation militating against the indefeasible right of Britons to enjoy the liberty of the subject, even in the act of ruining themselves and their children. Let us take care lest when we think we are worshippers of the Goddess of liberty, we are not indirectly acting as high priests of Moloch, by letting innocent children pass through a fire as terrible, as that which burned in the valley of Hinnom. I believe that there would be less of this fear of violation of liberty, if dipsomaniacs were regarded as part of a class whom the Sovereign is bound to protect, and whom our law Courts, whilst they deal with them freely, yet protect from improper curtailment of their rights with a jealous care.

I confess that I cannot see the difference between some of those cases where our courts will interfere at once, and those where they refuse to have any thing to do with them. A man of limited means spends large sums of money on diamonds, bracelets, rings, bohemian glass, and other things to which he has no title and for which he has no use. He sits at home grinning with delight at the contemplation of his treasures, and he impoverishes wife and children for whom he ought to be saving money. A petition to the Court of Session and a doctor's certificate are forthcoming, and, most properly, a *curator bonis* is appointed. It does not matter whether that be an efficient remedy or not; we have got at the fact, and that is what we want, that the Court will take cognizance of this man's lavish expenditure. But another man spends his money, not on jewels, trinkets, and ornaments, but on dozens of brandy, and gallons of whisky. He does not sit at home in quiet childish delight, but he is from time to time rushing about the house in fury, or is lying stertorous in beastly intoxication; he, too, is impoverishing wife and children, only worse than the other. But the former is recognised as a man with a weak mind, whereas the latter is called a man with a strong vice. I say that both are equally weak-minded, and both are acting under a propensity which they have no judgment to control. Where is the difference? Each is *rei suæ prodigus;* and, to my mind, if there is any difference at all, it is in favour of the first; he is not so bad a prodigus as the second. His trinkets may at least be sent to the auctioneer, and some of the money may be recovered, but the misspent money of the dipsomaniac is irrevocably gone off in alcoholic fumes—*tenues secessit in auras.*

To return, then, to the subject, from which I have so far made a digression, I hope not quite irrelevant, something is needed to be done, in the way of provision for the cases of weak-mindedness, which stand between those now dealt with by cognition as idiots on the one hand, and those dealt with by the mere appointment of a *curator bonis* on the other. This ought to include something more than a mere care of their estates, and ought to secure for cases which require it—such as the dipsomaniacs—due custody and protection of the person against self-destructive habits, as well as against improper marriages and similar ruinous occurrences, which you cannot get rid of at present, unless you prove fraud and circumvention.

And now, may I be permitted, with much diffidence, to suggest some points to be attended to in reference to any future legislation on this subject?

1*st*, Let all cases of civil incapacity be dealt with on one uniform plan. Let a person duly interested present a petition to the Sheriff of the county, craving inquiry. Let the petition set forth, *imprimis*, that the person whose capacity is impugned is of unsound mind, in the plain common-sense meaning that there is something wrong with him mentally. Let the petition further state in what respect his mind is unsound, whether weak (imbecile), or perverted (insane)—to what degree it is so, whether totally or partially—and what in the petitioner's view is needful to be done for the due management of the case, whether this be the mere appointment of a *curator bonis*, or the complete putting of the party in *statu pupillari*, or putting him in a place of security.

2*d*, Let the Sheriff, on receiving the petition, order two medical men to examine and report on the case, one of whom may be the ordinary attendant of the party, if he has one. These medical men are thus made officers of the court, and are protected from those vexatious prosecutions for only doing their duty, to which too many of their brethren have been exposed. Let the medical men shortly state in their report the character and symptoms of the mental defect, in fact, the grounds of their diagnosis, whether they hold the case to be one of mere facility from defective judgment or defective memory, or insane perversion of intellect, or want of control over the appetites and passions.

3d, Let the Sheriff, with the petition and reports before him, appoint a day for submitting the case to a jury,—a special jury being summoned for the purpose.

4th, Let there be the fullest opportunity for proof, documentary and oral, on the part both of the petitioning and the impugned party, and let the personal examination of him as at present be part of the proof if the petition be opposed. My belief is that now a few dipsomaniacs, to the great benefit of themselves and their families, would consent to anything being done for them, rather than abide this test. The opportunity of producing rebutting evidence may, no doubt, tend to keep up expensiveness more than is desired, but to some extent this is unavoidable, and nothing should be permitted to have the effect of limiting proof.

5th, Let the verdict of the jury say how far they hold the conclusions of the petition to have been substantiated by the proof, and let the judgment of the Sheriff, founded on this verdict, say whether and to what extent the crave of the petition is to be granted.

6th, Let there be an appeal from the local to the Supreme Court by either party, perhaps a judge of the Supreme Court being specially appointed to deal with such cases, either by confirming the judgment of the court below, or ordering a new trial before himself and a jury.

7th, Let there be, as at present, a power of terminating the curatory, or tutory, or custody, by declarator of convalescence, which will be granted if unopposed, or if opposed will be remitted to the Sheriff, to be disposed of like the original petition by trial before a jury.

8th, Let the expenses, as at present, be paid out of the estate of the incapacited party, unless in refusing a petition the jury find that it was malicious, in which case let the petitioner bear the costs.

Lastly, let me say, though it is somewhat foreign to my present object, that I do not see why ordinary cases of true insanity, irrespective of the question of capacity for managing affairs, may not be dealt with under some such form of procedure. All that would be required would be to extend, for cases of urgency, the period during which a person may be kept in an asylum on a certificate of emergency, so as to afford opportunity for reading the petitions and medical certificates to a special jury; and, in doing this, unnecessary publicity might be avoided, by reading the names of the parties as A. B., and I believe that, if this were done, the public mind would be relieved of much of its anxiety as to the exaggerated stories of sane people being immured in asylums, as each case would then, as it were, have the solemn sanction of the verdict of a jury, and an end would be put to scandalous actions against relatives and doctors for doing what is an onerous, ill-requited, and, at all times, most unpleasant duty.

But this is something "by the way." The more essential matter is to do something for improving the mode of dealing with those who are incapable of taking care of themselves and their property. I can only repeat, that if the ideas which I have broached are voted absurd or impracticable, I shall rejoice if some one in setting them aside will give us some plan more suitable—especially more simple. If there should be found any particle of sense in them, I wish that some one would take it up and turn it to better purpose than I can do.

ARTICLE II.—*On the Nature and Treatment of Uræmic Convulsions.* By D. RUTHERFORD HALDANE, M.D., F.R.C.P., Physician to the Royal Infirmary.

(*Read before the Medico-Chirurgical Society,* 1*st March* 1865.)

SINCE the publication of the "Medical Reports," the attention of the profession has been often directed to the frequency with which Bright's disease is accompanied with phenomena indicating an affection of the nervous system. In 30 out of 70 cases observed by Dr Bright, death was preceded by well-marked cerebral symptoms. Dr Christison, in his work on granular degeneration of the kidneys, writes as follows:—"Among the most formidable of the secondary diseases are various affections of the head, more or less allied to apoplexy. It is perhaps not quite correct to consider these affections as secondary. For, as stated under the head of the pathology of this disease, death by coma seems its natural termination, or the mode in which it proves fatal, when life is not cut short by some other affection of an incidental or secondary character."[1] Subsequent experience has fully borne out the truth of this statement. The frequency of the occurrence of cerebral symptoms in connexion with disease of the kidney, their striking and alarming character, and the number of cases in which they precede or occasion the fatal result, were well calculated to attract the attention of the physician, and to lead him to a careful investigation of their pathology and treatment. But additional interest has been imparted to them, since it has been shown that puerperal convulsions are of the same nature as those which accompany Bright's disease. Indeed, the occurrence of convulsions in the course of pregnancy or in connexion with parturition, when perhaps there had been few or no premonitory symptoms, is even more striking than their manifestation in the course of an organic disease with which it has long been known how often they are associated.

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[1] Page 92.

Considerable difference of opinion has prevailed, and indeed still prevails, as to the relation in which the renal affection and the cerebral symptoms stand to one another. The idea that the convulsions and coma are occasioned by arachnitis has long since been set aside; and opinions are now divided as to whether they are due to the presence in the blood of urea, or of carbonate of ammonia. The original idea was, that the presence of urea in the blood occasioned the affection of the brain; but it was soon found that the two circumstances did not stand to one another in a constant relation. Dr Christison repeatedly noticed cases where for weeks the amount of solids discharged by the urine was reduced to one-fourth of the natural amount, without the supervention of any head symptoms. And in one very interesting case recorded by him, the patient "passed no more than two ounces of light urine daily, for nine days before death, yet he remained sensible to the very last minute of his existence, and died simply of inanition and exhaustion from constant vomiting of everything he swallowed."[1] Similar cases were recorded by Dr Bright, Dr Owen Rees, and others. It seemed, therefore, very reasonable to conclude that though there was probably a connexion between the retention of urea and the occurrence of convulsions, the two could not be considered as standing in the direct relationship to one another of cause and effect.

In 1852, Frerichs advanced the opinion that the cerebral symptoms, though not directly due to the accumulation of urea in the blood, are indirectly occasioned by it; the urea, by the action of a peculiar ferment, being converted into carbonate of ammonia, and then giving rise to the phenomena in question.[2] This doctrine was based upon the following statements,—1*st*, that urea, under favourable circumstances, is readily converted into carbonate of ammonia; 2*d*, that in the blood of persons suffering under the symptoms of uræmia, carbonate of ammonia can constantly be detected; and, 3*d*, that carbonate of ammonia, when injected into the blood of animals gives rise to the symptoms of uræmia. This doctrine was extremely plausible, and met with very general acceptance. In fact, supposing the premises to be correct, the conclusion drawn by Frerichs was irresistible.

Regarding the truth of the first statement there can be no doubt; indeed, the extreme facility with which urea can be decomposed into carbonate of ammonia has been a frequent source of error. Unless blood be examined within a very short time after life has been extinct, ammonia will be detected in it; but it by no means follows that the ammonia was present during life. Frerichs' second proposition, that in the blood of persons suffering under the symptoms of uræmia, carbonate of ammonia can constantly be detected, has been distinctly disproved. Without referring to the experiments of Dr Hammond, of the University of

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[1] Loc. cit., p. 95.

[2] Die Bright'sche Nierenkrankheit, p. 111.

Maryland,[1] or to those of Dr Oppler of Berlin,[2] which are very satisfactory, I prefer to allude to the researches of Drs Kühne and Strauch, which seem absolutely conclusive.[3] These gentlemen undertook a series of experiments, in order to determine whether the blood of healthy animals, at the temperature of the body, contains any free ammonia; and whether the blood of animals suffering from uræmia contains any carbonate of ammonia. The ordinary mode of obtaining the gases of the blood, by driving them out by means of a stream of atmospheric air, is open to objection. The authors accordingly had recourse to a method which excluded the possibility of fallacy. They passed a stream of perfectly pure hydrogen gas into a flask containing a little water, and connected with a bent tube containing Nessler's reagent. (Nessler's reagent is made by dissolving two grammes of iodide of potassium in five cubic centimetres of water, saturating this solution with red iodide of mercury, and then adding twenty cubic centimetres of concentrated potash solution. When ammonia is brought in contact with this fluid a reddish brown turbidity or precipitate is formed.) Having proved that the hydrogen passing through the apparatus was pure, they introduced into it blood directly from the carotid of a living animal. They found that under these circumstances the vapour of the blood of healthy animals contained no free ammonia. They found, however, that when the blood was heated to a temperature of about 158°, ammonia was given off, which certainly had not existed in a free state in the blood. The authors then examined the blood of animals suffering from uræmia. They determined in the first place how small a quantity of carbonate of ammonia could be detected in the blood, by driving hydrogen through it, and then testing the blood vapour with Nessler's reagent. By adding known quantities of carbonate of ammonia to known quantities of blood they were able to ascertain that when the blood contained one-millionth part of carbonate of ammonia a decided reaction could be obtained. The authors next induced uræmia in seven dogs; in four of them they tied both ureters at their entrance into the bladder; in the case of the other three they extirpated both kidneys. On testing the blood of these animals after symptoms of uræmia had set in, they were unable to detect any free ammonia. The blood, in fact (in so far as its ammonia reactions were concerned), resembled that of healthy animals. It therefore follows, that if in uræmia the blood contains carbonate of ammonia, it must be in less quantity than one-millionth part. The authors conclude their memoir with the following sentence:—"In conclusion, we may unhesitatingly answer in the negative the question whether the blood in uræmia contains carbonate of ammonia."[1] We are therefore forced to conclude that Frerichs' second statement is erroneous.

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[1] American Journal of the Medical Sciences, January 1861.

[2] Virchow's Archiv, band xxi. heft 3; in Edinburgh Medical Journal. October 1861.

[3] Centralblatt für die Medecinische Wissenschaften, 1864, No. 30,137.

Neither has his third proposition,—that carbonate of ammonia when injected into the blood of animals gives rise to symptoms of uræmia,—stood the test of experiment. It has been shown by Hammond and Oppler, that the symptoms produced by the injection into the blood of carbonate of ammonia are different from those of uræmia or from those which may be induced by the introduction of urea. When carbonate of ammonia is injected the great feature is the induction of violent tetanic convulsions; there is none of that depression of the nervous system which characterizes uræmia, and the convulsions speedily terminate either in the death of the animal, or in its complete restoration to health.

We are accordingly compelled to abandon the theory that uræmic symptoms are due to the decomposition of urea and its conversion into carbonate of ammonia; and, in the meantime at least, to revert to the older view, that they are directly connected with the presence of urea in the blood. Urea when swallowed is by no means a poisonous substance. It sometimes produces vomiting, sometimes purging, sometimes diuresis, but does not appear to be readily absorbed into the blood. When injected into the veins, however, it acts as a poison; but even here there has been some discrepancy in the results obtained. A more satisfactory method of experimenting was that adopted by Dr B. W. Richardson,[2] who introduced a watery solution of urea under the skin or into the peritoneal cavities of animals. Without entering into the details of these and other experiments, I may merely state that it seems proved that urea when introduced into the blood may give rise to the symptoms of uræmia.

Still, I am by no means prepared to accept the conclusion, that the presence of urea in the blood is the sole cause of the symptoms of uræmia. Vital actions are too complicated to render such a purely chemical explanation probable; besides, the facts stated at the commencement of this paper are in opposition to it. While, therefore, the presence of urea is undoubtedly closely connected with the production of the condition in question, it seems certain that some other causes must be simultaneously in operation. What these are we do not know; but it seems rational to believe that in some persons there may be a greater susceptibility to the action of the poison than in others. It is also, I think, not improbable that other conditions of the blood may favour the occurrence of convulsions. Whether the presence of an excess of extractive matter may, as supposed by Oppler, have this effect, has not been ascertained. But it is, I think, extremely probable that a thin and watery state of the blood may, by impairing the nutrition of the brain, increase its irritability and at the same time render it less capable of resisting the effects of a toxic agent.

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[1] I have not myself had an opportunity of referring to Kühne's paper, but am indebted to my friend Dr Arthur Gamgee for the above abstract of it.

[2] Clinical Essays: Uræmic Coma, p. 177.

In connexion with this part of the subject, there is only one other point to which I would allude. All who have seen cases of uræmic or puerperal convulsions must have observed that there are great differences between them. In some the symptoms are markedly apoplectic in character; the skin of the head is hot; the carotids pulsate strongly; the eyes are suffused; the pulse is full and strong. In others the skin is cool; there is no visible arterial pulsation; the pulse is rapid and small. In the former class of cases there seems to be an increased quantity of blood in the head, or an increased tension of the cerebral arterial system. I cannot help thinking that this may in some cases depend upon an increase in the entire mass of blood. When the urine becomes very scanty, there may be some other outlet for the water of the blood, such as the bowels; or the distended veins may relieve themselves by the escape of part of the serum giving rise to dropsy. But suppose that the urine be scanty, that there be no compensating discharge, and that the amount of serous effusion be insufficient to make up the balance, it seems clear that for a time at least the mass of blood must be increased. Frerichs has observed that the disappearance of œdema specially increases the liability to coma, and he explains this on the supposition that the purification of the blood is prevented by the cessation of serous exudations, and that so much the greater quantity of urea will be retained in the blood. Without denying the truth of this explanation, I would supplement it by saying that there is another circumstance in operation tending to produce the same result. The disappearance and cessation of serous exudations determine an increased retention of water in the blood by which its mass is increased, with the probable result of predisposing to cerebral pressure. I shall endeavour to show presently that this question is not of mere pathological interest, but that it is of considerable practical importance.

Before proceding to a consideration of the treatment of uræmic convulsions, I shall read the particulars of a case which will illustrate some of the preceding observations, and will furnish a text for the remarks I have to make on that subject.

John Cosgrove, æt. 17, a native of Ireland, was admitted into the Royal Infirmary on the 29th November 1863, on account of general dropsy of a fortnight's standing. The patient stated that his health had always been good until his present illness. Between two and three weeks before admission, while much overheated by violent exertion, he got a chill, and two or three days afterwards observed that his feet and legs were swollen. He remained at his work three days longer, but was then obliged to discontinue it on account of the stiffness occasioned by the swelling. At this time he had pains in both lumbar regions; his urine was scanty and high coloured; and he had great thirst. His face and abdomen next became swollen, and two days before admission his scrotum became enlarged and œdematous.

When seen on the 30th November, there was slight swelling of the face, which had by no means an anæmic appearance. The scrotum was much distended, and there was considerable œdema of the feet and legs, extending up to the thighs. The skin was moderately moist; its temperature was not increased. The patient complained of no pain; he stated that his appetite was good, that he had considerable thirst, and that his bowels were regular. Twenty ounces of urine had been passed during the last twenty-four hours; it was of somewhat muddy appearance, but its colour was not heightened; specific gravity, 1015; reaction neutral; it contained much albumen; on microscopic examination, numerous fibrinous and granular casts were seen. Heart and lungs natural. Pulse, 76; of good strength. He was ordered a drachm of compound jalap powder, to be followed by the bitartrate of potash electuary when the bowels had been opened. On the 2d December, the quantity of urine was increased to 40 ounces, but had the same characters as on admission. He was directed to have a hot-air bath every night. During the following week he remained in much the same state; the quantity of urine passed daily averaging about 40 ounces. On the 9th, the urine had diminished to 32 ounces; it was of reddish colour, alkaline reaction, and contained some blood. There was no pain in the loins. He was directed to omit the electuary; to have a dose of compound jalap powder, and to continue the hot-air baths. During the next twenty-four hours only 22 ounces of urine were passed. About nine o'clock in the morning of the 10th December, he was suddenly seized with violent epileptiform convulsions, which returned every quarter of an hour in spite of the use of chloroform, which was freely administerd by Dr Rutherford, my house physician. About twelve o'clock he became comatose. Very shortly afterwards I saw him, and found him in a state of complete unconsciousness; he was breathing stertorously; the face was congested; the forehead hot; the carotids and temporal arteries pulsated strongly; the pulse was full and hard; the conjunctivæ were congested, and the pupils much contracted. Under the circumstances I had no hesitation as to the propriety of bleeding, and accordingly venesection was at once performed. Just as the lancet was introduced a violent convulsive attack came on, and as it was impossible in consequence of the movements of the arm to keep the openings in the vein and in the skin in correspondence, no blood flowed. During the paroxysm the respiration was almost completely arrested, and the patient's face became livid. After the termination of the fit, which lasted for two or three minutes, the blood flowed freely, and 10 ounces were withdrawn.[1] Consciousness was not restored, but the strength of the pulse was somewhat reduced. The head was ordered to be shaved, cold to be applied, and a turpentine enema to be administered. I saw him again at four o'clock, and found him still comatose; the convulsions had recurred at intervals. He was in much the same state as at midday; face and conjunctivæ congested; pupils contracted; pulse full and hard. While standing by him a paroxysm came on; it was attended with contraction of the muscles of respiration, and the lividity of the face became rapidly very great, the veins of the head started out like cords. As there was little or no movement of the limbs, I at once re-opened the vein. At first the blood merely trickled slowly away, and was of very dark colour; but as the paroxysm gradually passed off the blood came away freely, and became of a markedly lighter colour. About six ounces of blood were drawn. During the day water was once or twice passed involuntarily; but its quantity could not be estimated, nor its character ascertained. Next morning he was considerably better. After the second bleeding two or three slight attacks of convulsions had occurred; but there had been none after eight o'clock in the evening. He had been conscious at intervals, and at visit put out his tongue when orderd to do so; pulse, 104; of fair strength. Some urine had been passed voluntarily this morning; it was of a muddy brown colour, of acid reaction, density 1022, was highly albuminous, and contained blood globules, and crystals of uric acid. He was ordered to be dry cupped over the loins; to have a hot-air bath, and to take a mixture containing acetate of potash. For the next two or three days he went on gradually improving; the convulsions did not recur; consciousness returned perfectly, and his memory and intellectual faculties were found to be unimpaired. The quantity of urine passed daily rose to about 40 ounces, but the œdema of the scrotum and legs became considerably increased. On the 15th of December, the patient's father and mother expressed their intention of taking him home; and in spite of my most urgent remonstrances persisted in carrying out their intention. He was accordingly removed to a damp, dark, badly ventilated room in the Cowgate.

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[1] Owing to a misunderstanding the blood drawn at this time was allowed to stand for many hours exposed to the air before being submitted to chemical examination; at that time no urea could be detected, but of course under the circumstances this was not to be expected.

Exactly a week afterwards (on the 22d December), he was readmitted, the swelling of his face and legs having considerably increased. There had been no return of the convulsions. At this time there was some impairment of the memory. The urine still contained a good many blood globules with pretty numerous tube casts. He was ordered dry cupping over the loins, hot-air baths, and acetate of potash with sweet spirit of nitre. For the next four or five days there was little or no change in his condition, except that his memory became materially impaired; the quantity of urine passed daily varied from 25 to 30 ounces; the œdema rather increased. At half-past six in the morning of the 29th December, he was seized with convulsions similar in character to those on the former occasion. They occurred at intervals of about half an hour up to 11 o'clock, when they became more frequent. During the first two hours he was conscious in the intervals between the attacks, but he then passed gradually into a state of profound coma. Feces and a small quantity of urine were passed involuntarily. The treatment consisted in the application of ice to the head, and vinegar and beef-tea were administered, at first by the mouth, latterly by the rectum. When I saw him at 12 o'clock I found him quite insensible; the convulsions occurring at intervals of a few minutes. Though there were not such marked evidences of cerebral congestion as on the former occasion, I thought it right to bleed him, and accordingly eight ounces of blood were drawn. The blood flowed rapidly from a free opening, and was caught in a funnel introduced into a bottle. The bottle was immediately corked and sealed up, and was sent without delay to Dr Arthur Gamgee for analysis.

After the bleeding the fits recurred with diminished frequency and severity, and ceased at 6 A.M. on the 31st. During this interval he remained in an unconscious state, but was able to swallow. A little urine and feces were passed in bed. The pulse fell to 92 and became soft. In the course of the 31st of December, he was seized with watery diarrhœa. At this time the treatment consisted in the use of the hot-air bath, and in the administration of beef-tea and small quantities of brandy. In the morning of the first of January, he was semi-conscious, and spoke though incoherently; in the course of the day he improved considerably, and in the evening expressed a desire to get up to stool. Some urine was passed voluntarily; its specific gravity was 1015; reaction slightly acid; contained neutral phosphates, a few blood globules, renal epithelium, and compound granular corpuscles. The swelling of the face, legs, and especially of the scrotum, had considerably abated since the occurrence of the diarrhœa. Next day, he expressed himself as feeling much better, and said that his head felt clear. The diarrhœa, however, had stopped, and the œdema was a little increased. He complained also of a little pain in the back. Urine, 36 ounces. Pulse 84, of fair strength. Brandy was ordered to be stopped; he was directed to be dry cupped over the loins, and to continue the hot-air bath. He continued in much the same state till the 8th of January, when he complained of giddiness, with slight headache. At 2 P.M. he had a convulsive fit; it was of epileptiform character, but was much less violent than on the former occasions. There were few or no indications of cerebral congestion; the pupils were widely dilated; the pulse was soft. He was put under chloroform, and was kept under its influence for an hour. During the next twenty-four hours he had twelve fits, but was quite sensible between the attacks. Chloroform was administered several times, and always with relief to the symptoms. Latterly the fits recurred at intervals of about an hour. At 2 P.M. of the 9th, he had an attack, and at 3 o'clock, when another was expected, chloroform was given, and he was kept under its influence for two hours. After this there was no recurrence of the fits. On the 10th, he was much better; he was quite rational, and expressed himself as free from pain. During the next few days the œdema gradually increased; the quantity of urine could not be exactly ascertained, as part of it was passed in bed and at stool. On the 15th, a blush of redness showed itself on the lower part of the abdomen and right thigh; this increased, and on the following day these parts had a distinctly erysipelatous appearance. From the time of the occurrence of this symptom his strength rapidly failed, and he died quietly at eleven in the evening of the 16th.

No post-mortem examination could be obtained; indeed, so determined were his friends that no attempt of the kind should be made that the father of the patient sat at the door of the dead-house until a coffin could be made for the removal of the body.

Dr Arthur Gamgee has kindly furnished me with the following report of the analysis made by him of the blood drawn on the 29th of December.

" The blood examined was sent to me from the Royal Infirmary in a corked bottle, which had been very carefully sealed. When I received it, it was still warm. On opening the bottle and testing the temperature with the thermometer, I found it to be 60° Fahr., the temperature of the laboratory being 54°. The analysis was then immediately commenced. In conducting it I had in view the determination, 1*st*, of the existence of free ammonia or carbonate of ammonia; 2*d*, of urea.

" 1. A watch-glass in which a drop of pure hydrochloric acid had been placed was placed over the bottle, which was very gently heated. After being exposed for some time the acid in the watch-glass was evaporated, but no crystals of chloride of ammonium could be detected with the microscope. The experiment was repeatedly performed with exactly identical results.

" 2. The greater part of the blood was evaporated to dryness, and treated with absolute alcohol. The alcoholic solution was evaporated to dryness. The residue was heated with a little distilled water, filtered and concentrated. The fluid thus obtained was divided into two portions; to the first nitric acid was added, to the second a little oxalic acid. The solutions were placed over sulphuric acid, when after some time most characteristic crystals of the nitrate and oxalate of urea were obtained."

Instead of making the above case the subject of much especial remark, I shall refer to it chiefly to illustrate the observations I have to make regarding the treatment of uræmic convulsions. The case, it will have been remarked, was in some respects an unfortunate one. It was unfortunate that the parents of the patient absolutely refused to permit a post-mortem examination; for although there could be no doubt that the case was one of acute Bright's disease, it would have been satisfactory to have made out minutely the condition of the kidneys; but it was doubly unfortunate that the parents would not permit their son to remain in the hospital after his recovery from the first attack of convulsions. At that time he was going on altogether favourably, and had he not been removed, I think his ultimate recovery was not to have been despaired of. But his parents were so determined, that they removed him in opposition to my strongest remonstrances; being influenced, I fully believe, by the impression, that if their son died, I would have insisted upon examining the body. The patient was out of the hospital for seven days, and when readmitted, not only was he in a worse state than when he left, but valuable time had been lost, and the future prospects of treatment became so much the less encouraging. From the very first the case was a markedly severe one, and attempts to restore the flow of urine were, to a great degree, frustrated, the extremely irritable condition of the kidneys rendering the use of even the mildest diuretics very difficult, the urine, in fact, being scarcely ever free from blood.

I now pass on to a brief consideration of the principles upon which the treatment of uræmic convulsions should be based. With regard to their prophylaxis I have but little to say. Our object should be to relieve renal congestion, to call the kidneys cautiously into increased action, and to assist the depuration of the blood by increasing other secretions. The first indication may be fulfilled, wet or dry, by cupping over the lumbar region, and by the use of hot fomentations. The second is attained by the cautious use of mild diuretics; the bitartrate, acetate, and bicarbonate of potash being perhaps amongst the best. For the third indication we call the skin into action by the hot-air bath and diaphoretics; while hydragogue cathartics, when there is no contra-indication, are useful in promoting elimination by the bowels. It is a more difficult point to determine what is the most suitable treatment during the paroxysm. The first and perhaps the most important question which arises is with regard to the propriety or the reverse of employing bloodletting. There is considerable difference of opinion upon this point; while some maintain that both in the convulsions of Bright's disease, and in puerperal eclampsia, bloodletting is the most efficient remedy, others are of opinion that venesection is injurious, and that it ought, as a rule, to be avoided. Perhaps the most prevalent doctrine on this point is, that although we should not be justified in bleeding merely because convulsions are present, it may be necessary to relieve congestion by withdrawing a moderate quantity of blood. Even those who are of opinion that convulsions are produced by a deficiency rather than by an excess of blood in the brain, recommend moderate venesection where the brain is gorged with black blood, and where an attack of apoplexy is imminent.[1] The question then arises, how are we to determine in what cases bleeding is likely to be useful. In extreme cases there can be no difficulty; where we find a hot skin, pulsating carotids, suffused conjunctivæ, stertorous breathing, and a full pulse, we are not likely to do harm in having recourse to the lancet; on the contrary, where the face is pale, the skin cool, and the pulse soft, we shall not be tempted to employ active depletion. But there are numerous cases intermediate between these where the practitioner is often at a loss to decide on what grounds his practice as to bleeding should be regulated. Here his object should be to determine whether or not the quantity of blood in the brain is increased, and whether that organ is subjected to injurious pressure. For this purpose most valuable information may be derived from an examination of the pupil. It appears to be undoubted that in general the size of the pupil is an index to the state of the vessels, not only of the iris, but of the brain. When the vessels of the head are full, the iris is broad and the pupil is small; on the contrary, when the cerebral vessels are emptied, the iris is narrow and the pupil large. Thus, when an animal is hung with its head downwards, so that the vessels become turgid, the iris becomes broad and the pupil small. On the contrary, when the vessels of the head are emptied by bleeding, or by tying or compressing the carotids, the iris becomes narrow and the pupil large. In the patient suffering from acute meningitis the pupils are greatly contracted; while in the hysterical or chlorotic female they are widely dilated. Dr Radcliffe has some valuable observations on this point in reference to the propriety of employing belladonna as a remedy in cases of epilepsy and other forms of convulsive disorder.[1]

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[1] Uræmic Convulsions of Pregnancy, Parturition, and Childbed, by Professor Braun, translated by Dr Matthews Duncan, 1857, p. 57. Lectures on Epilepsy, Pain, Paralysis, etc., by C. B. Radcliffe, M.D., 1864, p. 250.

In the case of Cosgrove I was guided a good deal by the state of the pupil in making up my mind as to the propriety of bloodletting. In the first attack the pupil was very much contracted, and the results of venesection were thoroughly satisfactory; in the second attack the contraction was not so decided, still the bleeding seemed to be rather beneficial than otherwise; while in the third attack farther depletion was contraindicated, both by the general symptoms and by the dilated condition of the pupil. Cosgrove's was, I think, one of those cases in which there was probably an increase in the mass of the blood, and it was to a slight diminution in its amount and a consequent relief to the tension in the cerebral vessels that I am principally inclined to refer the relief afforded by venesection. On the whole, the conclusion at which I have arrived in regard to bloodletting in convulsions is, that a contracted pupil is generally an indication for depletion, a dilated pupil for refraining from it.

The only other point in regard to the treatment of uræmic convulsions to which I shall refer, is to the employment of chloroform. It is certain that the inhalation of ether or chloroform has the power, for the time, of restraining convulsions, and this circumstance has been widely made use of, especially in obstetric practice. Many cases have been recorded in which the favourable termination of puerperal convulsions must be ascribed to the employment of chloroform. But we cannot expect chloroform to be so efficacious in the convulsions of Bright's disease. The action of chloroform is only temporary; it suspends the convulsions but does not remove their cause. Thus, Kussmaul and Tanner say, "Rabbits, when thoroughly stupified by ether, cease, as we have often observed, to be liable to convulsions during death from hæmorrhage, or when the arteries of the neck are compressed; but on recovering from their stupor they again become so."[1] Hence we can understand why chloroform should be more useful in puerperal than in simple uræmic convulsions. Obstetricians are, I believe, agreed that when convulsions are present it is desirable to accomplish delivery as speedily as possible; and it is evident that much good may be effected by keeping the convulsions temporarily in abeyance until the child can be removed. In the convulsions of Bright's disease, on the other hand, there is no crisis to be gone through after which we can look forward to a probability of their spontaneous abatement; there is a permanent cause in operation, and we cannot therefore expect any temporary expedient to be beneficial. At the same time, while I would not trust in chloroform alone to the exclusion of bloodletting, I believe that there are many cases where, in the absence of the indications for depletion, the inhalation of chloroform will have a soothing and beneficial effect.

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[1] Loc. cit., p. 244.

There are various other points in connexion with the treatment of uræmic convulsions to which I might have directed attention; but as this paper is already longer than I had intended, I shall detain the Society no farther than to read the following propositions, in which I have summed up the principal conclusions at which I have arrived:—

1*st*, In the present state of our knowledge we seem justified in believing that the retention of urea is the chief cause of uræmic convulsions; though the effect of this agent is probably aided by individual peculiarities, and by deficient nutrition and consequent irritability of the brain, the result of the hydræmic condition of the blood.

2*d*, It seems probable that the mass of the blood may be increased when deficiency of the urine is not compensated for by augmentation of other secretions, or by the occurrence of dropsy, and that this condition predisposes to apoplectic symptoms.

3*d*, In cases where symptoms of cerebral congestion are present; especially where well-marked contraction of the pupil exists, bloodletting is likely to do good.

4*th*, In cases where symptoms of cerebral congestion are absent or ill-marked, and where dilatation of the pupil exists, bloodletting is likely to be injurious.

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[1] Kussmaul and Tanner on Epileptiform Convulsions from Hæmorrhage: New Sydenham Society; 1859, p. 8.

5th, Chloroform is valuable as a palliative, especially in cases of puerperal convulsions; but when symptoms of cerebral congestion are present, it should not alone be depended on.

ARTICLE III.—*On the Influence which Consanguinity in the Parentage Exercises on the Offspring.* By ARTHUR MITCHELL, M.D., Deputy Commissioner in Lunacy for Scotland.

(*Continued from page* 794.)

THE counties of Ross and Wigtown present peculiarities, an examination of which may throw some additional light on the subject under discussion. I shall therefore detail more minutely the results of my investigation into these districts, of which a summary is exhibited in the following table:—

COUNTIES.	Total cases examined or reported on.	Number included in column A who were Idiots or Imbeciles.	Number included in column A who laboured under Acquired Insanity.	Number included in column A about whose parentage information could not be obtained.	No. of cases included in A, about whose parentage information was obtained.	No. included in E who were Idiots or Imbeciles.	No. included in E who laboured under Acquired Insanity.	No. of Idiots or Imbeciles (F) between whose parents there was a Blood-relationship.	No. of those labouring under Acquired Insanity (G) between whose parents there was a Blood-relationship.	Total No. between whose parents there was a Blood-relationship.
	A	B	C	D	E	F	G	H	I	K
Ross and Cromarty	189	146	43	68	121	91	30	32	3	36
Wigtown . . .	71	60	11	15	56	47	9	5	1	5
Totals	260	206	54	83	177	138	39	37	4	41

It appears, therefore, that of 177 insane persons, about whom reliable information was obtained, in 41 cases a blood-relationship within the degrees of first, second, or third cousins, was determined. This represents 1 in 4·3, or about 23 per cent.

Even if we take the whole number examined or reported on, and include the 83 about whom no information as to parentage is possessed, we shall have out of 260 cases 41 the offspring of relatives. This would represent 1 in 6·3, or about 16 per cent.

On either supposition, the influence of consanguine marriages in increasing the amount of unsoundness of mind is clear.

On referring to column H it will be seen that of the 41 cases who were the offspring of blood-related parents, 37 were idiots or imbeciles.

Of this class of the insane there is always a considerable proportion whose disease is not truly congenital, having its origin in the early period of extra-uterine life. Among those idiots or imbeciles, however, who are born of related parents, the proportion of this non-congenital idiocy I have found to be smaller than in the general class of idiots. In other words, given one hundred idiots the children of parents related, and another hundred the children of parents not related, it will be found that a larger proportion of the former are due to fœtal disturbances than of the latter.

Further, we find that proportionally a larger number of idiots and imbeciles are the children of related parents, than of maniacs, melancholics, etc., or of those labouring under that which, for convenience, I have called acquired insanity. If we take the whole number of idiots examined, 1 in 5½, or 18 per cent., is the child of related parents. If, again, we take the whole number of cases of acquired insanity the proportion falls to 1 in 13½, or 7·4 per cent. But if we deal only with those of whose parentage we have information, then in the one case it is no less than 1 in 3·7, or 27 in the 100, and in the other 1 in 9·7, or 10 in the 100. From this it appears that the amount of idiocy and imbecility is influenced by these unions to a greater extent than is the amount of the acquired forms of insanity.

It seems scarcely necessary to point out when such a statement is made as that 13 to 15 per cent. of all the idiots in the districts examined were the children of blood-related parents, that it by no means follows that the whole of that percentage is due to the consanguinity of parentage. Indeed, it is quite certain that it is greatly otherwise. All the other causes of idiocy will operate among the offspring of cousins as they do among the offspring of persons not so related; and it must always be remembered that these causes are very numerous and very varied. The idiocy of our country is not due to one but to a great many things, each of which contributes its share to make up the whole: one cause may be more powerful than another, but each influences the total amount. The facts which have been detailed render it very probable, if they do not *prove*, that a blood alliance between parents is one of these causes—influencing unfavourably the amount of idiocy in the land, but they do not exhibit definitely the measure of this influence, though they may and do aid us in estimating it.[1]

The official reports to the governments of Massachusetts and Connecticut, on the amount and condition of idiocy in those states, exhibit a line of inquiry somewhat similar to that with which we are now dealing. The facts elicited by these investigations are briefly as follows:—

In 1846, commissioners were appointed under an act of the legislature of Massachusetts "to inquire into the condition of the idiots of the commonwealth, to ascertain their number, and whether anything can be done in their behalf." The report was prepared by the well-known Dr Howe.

Of 574 idiots, with whom this report deals, the parentage was ascertained in 359 cases, and of these 17 were known to be the children of parents nearly related by blood. From collateral evidence it was concluded that at least 3 more cases should be added to the 17. This would show that more than one-twentieth (or about 5 per cent.) of the idiots examined were the offspring of the marriage of relations. Dr Howe says:

"The statistics of the 17 families, the heads of which, being blood-relatives, intermarried, tells a fearful tale.

"Most of the parents were intemperate or scrofulous; some were both the one and the other; and, of course, there were other causes to increase the chances of infirm offspring, besides that of the intermarriage. There were born unto them 95 children, of whom 44 were idiotic, 12 others were scrofulous and puny, 1 was deaf, and 1 was a dwarf! In some cases all the children were either idiotic, or very scrofulous and puny. In one family of 8 children 5 were idiotic."

All the 17 cases referred to appear to have been cases of true congenital idiocy.

Again, in the report of the commissioners on idiocy to the General Assembly of Connecticut, in 1856, we find that 310 out of 531 cases reported an adequate cause. Of these causes, consanguinity of parents was the probable one in 20 cases, or nearly 7 per cent.

On examining the results of the inquiry, however, we find that the question—"Were the parents of the idiot related by blood?" was only answered in 160 cases,—in 140 negatively, and in 20 affirmatively. Of those idiots, therefore, the relationship of whose parents was ascertained, one-eighth were the offspring of consanguineous marriages, or 12½ per cent.

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[1] There are many causes of idiocy which are undoubtedly of much greater power than kinship of parentage. Hooping-cough, scarlatina, and measles, for instance, produce a large amount of the idiocy of Scotland, as they do probably of other countries. Hooping-cough, in particular, is very often followed by imbecility or idiocy. We are too apt to think of idiocy as a congenital condition or disease. In point of fact, however, a large proportion of the idiocy of the country has an extra-uterine origin, and is acquired and not congenital.

Of the parents of the 20 the degree of relationship was as follows:—

Children of own cousins,	12
〃 of second cousins,	3
〃 of third cousins,	1
〃 of double cousins,	3
Great-grandparents' own cousins,	1
	20

Deaf-mutism in Connexion with Consanguineous Marriages.

Deaf-mutism is another of those defects which kinship of parentage is alleged to produce in the offspring; and we shall now endeavour to show how many of the deaf-mutes in this country are the issue of parents related to each other by blood, and how many of parents not so related.

The writer of an able article on the Vital Statistics of the Deaf and Dumb, in Knight's Cyclopædia, says, that, next to hereditary transmission, consanguine marriages are the most fertile source of deafness. "Every institution in the kingdom," he adds, "bears witness to this fact, in the numerous cases of pupils who are the offspring of first cousins."

Mr Burton, of the Liverpool Institution for the Deaf and Dumb, in a paper published in the Medico-Chirurgical Journal of that city (Jan. 1859), says—"In an inquiry which I made some time ago, from a large number of persons, I found that about every tenth case of deafness resulted from the marriage of cousins."

Dr Peet, the well-known Principal of the New York Institution, "gives it as his impression, that there is one such case, on an average, out of about every ten congenital cases, in which the inquiry has been made;" and he estimates that there is in that part of the United States hardly one family in fifty of which the parents were first or second cousins; so that, if, in the general population, there be 1 child congenitally deaf in 3600 born, there would be 1 in 700 of the children of cousins, or five times as many.

Regarding Ireland, we have some valuable facts, to which I shall presently allude, illustrating the extent of this cause of deaf-dumbness; but as regards America, much as it has added to the literature of deaf-mutism, and few countries have done more, on this point we have nothing but *impressions*, and I have searched in vain for *facts*. Not more successfully have I searched for such facts in the reports of our own institutions. Many of these are documents of the highest interest, and full of precise and well-arranged information; yet on this particular point we have little beyond the general expression of a strong opinion that such marriages are a fruitful cause of this calamity.

Under these circumstances, I resolved to write to the superintendents of the sixteen institutions in Great Britain, requesting information as to the number of pupils under tuition who were the children of blood-related parents. I have to thank *all* of these gentlemen for a ready and courteous reply. In six of the sixteen cases, however, it was found impossible to give the information I desired. The state of matters, however, in the remaining ten is represented in the table below.

	1.	2.	3.	4.
	No. of Pupils in Institutions.	No. of Families represented.	No. of Pupils the offspring of consanguine marriages.	No. of Families represented.
I. *Scotch Institutions,*— Glasgow, Dundee, Aberdeen, Donaldson's Hospital, .	201	181	12	9
II. *English Institutions,*— Bath, Newcastle-on-Tyne, Swansea, Exeter, Doncaster, Brighton, ...	343	323	16	15
Total, ...	544	504	28	24

From the total number of pupils we must deduct 25 per cent. for acquired deaf-dumbness—a form over which consanguine marriages can have but a small influence. This represents the average proportion of acquired deaf-mutism for Great Britain, and is far below that for the United States, which is 42 per cent., and that for Germany, which is 52 per cent.

This deduction being made both from columns 1 and 3, out of 408 deaf-mute, we have 25·1 whose parents were blood-relations, or 1 in 16·2, which is considerably below the estimate arrived at by Mr Burton and Dr Peet.

If cousin-marriages, however, have no influence in the production of this result, then such unions, in the general community, ought to be to others in the proportion of 1 to 17. I have no figures to show that this is not the case, nor can I obtain them; but I believe all will at once agree with me in considering such a proportion as far too great. The average for Great Britain is probably not more that 1 to 60 or 70.

It will be observed that the 24 cousin-marriages yielded 28 deaf-mutes. Had the same proportion existed through the entire number of pupils, they ought to have been represented by 466 instead of 504 families. There is therefore a greater frequency of two defective members in one family when dealing with the offspring of blood-relations than when dealing with others. In the Irish returns (1851) this is still more evident. 154 cousin-marriages, in which deaf-mutism occurred, yielded no less than 235 mute members.

Dr Peet, in his thirty-fifth annual report, in analyzing Wilde's "Statics of Disease," says, that it appears that "of the Irish deaf and dumb, from birth, about 1 in 16 were the offspring of parents who were related within the degrees of first, second, or third cousins."[1] This is almost identical with the estimate for Great Britain which I have formed, and which is 1 in 16·2. Supposing cousin-marriages to be to others as 1 to 70, it will follow, Dr Peet says, that congenital deafness appears at least four times, perhaps five times, as often from a marriage between cousins as from a marriage between persons not related.

Of the 235 deaf-mutes in Ireland who were the offspring of cousins, only 7 were cases of acquired deafness. This is greatly below the proportion in the deaf-mute population of all Ireland, which shows 11 per cent.[2] of acquired deafness, and 7 per cent. uncertain. Instead of 7, therefore, there should have been 26 cases of acquired deafness. In other words, deaf-mutism, as it appears among the children of cousins, is to a larger extent congenital than when it appears among the children of persons not related to each other by blood.

* * *

We now come to the second mode of investigating this question, which consists in taking certain localities, and collecting the family history of every marriage among the people there, and then comparing those in which a kinship existed with those in which it did not exist.

I have already pointed out the difficulties and disappointments which I encountered in conducting this line of inquiry, and I have also shown that it will be necessary to deal with each locality separately—groupings and comparisons being impossible. This mode of giving the information which I have obtained will, I think, best show the truth of the matter. I shall give it fully, even though some of the details may at first sight appear irrelevant, or opposed to my conclusions, believing this to be the fairer and safer course.

St Kilda.

Early in June 1860, I sent a list of queries relating to the people of this island to Mr M'Raild, the factor, who makes a long annual visit to it. Having reason, however, to fear that he had left the mainland before the arrival of my letter, and understanding that

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[1] I have myself examined Wilde's report, and have some difficulty in seeing how Dr Peet obtains these figures.

[2] This again is greatly below that for Scotland, England, America, or any of the countries on the continent of Europe.

Captain Otter, in the course of his survey of the Western Islands, would probably go to St Kilda, I transmitted a similar set of queries to him, with the request that he would obtain answers through Mr Kennedy the schoolmaster and catechist. From both sources I have fortunately received replies to my communications, and these agree in all important respects. I am further indebted to Captain Thomas, who also visited the island last summer (1860), for much valuable information.

The population consists of 78 persons—33 males and 45 females. Of these only 4 are below the age of five, 6 between the ages of five and ten, 9 between ten and fifteen, 13 between fifteen and twenty, 12 between twenty and thirty, 9 between thirty and forty, 12 between forty and fifty, 11 between fifty and sixty, 1 between sixty and seventy, and 1 between seventy and eighty. At the census of 1851, the population consisted of 110 persons, with that great preponderance of females which still exists—a preponderance which, then as now, showed itself chiefly below the age of twenty. Above that age the difference is but slight. Forty years ago, Macculloch found exactly 103 individuals in 20 families; while Martin, 160 years ago, found 180 persons in 27 families. As regards my present inquiry it is of importance to find that the population is a diminishing, and not an increasing one.

There are 14 married couples on the island, being a fall from 19 at the census of 1851.

In not one of these couples is the relationship between husband and wife that of full cousins. Having in mind the small number of the people, and their complete isolation, this fact surprised me, and differed much from what I expected.

Not less than 5, however, of the 14 are marriages between second cousins. Of these five couples 54 children have been born, of whom 37 died in early infancy, leaving 17 alive. Those who perished, passed away at an age to be reckoned only in days, and of them nothing can be told. But of the 17 survivors it is distinctly stated that *not one* is either insane, imbecile, idiotic, blind, deaf, cripple, deformed, or in any way defective in body or mind.

With regard to the rest of the population, only one insane person was found on the island, viz., C. M'L., who is described as upwards of fifty years of age, and of weak mind. As to her parentage, the schoolmaster was not able to give any information to Captain Otter, who kindly made inquiries at my request. At the date of his last visit she had left St Kilda, and was residing in Harris.

In addition to this woman, I am informed that one or two others are "*slightly silly;*" but, on inquiry, it appears that these are cases rather of bodily than of mental weakness, the result, it is said, of accidents on the cliffs. They are young persons, however, and Mr Kennedy states that, when under his care at school, he considered them "dull, but not silly or wrong in mind."

These particulars show the efforts which I made to arrive at a knowledge of the exact state of the case, and not to overlook those instances of slight defect which, in a backward community, are apt to pass unnoticed. "It is certainly strange," as Captain Otter remarks, "that though they marry so much amongst themselves, there is only one—a spinster—who is weak in intellect." Even this, of course, being 1 in 79, is far above the average for Scotland; but still it does not realize the disastrous results which one would expect to find in a community where more than one-third of all the married couples are related by blood,—even though this relationship is one removed a degree beyond that of full cousinship.

Among the queries which I transmitted, was one asking whether it was generally thought by the people of the island that a blood-relationship between the parents is injurious to the mental or bodily health of the offspring. The answer to this is, that no such opinion is commonly entertained. Martin, however, states that it was otherwise in his time, and that such unions were then condemned. From the fact that no marriage between full cousins exists among them, it is not improbable that the opinion may still operate, as far as is possible in a community so peculiarly placed.

Where the whole number of the people is so small, one or two strangers occasionally settling among them would, practically, be a large introduction of new blood. I, therefore, directed another query to this point, and I learn that of the 28 married persons in the island, who, with their offspring, represent within 4 or 5 of the entire population, there is "only one who does not belong to it, who has been imported, and who is not in the full sense a St Kildian." This person is a woman from Lochinver, who married a native, and who had 14 children, of whom only two live—both unmarried. The common opinion that a stranger very rarely settles in the island appears, therefore, to be correct. At the census of 1851, every person found on the island was born there, except one woman who had come from Sutherland as the wife of a St Kildian. This is the same woman who is still found to be the only stranger. None of her offspring are among the married couples. I am informed that the children of a father or mother, not native, are always the most ready to quit the island,—a natural result, but one by which the benefit of an introduction of new blood is lost to the general community.

The physical condition of the people appears to be good. Captain Otter says, "that when they pass the first fourteen days they grow up robust, healthy, and particularly clean in skin." Captain Thomas describes them as well-made, well-fleshed, good-looking, and smooth-skinned. He found many more fair than dark, the large majority presenting the Norse type. Macculloch says, "The men were well-looking, and appeared, as they indeed are, well fed; exceeding in this, as in their dress, their neighbours of the Long Island." All those who have had opportunities of comparing the condition of the St Kildians with that of the Lewis people, those of Barvas and Shader in particular, seem willing to endorse Macculloch's opinion.[1]

Mentally, the St Kildians are described as intelligent, sharp, cautious, sober, and moral.

In the course of these remarks I have made mention of persons having large families, of whom only two or three remain alive. This, unfortunately, is not the exception, but the rule; and I have now to draw attention to a very dark spot in the vital statistics of St Kilda, viz., the infant mortality, which is enormous. The cause of this is an affection now happily unknown, or all but unknown, to the rest of our country, viz., trismus nascentium.

Out of 125 children, the offspring of the 14 married couples residing presently on the island, no less than 84 died within the first fourteen days of life; or, in other words, 67·2 per cent. This exceeds the mortality from the same cause in the small island of Westmannó, near Iceland, where, on a calculation of twenty years, 64 per cent. of all children died of trismus, between the fifth and twelfth day of life. In Iceland itself, between 1827 and 1837, 4479 deaths are recorded under this heading, or 30 per cent. of the total mortality. In the Faroe Islands, also, this cause of mortality is very great. In the west parishes of the Lewis—Uig and Barvas—it is still known, but it is rare, and it is believed that it is becoming more and more so. St Kilda and the west of Lewis are the only two spots in Scotland, if not the only two in Great Britain, where the disease still exists in an endemic form. The seizure usually occurs on the third day of life, and proves fatal within the week following. It is called in St Kilda "*the seven days' sickness*," and in the Lewis "*the five nights' sickness*."

One curious effect of this great infant mortality is an increased fertility of the women. The average age of the 14 married women is 43½, and the average number of children to a marriage is 9; or 10, if we except the case of one couple without children. This fecundity appears to me to be explained by the infant's death doing away with the period of lactation, and so permitting impregnation again at an early period after parturition. On observing this, I turned with some interest to Schleisner's statistics of Iceland, and I find that exactly the same thing occurs there. He begins by observing that "almost all foreigners who have travelled in Iceland have mentioned the extraordinary fecundity of the nation as something remarkable. It is noticed that marriages with twenty children and upwards occur frequently. After a very careful analysis of the facts, he himself concludes that "the fertility of the Icelandic women is a great deal greater than that of the Danish." He offers, however, no explanation, but the way in which I account for it in St Kilda appears sufficient for Iceland also.

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[1] The backwardness in both islands, however, is excessive; and it has to be observed that it is a condition of backwardness even more than one of poverty. A gentleman, who had seen both places, writes of Lewis, that "it would be difficult to imagine anything more primitive, except nudity and raw flesh;" while in the "Census of Great Britain in 1851, published by authority of the Registrar-General," the dwellings of St Kilda are spoken of as "dirtier than the dens of wild animals."

One woman in St Kilda, at the age of thirty, has given birth to 8 children, of whom 2 live, while two others have born 14 each, or 28 in all, of whom 24 are in their graves. The pestilential lanes of our great cities present no picture so dark as this. It is doubtful if it is anywhere surpassed, unless in some of the foundling hospitals of the Continent.

In writing of a visit to a people whose fecundity is so great, one would hardly expect to meet a remark like the following:—"The absence of children about the houses is most remarkable;" yet, such occurs in the letter of one of my correspondents.

What influence this great infant mortality may have on the surviving offspring, taken *as illustrative of the effects of consanguine marriages*, it is not easy to say. Had trismus been a disease which exercised a marked preferential claim on the feeble, or had it been one from which recoveries occurred, bequeathing cerebral and other nervous lesions to those whose lives it spared, the influence would have been more clear. It does not appear, however, to make any such selection. The most robust children as well as the weakest, those born of vigorous parents as well as those of feeble, those born of incomers as well as those born of full-blooded natives, all appear subject to it, and none recover. I think, however, that this large infant mortality renders the case of St Kilda altogether so exceptional and so peculiar, that it must be used with caution in this inquiry.

The occurrence of trismus in St Kilda appears to me to be closely connected with the character of the houses in which the people live. I have eleswhere[1] described the dwellings of the Lewis people, from which those of St Kilda differ but slightly. I am assured, however, that this slight difference is to the advantage of the St Kildians. Captain Thomas, who has had excellent opportunities of comparing the condition of both people, is of this opinion. He writes me that he regards them "as cleaner, better clothed, better fed, and better lodged." In this he is supported by Macculloch, Martin, and others, and much indirect evidence has been furnished to me leading to the same conclusion. From all I can learn, however, four important things are common to the houses of both places:—1*st*, There is no smoke hole. The thatch is put on nearly as much to be an accumulator of soot as a protector from rain, and it is removed every year for manure. 2*d*, The dung of the cattle, which are under the same roof, or rather in the same room, with the people, is allowed to accumulate below them from autumn to spring. 3*d*, There is a most scanty admission of daylight,—very often none at all but that which enters by the door when open, or by chinks in the wall or roof. It is not a rare thing to find *no special provision* for the admission of light. *4th*, As the thatch or roofing ends in the centre of the wall (which is 5 or 6 feet thick, and built of stone and turf), instead of overlapping it, and so throwing off the rain, the houses in such a climate are always, and of necessity, damp.

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[1] Appendix to third annual report of the General Board of Lunacy, Scotland.

It is a painful thing to see our fellow-countrymen inhabiting dwellings whose construction is so uncomplimentary to human intelligence, and whose internal economy the instincts of the lower animals would scarcely tolerate.

It is a generally received opinion, that "nothing can be more satisfactorily proved than the tendency of a vitiated state of atmosphere to produce trismus."[1] Sixty years ago, in the Dublin Lying-in-Hospital, every sixth child born there died within a fortnight after birth, and nineteen-twentieths of these deaths were attributable to trismus. Dr Clarke blamed the ventilation, improved it, and the mortality fell at once from 1 in 6, to 1 in 19·3; while under further changes, when Dr Collins was master, the whole deaths fell to 1 in 58½, and of that diminished mortality only one-ninth resulted from trismus.

Schleisner attributed the disease in Iceland to the use of birds' excrements as fuel, and birds' fat for lighting purposes, and in St Kilda it certainly happens that the oil of the bird called the fulmar is burned in the lamps, but I cannot believe that this is the cause of the disease.

I made careful inquiry as to the mode of dressing the umbilical cord, but I did not find anything so exceptional in this matter as to lead me to suppose that it was in any way connected with the disease.

In short, I can discover nothing which appears to me to be so probably the cause of this disease in St Kilda as the style of house in which the people live, and I am of opinion that if their dwellings are improved, as has been generously contemplated, one result will be the extripation of trismus, and another, the seeming paradox, that the women will bear fewer children, and yet have more.

Had it been a question of lambs instead of children, the people would themselves, long ere this, have found the remedy for this vast preventible mortality. It may be, however, that, with Macculloch, they regard it as, "politically speaking, a piece of good fortune."

These details must not be regarded as wide of the question of consanguine marriages. In dealing with it, I felt that I had to consider everything bearing on it, directly or indirectly. One pathogenic feature of a locality or people can hardly be studied alone, if we are to avoid unsound conclusions. The proof of this I think we have before us, for the large infant mortality, in itself and in its consequences, obliges us to use the St Kildian experiences with caution and hesitation in the elucidation of the influence of consanguine marriages upon offspring.

[1] West on Diseases of Children, p. 143.

Scalpay.

It is generally supposed that in no part of Scotland are marriages of consanguinity so frequent as in the West Highlands and Islands. When I came, however, to make an effort to get the extent of this frequency definitely stated in figures the result often surprised me,—the popular impression sometimes appearing altogether groundless. The island of Scalpay presents a case in point. The schedule of queries was forwarded to the schoolmaster, who has long resided there, and knows every person on the island. The population he fixes at 341, and the number of married couples at 63, amongst whom it is said that there is not a single case of cousinship. Whether referable to this or not I cannot say, but it happens also, that there is not on the island either an idiot, an insane person, a cripple, or a deaf-mute. There is one case of blindness, but it was acquired in extreme old age.

In the marriage records of the Registrar-General one of the headings, suggested I believe by Dr Stark, is as follows:—"Relationship of parties, if any." In the large island of Lewis more than in any other place was I led to believe that these marriages abounded. The subject is much discussed there, and both the distinguished proprietor, and his chamberlain Mr Munro, have directed their attention to it, as a source of evil to the people. I was, therefore, curious to learn the number of marriages registered there as between relations, and I embraced an opportunity of examining the registers of the parish of Uig, and with this result. Of 103 marriages registered during the five years from 1855 to 1859, a relationship between the parties is recorded only in two cases.

Again, through the kindness of Mr Munro, I possess similar information for the parishes of Stornoway and Barvas. In the first, for the years 1858 and 1859 and for 1860 to October, 109 marriages were registered, and in none was any relationship recorded. In Barvas for the same period, 99 marriages were entered, with relationship given in two cases, and here it was not close, being in the one case that of "second," and in the other that of "third cousins." Out of a total, therefore, of 311 marriages, there were only 4 in which the union was between persons related by blood, or 1 in 78. And this, too, in a district where cousinship between man and wife is reputedly very common, and where the fixity of the population gives such a reputation an *a priori* probability.

My conviction, however, is that the public records do not exhibit the true state of the matter. I think the popular notion exaggerates the reality, while on the other hand the marriage registers understate it.

Let me here detail some facts, which were the result of my own observation in the island of Lewis, and which bear directly on the question we are discussing.

I reported on 35 cases of insanity, and of these 31 were idiots or imbeciles.

When analyzed in their bearing on consanguine marriages it is found that there were born,—

1. Of parents known not to be related,		16
2. Of parents known to be related,—		
Of cousins,	2	
Of second cousins,	3	
Of parents more distantly related,	6	
Of parents who were the children of cousins,	1	
	—	12
3. Of parents of whom nothing could be determined. (This includes illegitimates),		7
		—
		35

It thus appears that *at least* one-third of all the cases show a blood-relationship in their origin. On the supposition that this relationship has no influence on the production of idiocy, we should expect to find it in one-third of all unions in the island. This, however, would greatly exaggerate the frequency of such marriages. So that, after deducting freely for other causes of idiocy, many of which are unusually strong in this island, there still remains a large measure of this calamity, which with good reason we may regard as due to consanguineous marriages.

Bodily malformations are frequent in the Lewis. In the parish of Uig, hare-lip is very common. Nine cases were brought to my own knowledge. In the Lewis, and the parishes opposite to it on the mainland, I saw five cases in which there were supernumerary little fingers, one in which there were two thumbs, and one in which the fingers and toes were webbed. Curvature of the spine, deformity, and lameness were often seen in the island. Cases of congenital blindness and deaf-mutism are also numerous. I saw seven epileptics, several instances of chorea, and many of paralysis.

Berneray.—Lewis.

For information respecting this island, I am indebted to Mr John Macdonald, to whom I transmitted a series of questions, and who put himself to much trouble in order to give correct replies. He is the land-steward, an old residenter, and intimately acquainted with every family.

Berneray, which is in the parish of Uig, contains 427 people, and 74 married couples. Two of these are between full cousins, yielding 10 children, of whom 8 live. Not one of these is either insane, imbecile, idiotic, deaf, dumb, blind, lame, deformed, or in any other way defective in mind, morals, or body. Further, 6 marriages are between second cousins, yielding 20 children, of whom 18 are alive.

Nine of the living children belong to one couple, the remaining 9 springing from 5 marriages. As in the offspring of those who were full cousins, so of these also is the report of both bodily and mental health without flaw.

We have thus a population where every ninth marriage is between blood-relatives; yet, instead of finding the island peopled with idiots, madmen, cripples, and mutes, not one such person is said to exist in it.

Large as the number of such marriages is, it is below the expectations of my informant, who no doubt accepted the popular estimate of their frequency. My attention was directed to Berneray as a place where I should find them excessively numerous, considerably more so even than in the Lewis itself, and such appears to be the case. But had I taken the estimates which were given to me orally or by letter, without subjecting them to actual numeration, I should have been led to conclusions of great inaccuracy. This remark does not apply to Berneray alone, but to many other places to which my attention was drawn as likely to furnish information on this subject, and about which I took occasion to correspond. Of one such, for instance, on the N.E. coast of Scotland, I was told that "about 50 per cent." of all the marriages were between persons related by blood. I visited this village, and satisfied myself that the estimate was enormously above the fact, and that without actual numeration I could not safely use any such general and indefinite information.

Mr Macdonald expresses his astonishment thus:—"I was much surprised to see so few first and second cousins married in Berneray island, and were I not certain of it, as I am now, by a minute search, I would doubt the fact, from the island being inhabited by the present race from time immemorial."

He tells me that the island is remarkably healthy, and that cases of longevity are common. He cites the instance of a man who died in 1859 in his 99th year, having 2 sons, 5 daughters, and 132 grand- and great-grand-children.

One of my queries ran thus:—"In general terms, have you observed in the island of Berneray that the intermarriage of blood-relations affects the offspring injuriously in their bodily, mental, or moral health?" To which he replies,—"I have observed no such injurious tendency in this island." Had he answered this question before collecting the foregoing facts, I feel satisfied he would have done it differently, for immediately after the answer he appends this note:—"In Valtos, a township in this parish, a couple, full cousins, natives of Berneray, have two or three children both deformed and imbecile." The great affliction in this case, associated with a cousinship of the parents, would almost certainly have presented itself to his mind when called on to give the opinion asked, and would probably have coloured his reply had he not had the facts regarding Berneray in figures before him.

Phthisis in the Hebrides.

I have here to direct attention to a singular pathogenic feature of the Western Islands, to four of which special reference has been made. I first heard of it from M. Boudin, whom nothing relating to the geographical distribution of disease seems to escape. Several years ago, when writing to me about the absence of phthisis in Iceland and the Faroes, he pointed out that in Great Britain also it seemed to diminish in frequency as we went North, and that in Lewis especially its rarity was very observable. Now, consumption and strumous diseases generally are believed to be among the most certain results of consanguine marriages; and it appeared to me strange that in that part of Scotland where these marriages most prevail, this form of disease should be notoriously rare. Accordingly, while in these islands, I made careful inquiries on the subject. Immediately after my return, a very able paper on "Phthisis in the Hebrides," written by Mr Morgan, appeared in the Medico-Chirurgical Review.[1] He founds his conclusions on his own observation in Raasay, and on the testimony of a large number of medical men practising in the North-West Highlands and Islands of Scotland, who communicated to him their experience on the subject. The impression which a perusal of this evidence leaves on the mind is, that the disease is not simply comparatively rare, but that it is almost absent. The predecessor of Dr Millar (in Stornoway), when filling up schedules of life insurance, to the question relating to the death of the proposer's relatives by phthisis, is said to have invariably answered,—"No such disease is known in the island." Both Dr Millar and his colleague Dr Macrae, the only two medical men in the island, I understood to say, that though the disease appeared to them rare, it was by no means absent. Had I been left to form a conclusion from what I myself had an opportunity of seeing and learning while on the spot, it would have been to the same effect. The *impression* of its rarity was irresistible, while at the same time I was assured of its presence by actually seeing several cases. I am inclined, however, to think that it is not so rare as Mr Morgan appears to regard it; but the difference of opinion hinges only on the degree.

In July 1860, when in the parish of Uig, on the west coast of Lewis, where I was led to understand the minimum of frequency was attained, I examined the registers for the two years 1858 and 1859. During that period 75 deaths are recorded, and of that number 8, or 1 in 9 are entered as resulting from consumption. Yet in the parish of Harris, immediately to the south of that, Dr Clark, during 32 years, could not remember more than half-a-dozen deaths from phthisis.[2]

It is possible, however, that these 8 people did not really die of phthisis, as I found that, of the 75 deaths, in 2 cases only was the cause of death entered under the certificate of a medical man. From 1855 to 1859 inclusive, only four deaths were so certified. Those who have visited this part of Lewis, and know its extreme inaccessibility, will not wonder at this. From such data, therefore, conclusions must be drawn with great caution.

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[1] Med. Chir. Rev., No. 53.

[2] Med. Chir. Rev., No. 53, p. 484.

It may serve further to strengthen this view, if I give all the causes of the 75 deaths, when it will be observed, that it is not merely in the absence of phthisis that the pathogeny of this district differs from the rest of Scotland :—

1859.		1858.	
3	Consumption.	5	Consumption.
1	Croup.	2	Croup.
1	Asthma.	1	Influenza.
14	Influenza.	11	Old age.
3	Inflammation.	8	Not known.
1	Old age.	4	Inflammation.
7	Not known.	2	Dropsy.
2	?	1	Exposure to cold.
—		1	Rheumatism.
32		1	Epilepsy.
		3	Drowned.
		1	Palsy.
		1	Fever.
		1	Smallpox.
		1	Lockjaw.
		—	
		43	

The north-west parishes of Scotland as well as the Hebrides reputedly enjoy a comparative immunity from consumption.

In Ardnamurchan, with a population of 5000, a medical man assured Dr Browne that, during sixteen years, he had seen only two cases of phthisis. In the neighbouring parish of Kilmallie, however, 306 persons died during the five years from 1855 to 1859, and of these 33 died of phthisis, or about 1 in 9, while pneumonia, asthma, and bronchitis together, killed other 22. In all, 55 appear to have died of thoracic disease, or 1 in 5½.

Struma in its other forms is also said to be rare in these districts, and my own observation, which would have been of little value, from the shortness of my stay, had my attention not been directed to the subject, supports this.

I have introduced these remarks on phthisis in the Hebrides because I felt that, in considering the effects of consanguine marriages on the production of idiocy in this district, the marked rarity of one of the most generally admitted effects of those marriages demanded allusion and comment.[1]

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[1] Since writing the foregoing (in January 1861), I have had an opportunity of perusing Dr Stark's observations on the same subject in the First Detailed Annual Report of the Registrar-General. He seems to regard it as proved that the Western Islands exhibit a marked freedom from consumption, but does not think the registers afford materials for giving the precise measure of the exemption.

Burnmouth and Ross—fishing village on the south-east coast of Scotland.

My attention was strongly drawn to this place as being one in which consanguine marriages were very frequent, yet where the alleged evil results were not found.

I have had three opportunities of visiting this village, and on each occasion I made minute inquiries regarding the frequency and effects of such marriages, and I collected at the same time as many facts as possible bearing on the question.

The village is situated at the foot of the cliffs and close by the seashore. Its position has originally depended on its suitability for smuggling operations, and not on its salubrity, since the houses are necessarily rendered damp and unhealthy by the proximity of the rocks. The colony is not more than 120 years old, and the first settlers are believed to have been of a good stock,—bold and enterprising men. Some of them, in the navy and merchant service, distinguished themselves during the war, and rose to good positions, not through courage alone, but also through a superior intelligence.

The fishermen of Burnmouth at present are tall, strong, active men; swarthy, high-featured, and strongly whiskered.[1] They were long regarded as a saving, provident, and sober people. They are remarkably well and warmly clothed, every man having a good and complete waterproof suit for use at sea. In this respect the contrast between them and the fishermen of the Ross-shire and Morayshire coasts is striking. They are good boatmen, and have large decked boats, with which they go to Yarmouth, the Isle of Man, and the Lewis. Something of the character and spirit of the men is learned from the very names of their boats—"The Dexterous," "The Flying Cloud," "The Speedy," and such like,—contrasting curiously with such names as "The Isaac Main," "The Martha Paterson," etc., so general in the north-east. The two fish-dealers or curers of the village are of their own stock, and not of a different and imported class, as happens so generally in the north. They nearly all read and write, and at present very few children at a school-going age are not regularly there.

The women are also tall, stout, and high featured, have no peculiarity of dress, and do not carry the fish to market. Each family keeps a servant—often the daughter of a hind.

The houses, in spite of their dampness, are clean, orderly, and well furnished, and equal in all appliances for comfort to the houses of the labourers or tradesmen of the district. My description is that of an average house, to which of course there are exceptions. There is, in short, a general evidence of well-being in the whole surroundings of the people.

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[1] They are believed to be of Anglo-Norman origin.

The population of the two connected villages of Burnmouth and Ross is generally estimated at 420.

Careful inquiry only brought to light seven marriages between full cousins, and I could not hear of a single case where man and wife were in the relation of second-cousins. There are, however, many marriages between persons where a blood-relationship is recognised, but which is so distant and undefined as to be without a name.

Of these 7 cousin-marriages the result is seen in the following table:—

	No. of Children born.	No. Dead.	No. Living.	Remarks regarding the Children.
I.	6	3	3	Those living, big, strong, sound, and healthy. Those dead, said to have been sound.
II.	6	...	6	"Never had a headache."
III.	9	1	8	All sound and sane.
IV.	1	...	1	Sound. Newly married.
V.	3	2	1	The living one not robust.
VI.	5	1	4	All sound.
VII.	5	...	5	All alive and sound.
Totals,	35	7	28	

This certainly does not appear to speak strongly against a blood-relationship of parents. Thinking that I might find the evil manifesting itself in the next generation, I made inquiries as to the families of those of the 28 living children who are married. Of these, there were only three, and each of these had married a person distantly related by blood. The result is shown below.

	No. of Children born.	No. Dead.	No. Living.	Remarks regarding the Children.
I.	7	1	6	All healthy.
II.	6	2	4	All the living sound. The two deaths were in infancy.
III.	...	...	...	Barren.
Totals,	13	3	10	

Here again, however, I did not find what might have been expected, and I then made inquiry as to the number of persons defective in mind or body in the whole community, and with the following results. There were found,—

a. Two imbeciles, whose weakness of mind was not great, and who were both self-supporting. Neither was the child of parents related by blood.

b. Two cases of acquired insanity, both women. The disease in both cases resulted from grief and fright on suddenly hearing of the death of their husbands by drowning. The parents of one were very distantly related.

c. One case of epilepsy in a child, whose parents were not related. The fits are not frequent, and the mind not much impaired.

No lame, deformed, blind, dumb, or paralytic person was heard of; and in the school, which was twice visited, and where nearly all the children of the village were seen, strumous sores were not found, nor were the children puny, pale, or languid. They were, on the contrary, merry, active, well clothed, and with a look of substantial feeding. Their teacher, however, considers them to be slower and duller than the other children under his care.

None of the children of cousins were here found defective in mind or body. There was found, however, in the whole community a considerable number of unsound persons—a proportion to the whole population above that for Scotland generally.

On making inquiries as to the number of paupers in the village, I found that there were six—all widows. Of four, the husbands were drowned, and two are aged women (86 and 93), who have outlived husbands, children, and all natural supports. On the whole, this also indicates a favourable state of the community.

It then occurred to me that there might possibly be a larger infusion of fresh blood than was generally thought, and I am inclined to believe that this is the case,—more particularly of late. It is a saying of the district, that "a Burnmouth man never goes to the bankhead for a wife;" and in smuggling times, when secrecy was needed and when it was desirable to keep a knowledge of their doings and connexions among themselves, it is possible that this was nearly true. It is not so, however, now, for five of the married women are known to me to be imported, and there are probably more. All of these were the daughters of hinds, who entered the place as servants. In addition to these there are also many married women who are the children of imported mothers. There is thus a considerable infusion of fresh blood into this small community.

With regard to the rate of infant mortality, the tables which I have given show that this is not small, but it is by no means remarkably large. It further appears, from the parish registers which I searched, that during the three years, 1857 to 1859, 21 persons died, of whom 7 were drowned, and 4 died under the age of six months, and 1 more under ten years. During the same period 33 children were born, of whom 3 were illegitimate.

During the five years preceding my visits only one marriage has been recorded in the registers as between cousins.

The general feeling of the people, as communicated to me, is distinctly against such marriages, which they regard as "bad for the offspring." One shrewd old woman, however, added this important remark,—"But I'll tell ye what, Doctor, bairns that's hungert i' their youth aye gang wrang. That's far waur nor sib marriages."

(*To be continued.*)

ARTICLE IV.—*On Dropsy of the Ovum.* By ALEX. R. SIMPSON, M.D., Secretary to the Edinburgh Obstetrical Society.

(*Communicated to the Society*, 21*st December* 1864.)

THE object of this communication is to bring under the notice of the Society the products of conception obtained from three patients who had been the subject of hydramnios, and to offer one or two observations relating to this complication.

CASE I.—The first preparation consists of the ovum discharged by a poor patient, who came from the country to see Professor Simpson, on account of an enormous dropsical distention of the abdomen. The seat and source of this watery collection had been a subject of discussion between various medical men who saw her in her own home, some of whom thought it ovarian, whilst others seem to have regarded it as ascitic. The patient had suffered from severe pains in the abdomen from time to time; and when I saw her the diagnosis had become easy in consequence of dilatation having already set in in the os uteri so far as just to admit readily the finger, and to permit the protrusion through the orifice of the bag of membranes. Her menstrual history then showed that she was between five and six months pregnant, and that her large size was due to the excessive secretion of the liquor amnii. The membranes were punctured; basinfuls of water evacuated; and some hours afterwards the preparation before the Society was expelled. It is seen to consist of an unusually large bag of membranes, which can be distended with air or liquids so as to assume the ovoid or pyramidal form of the uterus, and when thus distended measures sixteen inches in length, and twelve and a-half inches across its broadest part. Doubtless, when this sac formed the lining membrane of the uterus and was supported by its strong parietes, it had been capable of much greater distention, and the measurements of the cavity of the organ before the escape of the contained fluid must have been considerably greater then is represented by these figures. In what corresponds to the back wall of the uterus the sack presents the large placental mass of eleven by eight inches in diameter; and through the transparent walls two fœtuses are seen lying in the common cavity, and having their respective cords running into the placenta towards opposite margins.

It is no uncommon thing to have hydramnios associated with the birth of twins. Kilian,[1] for example, in treating of this complication, notes it among "the very striking facts that so frequently twin-children are associated with dropsy of the ovum;" and Hatin[2] goes so far as to say that "dropsy of the amnion is more frequent in double pregnancies than in those with a single fœtus." Dr M'Clintock,[3] in his masterly and only too short memoir on this subject, speaks of as many as eleven instances of twin-birth among thirty-three cases of hydramnios that have come under his observation. But the present case differs from any of those noted by Dr M'Clintock, in that both twins are contained in one common distended cavity, whereas, ordinarily, the fœtuses are located in separate compartments, only one of which became affected with dropsy. The patient made a good recovery, and there was only one other noteworthy feature in connexion with her delivery to which reference will be made anon.

Case II.—On the evening of 20th December I was sent for to attend the wife of a medical practitioner in her labour. She had previously given birth to two well-developed healthy children. She had ceased to menstruate in May 1863, and had gone on well up till about six weeks before the date of her confinement, when she had accompanied her husband on a visit to Shetland, where she had had frequent occasion to make lengthened journeys in carts,—sometimes furnished with springs, sometimes without. She had suffered great inconvenience from the jolting of the vehicles on the rough roads, and had often been obliged to double herself together in consequence of the distressing sensations experienced in the lower part of the abdomen. From this date she began to increase in size with uncommon rapidity, and during the last two weeks began to suffer great discomfort from the increasing bulk. Spurious pains, too, set in from time to time in different parts of the uterine parietes, and added much to her discomfort. These became tolerably severe and constant on the evening of the 19th, but remitted sufficiently afterwards to allow the patient to get a night's rest. In the course of the forenoon of the 20th they again recurred, and towards evening became tolerably constant and severe, so that when I saw the patient about 11 o'clock P.M., the os uteri was dilated to more than the size of a crown piece, and was occupied by a firm bag of membranes which projected from it in a somewhat conical form. The fœtal presentation could not be made out, for it was with difficulty it could be reached, and when touched, it at once receded from the exploring finger. The fœtal heart-sounds could occasionally be heard on auscultation over the uterus, but the great mobility of the fœtus prevented them from being easily detected. When the os uteri had become a little more relaxed and dilated, the membranes were ruptured artificially; and after the escape of large quantities of fluid, the orifice became filled up with a presentation which was altogether abnormal, and which, as I stated at the time to the patient's husband, I could diagnose to be nothing else but the deformed head of an anencephalic child. A few vigorous pains sufficed for the expulsion of the child through the pelvic canal, and the delivery of the afterbirth speedily followed.

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[1] Die Geburtslehre, ii. band, p. 456.

[2] Cours complet d'Accouchemens et de Maladies des Femmes et des Enfans, 1835, p. 426.

[3] Clinical Memoirs on Diseases of Women, 1863, p. 376.

So far the case had gone on most satisfactorily in all respects as regarded the mother. But now there supervened a symptom to which I must ask your special attention, and which would have been a source of the greatest apprehension and alarm, had I not been prepared for its occurrence by having already witnessed it in the patient whose case I first related. The symptom referred to was the supervention of a very severe and prolonged shivering fit. In the first of the two patients, this rigor came on in the interval between the birth of the two fœtuses, and was of such a character that the whole bed shook from the violence of the patient's tremblings. It continued for some length of time too, in spite of the exhibition of hot drinks and stimulants, and finally passed off by leaving her perspiring profusely from every pore in head, body, and limbs, till all her clothes were soaked with wet. There was another but barely noticeable rigor experienced after the extrusion of the second fœtus. The darkest results were anticipated from the occurrence of a symptom so alarming, but the patient made an excellent and speedy recovery. And now, in the second case, after the patient had been freely delivered, and she was wakening up from the effects of chloroform, when she began to yawn and complain of cold, I was quite prepared to witness without alarm the onset of a rigor. The rigor did supervene, very much modified, however, by the use of a hot drink, which induced vomiting; and it terminated in a hot stage, which was sufficiently distinct, though not marked by the very profuse perspiration witnessed in the other case. It was succeeded after short intervals by two other similar but slighter shivering fits, but was attended with no untoward results, and the patient made a fair recovery.

As regards the cause of the hydramnios in this second instance, I think we can have no hesitation in referring it to the morbid condition of the fœtus with which it was associated. The fœtus presented a perfect specimen of the most complete form of anencephalous monster, for not only was the entire brain and vault of the cranium absent, and replaced simply by the red fleshy substance covered with serous membrane so common in such cases, but the ears were approximated to the shoulders in consequence of the shortening of the cervical vertebræ and the patency of the spinal canal. This canal was open in all its extent down to the lowest lumbar vertebra; there was no trace of spinal marrow, and the groove, which had an average breadth of about 1⅞ inch, was covered with a serous membrane, in which the spinal nerves could be seen running outwards to either side between the several vertebræ. In cases of hydrocephalus and hydrorachitis, we know that the same membrane which in this case lies exposed on the surface becomes the seat of such a morbid change as leads to the effusion from it of an abnormal quantity of fluid into the closed cavity of which it forms the lining membrane. When this morbid secretion goes on for some length of time it may cause the fœtal head to be enormously enlarged, or may lead to the formation in some part of the spinal column of a bulky cyst sufficient to present a very serious obstruction to the birth of the child. When such active secretion can go on in a cavity surrounded by unyielding walls, it is easy to see that if the disease had set in at an early period of intra-uterine life, these same cavities would more readily give way and burst; and so leave their lining membrane free to pour its excessive secretion into the more easily distensible cavity of the amniotic sac.

The possibility of such an occurrence has been indicated by Vrolik, who gives, in his work on monsters,[1] a description of an hydrencephalous fœtus, in which part of the contents of the skull have come to be protruded through a small opening in the upper aspect of it, so as to form a large cyst on the summit of the head, which appeared as if it were ready soon to give way and pour its fluid into the general amniotic cavity. The whole morbid process with its result has been fully traced out and illustrated by Virchow in his work on morbid growths.[2] But I have yet another preparation to show, which exhibits still more strikingly the relation that may subsist between disease of the cerebro-spinal system of the fœtus and dropsy of its amnion. It was born at the Lying-in Hospital here, under the following circumstances, as related to me by Mr Taylor the excellent house-surgeon:—

CASE III.—Mrs B. was brought into the Lying-in Hospital, after she had been suffering from the presence of labour pains, attended with severe vomiting, for four days. The patient was very weak and feverish, and the abdomen unusually large. The os was widely dilated and relaxed, and through it the fœtus could be easily felt; but the presenting part receded very readily into the roomy cavity of the uterus. The pains having ceased for some time, a catheter was introduced between the membrane and the uterine wall, by the advice of Dr Keiller, and when the pains returned, and the waters, which escaped in enormous quantities, were evacuated, the fœtus was found presenting by the breech. Delivery was completed in two hours thereafter. The child was born asphyxiated, but the attempt at its resuscitation was so far crowned with success that it began to respire, and lived for eight or ten hours. Its breathing, however, was never at all satisfactory. The mother recovered well, and was discharged, at her own request, on the ninth day after her delivery.

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[1] Tabulæ ad illustrandum Embryogenesim Hominis et Mammalium. Tabula xliv.

[2] Die Krankhaften Geschwulste. i. Bd. Neunte Vorlesung, p. 169.

The child, a female, measures 17¼ inches in length. The head measures 5¼ inches in its longitudinal or occipito-frontal diameter, and 4¼ inches in its transverse or biparietal. The spinal canal was incomplete in the lower lumbar and in the sacral vertebræ, which were cleft posteriorly; and over these bones the skin was supplanted by the thin, semi-transparent, serous-looking walls of the collapsed sac of a spina bifida, which measured an inch in its transverse, by an inch and a-half in its longitudinal direction, and presented at its upper extremity a small valvular opening, through which the fluid contents had found egress. It was easy to perceive the connexion of the spina bifida with the cavity of the skull; for on compressing the head the contained hydrocephalic fluid could be seen, first distending the walls of the sac, and then escaping by the opening at the upper angle. On the other hand, fluids injected into the cavity of the spinal canal ran along it, and distended the head; and on dissection it was found that the sac contained in its walls the nerves into which the lower end of the spinal cord was breaking up, and that the cavity was continuous with the central canal of the cord, which was dilated in all its extent, so as easily to admit the ordinary blow-pipe of a dissecting case, and was continuous at the upper extremity with the distended ventricles of the brain. It formed thus an uncommonly beautiful example of the hydrorrachis interna cystica, described by Förster[1] and others; but is chiefly interesting to us in regard to our present subject of observation, as showing how the quantity of fluid in the cavity of the amnios is liable to become augmented from the presence of dropsical affections of the fœtal cerebro-spinal system.

ARTICLE V.—*On the Physiology and Pathology of Certain Forms of Dilated Pupil.* By JOSEPH BELL, M.D., Assistant-Surgeon Surgical Clinical Wards, Royal Infirmary.

(*Read before the Medico-Chirurgical Society, March* 1, 1865.)

J. B., æt. 43, a strong healthy man, in July 1864, noticed that the pupil of the left eye was much larger than that of the other; and, on experimenting, found that he could not see so well with the dilated eye.

He applied at the Eye Infirmary. Could give no positive history of any blow or injury which could be credited with the case. He could read only the largest print on the card (about 15 of Jager's series), and distant vision was also defective. The solution of Calabar bean was applied, and in seventeen minutes he was able to read with ease the smallest print (No. 2, or pearl type). Improvement in vision was complete a minute or two before the contraction began to be noticed. Contraction went on to about the size of a pin's head. The drops were repeated two or three times on alternate days, and the cure seemed to be complete.

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[1] Die Missbildungen des Menschen, 1861. Taf. xvi. Fig. 6.

We saw no more of the patient till January 1865, when he returned—this time rather low in his mind, as he said he thought the other eye was giving way too, as he could not see very small objects with either. Left pupil again dilated; right apparently normal. On trying him with the weakest presbyopic glass, I found that it was only the change of focus common to his time of life which had alarmed him, so far as the right eye was concerned, and again applied the Calabar bean to the dilated pupil. He went off to get a pair of spectacles for reading with, and in much more cheerful frame of mind. He returned next day, telling me, that by the time he got along to the optician's, he found that the drop had made his left eye so perfect, that he thought he would let the spectacles alone for a year or two, but just return occasionally for his drop.

Three other cases were in every essential particular so exactly similar to this one as to require only a brief notice.

CASE 2.—T. T., æt. 27, August 1863, a remarkably powerful and intelligent engineer, had been working night and day at some machinery; noticed his right pupil dilated, and found that he could not see to read with it, or distinctly at all. The Calabar bean solution restored vision in fifteen minutes. He came occasionally to have it used, and when last seen, in November 1863, vision was perfect, though the pupil was still a little larger than the other.

CASE 3.—J. R., æt. 28, a healthy young woman, applied at the Eye Infirmary also in 1863. Dilatation of the right pupil, which was extreme, had been noticed by her friends for nearly six months. No cause could be assigned. The drop restored vision (which was very indistinct) in fifteen minutes. As she lived in the country, she was supplied with some of the solution, and in a few weeks the cure was permanent and complete.

CASE 4.—Mr J., æt. 25, of strong active frame, but who had tried his eyes much by study, applied to me in November 1864, with symptoms such as have been detailed in the other cases; but, in addition to the indistinctness of vision in the dilated eye, a certain amount of asthenopia in both. Under tonics, and the occasional use of the Calabar bean locally, he very soon was cured of his symptoms.

That four well-marked cases of this form of unilocular mydriasis, with loss of accommodation and such serious effects on vision, have come under notice during only eighteen months, shows that it is by no means an uncommon form of disease. So far as I can discover, however, no very distinct notice of it is to be found in any of the more accessible text-books on the eye; nor, so far as I am aware, could we have treated it satisfactorily till within the last year or two,—since the introduction of the Calabar bean into ophthalmic practice; which great improvement the profession and the public owe to two members of this Society, Drs Fraser and Argyll Robertson.

A brief notice of the pathology and treatment, as well as of the physiological bearing of this group of cases, will, I think not be out of place; and I will endeavour, in brevity at least, not to trespass on your patience.

The condition of the pupils, as to contraction and dilatation is at once so easily observed, can be so accurately measured, and is so free from all the complications which are the result of the inattention or inaccuracy of patients, that it has for long centuries been a favourite item in description and guide to diagnosis both for the physician and the surgeon.

So distinct are the characters of the excess of either condition, and so marked are their relations to visual abnormalities, that excessive contraction of the pupil received the special name, in nosology, of myosis. Dilatation was called mydriasis.

As pathology became more and more a science, and specially as our knowledge of the nervous supply of the iris, its relations to deeper structures, and its position in the mechanism of vision, became more extensive, myosis and mydriasis have been gradually losing their individuality as distinct diseases, and assuming a humbler, though truer and more generally useful position, as symptoms, and frequently very important ones, of other lesions, ocular, cerebral, or spinal.

Were I to endeavour even to mention all the different groups of disease in which mydriasis, dilatation of the pupil, is an important and interesting symptom, I would fill a small treatise.

Even the very literature of the subject in the past is tolerably extensive, not only in ophthalmology proper, but also in old surgical works. Ambrose Paré's definition is so excellent, and so far before his age, that I quote it verbatim: "Mydriasis is a dilatation of the pupil of the eye, either from an internal cause, the offspring of a humor flowing down from the brain, or from an external occasion, a blow, fall, or contusion upon the eye." Ludwig, Plenck, and others, have similar definitions and descriptions.

It would also be out of place here to go into any anatomical detail regarding the structure and innervation of the iris; suffice it to say, that it is now generally held that closure of the iris is effected by the circular fibres at the free margin, to which the name of sphincter pupillæ is given, and which are supplied by filaments of the third nerve or motor oculi; while the radiating fibres are those by which it is dilated, and are supplied by branches of the sympathetic.

Looking at these from the anatomical point of view, mydriasis or dilated pupil, whether we consider it as a disease, or a symptom merely, may result either—

1. From paralysis of the sphincter pupillæ,—M. paralytica.

2. From irritation or contraction of the radiating fibres,—M. spastica.

Under these two heads I will very briefly group the chief maladies in which mydriasis is present.

1. M. paralytica,—with partial or complete paralysis of third, very frequently accompanied by ptosis, and rarely by protrusion of the eyeball from want of muscular tension.

A.—Ordinary cases of ptosis, resulting from periostitic nodes, either syphilitic, or simply rheumatic.

I would specially refer to some cases recorded by Mr Workman, in vol. iv. part first of the Ophthalmic Hospital Reports.

B.—Cases of brain disease or injury accompanied by compression —as in certain cases of hydrocephalus. See Mackenzie under the head of "Hydrocephalic Amaurosis;"—and this also explains the dilated pupil, so well known as a symptom of traumatic compression of the brain from hæmorrhage.

C.—Result of intra-ocular pressure, in cases of glaucoma, with excessive hardness and tension.

D.—Cases of actual paralysis of the sphincter pupillæ itself, the result of excessive tension during the removal of the large hard lens in the operation of extraction.

E.—The well-known dilatation, the result of sudden and excessive retinal apoplexies. The very existence of such cases was unknown till the ophthalmoscope cleared them up, before which they had been called sudden cases of amaurosis, or perhaps had been put down to the credit of mydriasis simple, the dilatation being the only visible change.

Perhaps it is better to ascribe the dilated pupil, in these last cases, rather to the fact that the retina no longer receiving the stimulus of light, or at least being unable to transmit it to the brain, the third nerve does not receive any impulse from the brain, and therefore does not contract the pupil, than to the idea of the retinal apoplexy producing any intra-ocular pressure.

2. M. spastica,—from irritation of the fibres of the sympathetic which supply the radiating muscular striæ.

Such cases are much rarer, and not so well authenticated.

A.—Cases of dilated pupils coincident with the existence of worms, and other gastric derangement in children.[1]

B.—Soelberg Wells, in Ophthalmic Hospital Reports, ii. 304, refers to an observation of Graefe's, who had pointed out that mydriasis specially of one eye sometimes precedes mental alienation.

C.—We may perhaps include here some of those rare and curious cases, in which pain in, or extraction of, a tooth, has been followed by total and sudden blindness, with fully dilated pupil. One of the earliest recorded cases is to be found in Wirgman's translation of Heister's Observations, p. 193;—the date of the case is 1712.

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[1] Plenck (Vienna 1783) speaks of M. verminosa, "quae a saburra verminosa primarum viarum nascitur."

Now, the cases I have recorded here cannot well be classed under any of the preceding heads which I have so briefly sketched. They have all occurred in strong, otherwise healthy, patients, in whom there was *no* evidence of any cerebral change; no muscular paralysis; no ptosis, protrusion, or strabismus, very little or no intolerance of light; from the immediateness of the cure there could be nothing glaucomatous; and in all the cases one eye only was affected.

Like the scrivener's thumb, I believe it is a case of peripheral, not central paralysis. Two *certainly*, the other two cases probably, had been under special circumstances of overwork. It is not hard to find an explanation of the paralysis being confined to one eye only, when we remember how often a very slight difference in focal length or adjusting power causes the whole of the work at a given distance to be thrown on one eye only. This also explains the manner in which the Calabar bean, as a local excitant to the ciliary nerves supplying the sphincter pupillæ, causes a cure, in the same manner as atropia is now nearly universally acknowledged to act both locally and generally as an excitant of the vaso-motor system of nerves. On this cognate subject, I would refer to the papers published in the Edinburgh Medical Journal for 1857, by Mr Bell.

So far, then, good. Granted your local paralysis of the iris, the Calabar bean, your local excitant, sets *it* working again; that is surely enough. You get the pupil to contract, vision is restored; it is solely a paralysis of the iris, and of it only the circular fibres. If I were to stop here, I would stop too soon,—fail to illustrate a very interesting physiological point, and give an argument in favour of a theory which I firmly believe to be wrong.

For, could we say it was the iris alone that was paralyzed, we would have paralysis of the iris causing want of accommodation, removal of the paralysis restoring accommodation, a very important contribution indeed, were this the case, to the vexed question of the agents of accommodation. It stands, briefly, somewhat thus:—

1. Is accommodation of the eye to distance the result of a change in the *position* of the lens, or in its *shape?*

2. Is the change, whatever it be, the result of the action of the muscles of the eyeball, of the iris, of the ciliary body?

Without going into detail, I merely remind you, that Cramer stands up for the iris as the chief agent; and at first sight these cases of mine would seem to favour this view.

But, 1. Von Graefe has put on record a case of very great interest, in which the whole of the iris was removed. Yet, vision in that eye, a very few days after, was found remarkably good, and the accommodative power *almost* perfect. I would also refer you to a very interesting case recorded by Mr George Lawson (Oph. Hosp. Reports, iii. 272), of congenital absence of the iris in both eyes, in which accommodative power was fair, and very much better than in the mildest case of the form of mydriasis at present under your notice.

In short, we may almost go the length of the observation by Becker (translated by Windsor, and abstracted in the third part of vol. iv. of the Oph. Hosp. Reports), "The motions of the ciliary muscle and iris are associated and occur in co-operation; yet the iris has no direct influence over the alterations in the form of the lens; and,

2. The following point of great interest was noticed on every occasion of the use of the Calabar bean in the cases under review, that vision and accommodation were restored a few minutes *before contraction of the pupil commenced at all.* This was observed by Dr Argyll Robertson, in his experiments with Calabar bean on his own eye, that vision was affected and rendered myopic before the pupil was visibly altered. We are thus driven to look for something else by which to explain the symptoms and their removal, and we find it in the *ciliary muscle.*

Supplied by the third nerve, like the sphincter pupillæ, it sympathizes with the iris in its troubles, and also in its relief; and it has, what the iris has not, a very special function in the mechanism of accommodation. Though not so visible, the action of the Calabar bean solution, applied locally, is more rapid on the ciliary than on the iris, and by the removal of its paralysis the power of accommodation is most rapidly restored.

While on the subject of dilated pupil and its treatment by the Calabar bean, I may just notice other means that have been used for its relief. Opium dropped in, with very little success. Attempts to cause contraction of the pupil by irritation of the branches of the fifth, which are supplied to the eyebrow and nose. Admission of bright light into the affected eye is more apt to injure the retina than to cause contraction. Use of convex glasses for reading small print held near the eye. None of these in their results have proved either so rapid or so satisfactory as the local use of the Calabar bean.

There is an interesting but somewhat rare accident, hitherto irremediable except by mechanical means, which this same agent may probably render amenable to treatment. I refer to those cases in which, from blows on the eye, the inner free margin of the iris is ruptured, and *part* of the sphincter pupillæ at least is rendered useless, followed by dilatation of the pupil and disturbance of vision. This is not nearly so common an accident as detachment of the ciliary border, but still is frequent enough to deserve comment.

Experience has yet to show us how far the early and persevering use of the solution of Calabar bean may avail in restoring contraction, or, at least, in preventing excessive dilatation. It is true that such cases are frequently accompanied by, and complicated with, retinal detachment and extravasation, but the dilatation has been found to be very lasting and extremely annoying.

Mr White Cooper has recorded three interesting cases of this accident in the Association Journal for 1855. The only palliation hitherto discovered has been the use of an artificial diaphragm, consisting of a spectacle frame fitted with an opaque plate, and containing an aperture corresponding to the pupil, either circular or a mere transverse slit.

Experience alone will prove the value of the solution of the Calabar bean in such cases, but it is certainly well worth trying. The cases I have recorded in this paper have proved its power in the treatment of unilocular mydriasis.

ARTICLE VI.—*The Dietary of Scottish Agricultural Labourers.* By J. B. THOMSON, L.R.C.S.Edin., Resident Surgeon to the General Prison for Scotland, at Perth.

WE have lately been favoured by Dr Edward Smith with a considerable amount of information on the dietary of the Scottish agricultural labourer; but the extent of his inquiries was much more limited in regard to Scotland than the other great divisions of the United Kingdom. The number of families visited in Scotland was only twenty-nine, but these were no doubt selected with the view of representing a large body of farm-labourers. The inquiries were confined to the Western Highlands, including the counties of Inverness and Aberdeen, and Peebles and Roxburgh in the South. I am enabled to add to the sum of valuable facts (published by Dr Edward Smith, in the Sixth Report of the Medical Officer of the Privy Council) the dietary of the agricultural class in the county of Perth, and which agrees nearly with the results set forth in the blue book referred to.

The question of agricultural dietary was brought up a few weeks ago before the Sheriff-substitute of Perth, in the form of a complaint from the ploughmen of Williamston Farm as to the *quantity of milk the farmer was bound to supply to his ploughmen;* and the number of witnesses from different quarters was sufficient to establish the use and wont of the county. The evidence showed that the accustomed allowance to a farm-servant was,—oatmeal 17½ lbs. per week, and new milk a Scottish pint, or 12 gills, daily. The farm-servants pled for 16 gills of new milk, on the ground "that this quantity was essential for giving the required return in labour."[1] In the districts visited by Dr Edward Smith, he found oatmeal was given weekly to the extent of the long stone (17½ lbs.), and the yearly supply to married men varied from 65 long to 192 short stones (14 lbs.). Milk was usually given in quantities of one Scotch pint daily, but this varied with the means of supply.

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[1] The number of witnesses examined in this case on both sides would not be less than thirty. All agreed about the quantity of meal; but the ploughmen stood out for 16 gills of milk. The decision of the learned Sheriff was in favour of the Scotch pint of 12 gills.

Here, then, we have a large body of facts establishing what is the allowance of farm-labourers over a number of our Scottish counties. The absence of the potato has led to a greater consumption of oatmeal, especially in the Western Highlands; and the surplus of the potato cannot now be had for the feeding of a pig. In some districts, there is given voluntarily in good years a boll or two of potatoes; and in the hill districts a share of braxy (the lambs and sheep found dead), which is a useful addition to the dietetic allowances.

It is very plain from the above inquiries, that our farm-labourers have a simple, but most sufficient and substantial, we should say a redundant dietary supply; at least quite adequate to support hard out-door labour; and, to the surprise of Englishmen especially, it now transpires that our Scottish agricultural labourers, in quantity and in quality, are the best fed of the same classes in the United Kingdom. We give the nutritive value, according to the analysis of Dr Lyon Playfair:—

Oatmeal, 17½ lbs. per week, gives 40 oz. daily, which contains—

Nitrogenous principles, . .	6·4 oz.	
Carboniferous, ...	27·2	
Mineral, ...	1·2	
Total solid nutriment,	——	34·8

Add to this, new milk 1 pint, 12 gills, daily = 50 oz.

Nitrogenous, ...	2·00	
Carboniferous, ...	4·05	
Mineral, ...	0·35	
	——	6·4
Total daily nutriment in oz.,		41·2

There can be little doubt that the whole of the oatmeal is not consumed; but it came out during the investigation in Perth, and was pretty clear also to Dr Smith, that a considerable surplus was sold in exchange for other articles,—probably luxuries of far less importance to dietary. One of the most staggering conclusions reached by this dietary inquiry is, that, with higher wages, and of course increased means of obtaining good food, the farm-labourers of England have a less nutritious dietary than the same classes in other parts of the kingdom. A comparison of the results gives the following amounts of weekly nutriment for the average agricultural man:—

In England, . .	42·267 grs.
Wales, . . .	50·385
Scotland, . .	51·328
Ireland, . .	45·800

Or to arrange these four divisions of the United Kingdom in the order of their pre-eminence for good dietary, Scotland stands first, then Wales, Ireland, and lowest in the scale England. As to the money values of these national dietaries, we find the weekly cost was—

In Wales,	3s.	5½d.
Scotland,	3	3¾
England,	2	11⅞
Ireland,	1	9¾

Ireland being found the cheapest, yet having a better diet in Indian corn, or potatoes and buttermilk, for 1s. 9¾d., than the Englishman for 2s. 11⅞d.

Our national dietary of oatmeal and milk is certainly not to be excelled by any articles of diet in their nutritive qualities, and we hope will not, in the course of civilisation, be superseded by any of the English forms of dietary. The oatmeal is admitted to be the most important of all the cereals, and its flesh-formers, heat-givers, and mineral constituents, are well adjusted for the hardworking man. But in our milk supply we have a vast superiority to England, 98 per cent. in Scotland having a full supply of this most complete of all articles of food, which ought to be the substratum and model of all dietaries. Oatmeal or Indian meal ought to be freely introduced into England; and in spite of prejudice we have seen porridge and milk extensively tried on large bodies of Englishmen, and persevered in with uniform success.

Referring to the custom in Scotland of paying farm-servants partly in kind it does appear to work well, because the articles supplied, milk and meal, are essential and necessary articles of food; and thereby the farmer is assured that his servants receive adequate supplies so as to be enabled to endure severe out-door labour.

We cannot help noticing here that the Scottish agricultural labourer thrives well, and looks like his meat. Physically his industrial vigour and his endurance are quite equal to any. Tried by the dynamometer, Professor Forbes came to the conclusion that the average strength of the full-grown Scot was one-twentieth part greater than that of the full-grown Englishman; and while a good deal has been allowed for the influence of climate, soil, race, in the formation of national character, there can be little doubt that dietaries lie at the foundation of the people's moral and material greatness.

Note. — Our national dietaries for the agricultural labourers, estimated weekly, are:—

For *Scotland.*—Oatmeal 17½ lbs., and new milk 7 Scotch pints.
For *Ireland*, in winter.—Indian meal 14 lbs. and 21 pints skimmed milk.
〃 〃 in summer.—Potatoes 73½ lbs., and 14 pints buttermilk.
For *England.*—Bread stuffs, 12½ lbs.; meat, 16 oz.; sugar, 7½ oz.; milk, 32 oz.; fats, 5½ oz.; tea, ½ oz.; with beer and cider.

ARTICLE VII.—*Case of Lithotomy in the Forfar Infirmary.* By WILLIAM KETCHEN, M.D., L.R.C.S., Surgeon to the Forfar Infirmary; late Resident Physician, Edinburgh Royal Infirmary.

IN November 1863, I was consulted by R. E., æt. 60, an agricultural labourer, of a healthy though not robust appearance, on account of urinary symptoms which he had laboured under since the summer of 1861. In May of that year he had been severely exposed to wet and cold, and was laid up for some time in consequence. Previous to this he never had any urinary affection, but he then began to have pain at the point of the penis during and after passing water. It was to the exposure to cold he attributed the disease. About Martinmas 1862, he consulted a medical man, who examined him for stone in the bladder, but did not discover one. He generally suffered very much during the day when at work, but got comparatively good rest when in bed at night. I examined his bladder, and the instrument at once came in contact with a stone apparently of pretty large size. The urine was examined, and found to contain blood and pus cells. I recommended that he should come into the Infirmary and have the stone removed by operation; but the patient could not make up his mind to this at once, and he wished time to think over it. About six months after he first consulted me, he had a severe attack of smallpox, from which he made a good recovery, as far as that disease was concerned, but it left the stone symptoms very much aggravated, as now he suffered both night and day, not being able to lie in bed for more than ten minutes at a time from pain and the constant desire to pass water. About the end of November 1864, he requested me to operate upon him, and accordingly he was admitted to the Infirmary. On examining him after admission, the stone was found to be of considerable size, and to have a very rough surface. His heart and lungs were healthy. The urine was of acid reaction, and found to contain crystals of uric acid. He was kept about a week in the house before the operation, and during that time he took a mixture of bicarbonate of potash, and inf. uvæ ursi, but without any apparent good effect.

On 8th December, with the assistance of Dr Maclagan, who held the staff, and of Drs Smith, Alexander, Steele, and Murray, Forfar, I performed the usual lateral operation. On reaching the bladder by the ordinary incision, and introducing the forceps, the stone, which was distinctly felt by the sound previous to beginning the operation, could not be discovered. I withdrew the forceps and introduced the scoop, but with like want of success. The bladder was then injected by the urethra, the water issuing by the wound, and I again sought for the stone, but could not discover it. I then, with one hand, pressed firmly down from above the pubes externally, and introducing the scoop internally upwards behind the pubes, felt the surface of the stone apparently grasped in a pouch of the bladder at its fundus, and it could only be reached when forcible pressure downwards was exerted above the pubes. On two assistants pressing there, I was able to dislodge it with the scoop sufficiently to allow me to seize it with the forceps, and it was then extracted without difficulty.

The tube was then introduced and the patient put to bed.

The patient continued perfectly free from fever after the operation, the pulse for the first fortnight never rising above 76, and he otherwise being in his usual health. The tube was removed on the third day. The following is the note made on 21st December, the thirteenth day after the operation :—

The patient has continued quite free from fever, the pulse having averaged from 60 to 70, the tongue clean, and appetite good. No urine had come by the urethra until yesterday, when I introduced a catheter and drew off from 6 to 8 oz. This morning the urine came spontaneously by the urethra, and since then I have drawn off about a pint with the catheter. The wound is contracting, and to-day almost no urine has passed by it.

After this he was seized with orchitis on the left side, for which leeches and acetate of lead and opium fomentations were applied. His pulse rose to 88, and there was considerable thirst and anorexia. The inflammation soon subsided with the formation of a superficial abscess.

The following is the note made on 17th January :—

The patient for the last fortnight or three weeks has been passing water by the urethra only, and for the last ten days the wound has been quite healed up. He has suffered considerably from the orchitis, which resulted in an abscess and was opened on 3d January. Other two small abscesses formed afterwards, which were also opened. These incisions are now all but closed, and the patient is going about quite well.

23*d January*.—Patient dismissed.

Remarks.—The principal subject worthy of remark in this case was the difficulty in finding the stone. Had it not been that I was absolutely certain of the presence of a stone in the bladder, I might have begun to doubt the accuracy of my diagnosis. I was perfectly sure also that I had not missed the bladder in the operation, as I felt distinctly with my finger the prostate and narrow neck. The fact of the stone being surounded by the coats of the bladder, with only a small portion of its surface exposed, might lead to the idea that it was encysted, but this was not the case, as, previous to the operation, I felt it with the sound in a different part part of the bladder, and was able to turn it over on its side by introducing the sound below it; moreover, in getting hold of it with the forceps, I had no difficulty at all in dislodging it. Curved forceps would have been of great service in this case, for had it not been for the strong pressure made above the pubes, I should have been unable to reach the stone with the straight forceps which I had.

The stone weighs about two oz.: it consists of uric acid. The shape is a compressed oval, with a number of tubercles on the surface, which must have aggravated the patient's sufferings very much. It measures 3 inches by $2\frac{3}{4}$ inches.

ARTICLE VIII.—*On the Comparative Frequency of Twin-Bearing in Different Pregnancies.* By J. MATTHEWS DUNCAN, M.D., F.R.S.E., F.R.C.P.E., Lecturer on Midwifery, etc. etc.

IN a paper which was read to the Medico-Chirurgical Society, and which appeared in the number of this Journal for March last, I showed that the frequency of twin-bearing increased with the mother's age. This result was at first sight observed on inspecting the statistics of a large number of pregnancies of women of different ages. The great source of possible error in arriving at the conclusion regarding the influence of age, lay in the probable influence of the number of the pregnancy. Chiari Braun and Spæth, and Hugenberger, had shown that multiparæ more frequently bear twins than primiparæ; and it might be said that the preponderance of primiparæ among the younger, and of multiparæ among the elder women, was the real cause of the apparent increase of twin-bearing as mothers grew older. This source of error was eliminated from the statistics by confining them to women of different ages in the same pregnancy, or to women within two years of marriage. And I am convinced that the law of age is paramount.

In like manner, the statistics adduced in the paper referred to showed an increased frequency of twin-bearing as the number of the pregnancy increased. Here there was a source of necessary error in the circumstance, that as the number of the pregnancy advanced, so the average age of the mothers increased, and the already established law of age came into play, and might account for the increased frequency of twins as the number of the pregnancy increased. When I published that paper I had no means of eliminating from the statistics this source of error. I, therefore, refused the title of "law" to a general statement which might come to be a mere corollary of the law of age.

My object in this short paper is to present a table which is so constructed from the data at my command as to avoid error from the influence of age, women of the same age but of different pregnancies being compared. In it various adjacent pregnancies and ages are thrown together in order to attain considerable numbers, with a view to reaching a trustworthy conclusion, and these collocations diminish the value of the table.

Table showing the Comparative Frequency of Twins in Different Sets of Pregnancies of Wives of the Same Age.

Mother's Age.	25 to 29.			30 to 34.			35 to 39.		
	No. of Children.	No. of Twins.	One in	No. of Children.	No. of Twins.	One in	No. of Children.	No. of Twins.	One in
Pregnancies 2d, 3d, & 4th,....	3235	20	162	1628	19	86	568	9	63
Pregnancies 5th, 6th, & 7th,.	766	6	128	1568	27	58	993	17	58
Pregnancies 8th, 9th, & 10th,.	28	1	28	283	7	40	616	19	32

Yet an inspection of it will, I believe, convince the observer of the influence of the number of the pregnancy. And a comparison of this table with those previously published demonstrating the influence of age, leaves no doubt that the increase of twins with the number of the pregnancy is greater than could be accounted for by the error introduced by using quinquennial periods, that is, by the possible accumulation of the more advanced pregnancies in the last years of the quinquennial periods contrasted.

The increased frequency of twin-bearing as the number of the pregnancy increases may, therefore, be now regarded as a law of the production of twins.

Part Second.

REVIEWS.

Entozoa: an Introduction to Helminthology, with Reference more particularly to the Internal Parasites of Man. By T. Spencer Cobbold, M.D., F.R.S., Lecturer on Comparative Anatomy at the Middlesex Hospital. London: Groombridge: 1864.

This is the first original work in the English language professing to contain a complete account of the human entozoa, more particularly in connexion with those recent discoveries which have done so much to advance this department of medical science. The author is one peculiarly qualified for the task he has undertaken. Devoted for many years to the cultivation of comparative anatomy, possessing excellent powers of observation, and endowed with great artistic faculty of delineation, Dr Cobbold has in former communications showed the interest he took in the progress of Helminthology.

The result of his labours has been the present volume, which, whether we look to the value of its contents or the style in which it has been got up, must be considered as highly creditable to the medical literature of this country.

It is emphatically what our friends across the channel would call an *édition de luxe.* Printed on the finest paper and adorned with numerous and very beautiful illustrations, it is perhaps the handsomest work on a medical subject with which we are acquainted. We have but one fault to find with it, or rather, but one deficiency to regret, and that so far only as the medical reader is concerned; we mean the meagreness of the description of the symptomatology and treatment of the various entozoa. This, of course, is the result of the author's not being a practical physician; in every other respect, however, the work is very complete.

The work is divided into two parts, which treat respectively of systematic and special Helminthology.

The first of these gives a general account of the habits, structure, development, affinities, distribution, and classification of the entozoa, under the groups of Turbellaria, Trematoda, Nematoda, Acanthocephala, and Cestoda. The second part is devoted to a consideration of the special Entozoa which infest man; and as this is the portion of most interest to medical readers, it is to it that we shall exclusively direct attention.

The following is a complete list, according to Dr Cobbold, of the entozoa which infest man:—

1. *Fasciola hepatica*, Linneus.
2. *Distoma lanceolatum*, Mehlis.
3. *Distoma ophthalmobium*, Diesing.
4. *Distoma crassum*, Busk.
5. *Distoma heterophyes*, Siebold.
6. *Bilharzia hæmatobia*, Cobbold.
7. *Testrastoma renale*, Delle Chiaje.
8. *Hexathyridium pinguicola*, Treutler.
9. *Hexathyridium venarum*, Treutler.
10. *Tænia solium*, Linneus.
11. *Tænia mediocanellata*, Küchenmeister.
12. *Tænia acanthotrias*, Weinland.
13. *Tænia flavopuncta*, Weinland.
14. *Tænia nana*, Von Siebold.
15. *Tænia elliptica*, Batsch.
16. *Tænia marginata*, Batsch.
17. *Tænia echinococcus*, Von Siebold.
18. *Bothriocephalus latus*, Bremser.
19. *Bothriocephalus cordatus*, Leuckart.
20. *Ascaris lumbricoides*, Linneus.
21. *Ascaris mystax*, Rudolphi.
22. *Trichocephalus dispar*, Rudolphi.
23. *Filaria lentis*, Diesing.
24. *Filaria trachealis*, Cobbold.
25. *Trichina spiralis*, Owen.
26. *Strongylus bronchialis*, Cobbold.
27. *Eustrongylus gigas*, Diesing.
28. *Sclerostoma duodenale*, Cobbold.
29. *Oxyuris vermicularis*, Bremser.
30. *Dracunculus medinensis*, Cobbold.
31. *Dracunculus Loa*, Cobbold.

This seems, and no doubt is, a sufficiently formidable list; but it must be borne in mind that at least one-half, though scientifically interesting, are but of very slight practical importance. The first nine in the above list belong to the group of *Trematoda*, or flukes, which, though common enough in some of the lower animals, are rarely met with in man. The *Fasciola hepatica*, formerly known as the *Distoma hepaticum*, is the cause of the rot in sheep, and annually causes a very large mortality among them. The only one of the trematodes which occurs frequently in man is the *Bilharzia hæma-*

tobia, which is very prevalent on the borders of the Nile, and also, it would appear, in South Africa and the Mauritius. This small but formidable parasite is found in the portal veins, the veins of the mesentery, bladder, and other parts. The principal symptoms are derangements of the uropoietic functions, hæmaturia, diarrhœa, colicky pains, the so-called Egyptian chlorosis, and great prostration of the vital powers. The exact mode in which it obtains entrance to the body is unknown.

The next group, the *Cestoda*, or tape-worms, is perhaps the most interesting of the entozoa. They are interesting not only from their pathological importance, but from the very remarkable facts which have recently been discovered as to their development. Only two genera, the Tænia and the Bothriocephalus, are known to occur in man. According to Dr Cobbold, eight species of the former and two of the latter are human parasites.

The following is a brief summary of what is known as to the development of the tape-worm, and of the different phases through which it must pass. The mature tape-worm, or *strobila*, resides in the intestinal canal. It is composed of separate joints called *proglottides* or *zooids*. The anterior segment forms the head, and on its integrity the existence of the whole animal depends. The joints of the neck and anterior part of the body are sexually immature. The posterior proglottides contain ripe ova, which, while still within the body of the parent, are converted into six-hooked embryos, boring larvæ, or *proscolices*. In order that the farther development of these embryos may take place, it is necessary for them to find their way into the body of another animal. The remaining changes we give in the words of Dr Cobbold :—

"At this time the proglottis is about to undergo a passive migration, and having detached itself from the strobila, it is soon expelled from the bowel of the host, thus finding its way into some cesspool, or, it may be, into the open fields. The proglottides move about for a time, but the growth of the multitude of embryos within their interior causes the proglottis sooner or later to burst, and the embryos thus become dispersed; some are thus conveyed down drains and sewers, others are lodged by the roadsides, in ditches and waste places, whilst great quantities are scattered far and wide by winds or insects in every conceivable direction. Each embryo is furnished with a special boring apparatus, having at its anterior end three pairs of hooks; the entire group of embryos of any single proglottis is consequently called the 'six-hooked brood.' After a while, by accident as it were, a pig coming in the way of these embryos or of the proglottides, is liable to swallow them along with matters taken in as food. The embryos, immediately on their being transferred to the digestive canal of the pig, escape the eggshells, and bore their way through the living tissues of the animal, and having lodged themselves in the fatty parts of the flesh, they there rest to wait their further transformations or destiny. The animal thus infested becomes measled, its flesh constituting the so-called measly pork. In this situation the embryos drop their hooks or boring apparatus, and become transformed into the *Cysticercus cellulosæ.* A portion of this measled meat being eaten by ourselves, either in a raw or imperfectly cooked condition, transfers the cysticercus to our own alimentary canal, in which locality the cysticercus attaches itself to the wall of the human intestine, and, having secured a good anchorage, begins to grow at the lower or caudal extremity, producing numerous joints or buds to form the strobila or tape-worm colony."

The following table expresses the various changes which take place in the case of the *Tænia solium :*—

ZOOLOGICAL INDIVIDUAL.	*a.* Egg in all stages.	= protozooid.
	b. Six-hooked embryo = proscolex.	
	c. Resting larva or *Cysticercus* (*telæ*) *cellulosæ* (scolex).	
	d. Immature tapeworm.	
	e. Strobila, or sexually-mature *Tænia solium.*	
	f. Proglottis (curcurbitinus) = free segment = deuterozooid.	

The mature tape-worm, in the human intestine, is a source rather of annoyance than of danger. The great risk to life is connected with the presence in the internal organs of the scolices, the immature or cystic entozoa. The *Cysticercus cellulosæ*, the scolex of the *Tænia solium*, fortunately of comparatively rare occurrence in man, is met with most commonly in the subcutaneous and intermuscular tissue; next to that in the brain and eye; and, lastly, in the substance of the heart and other solid viscera. The danger to life, of course, depends upon the locality of the parasite, and a certain number of cases have been recorded, where its presence in the brain has been the cause of death. The cysticercus is very common in the pig, giving rise, as already stated, to measly pork, and is introduced into the human subject by eating that meat in a raw or imperfectly cooked state, when it becomes developed into tape-worm.

Tænia solium has been generally thought to be the only tape-worm indigenous in this country; but, according to Dr Cobbold, the *T. mediocanellata*, a hookless, flat-headed tape-worm, is nearly equally frequent. The cysticerci of this animal are met with in veal and beef.

The only other tænia of practical importance is the *Tænia echinococcus*, and it differs from the others in this, that the mature animal is never met with in the human intestine, while the larvæ are of frequent occurrence in the internal organs. The complete tape-worm infects only the dog and wolf; it is very small, seldom exceeding the fourth of an inch in length, but its larvæ are the well-known echinococci or hydatids. The mode in which the development of this parasite takes place we give in the words of Dr Cobbold:—

"In the main, no doubt, the developmental features of this species can be made to harmonize with those already set forth as regards the other cestodes, but the variations of detail and complexities arising out of unusually peculiar appearances render it no easy task to trace out all the phenomena as they successively come to light. Without further comment, I proceed to remark that the eggs, supposed to be about five thousand in number in a full-grown joint, develop in their interior the usual six-hooked embryo. These embryos are introduced into our bodies (and into those of other parasite bearers) by our partaking of water or food, into which the eggs have been carried by accident or otherwise. After arriving within the apparently selected host, the behaviour of the embryos accords, at least in the first instance, with that of other tape-worm proscolices. With an especial liking for the liver, they bore their way into this organ, or are carried along the circulating current into other organs. In these situations they sooner or later become transformed into simple vesicular, bladder-like bodies, commonly called acephalocysts or hydatids."

The inhabitants of Iceland have for many years suffered greatly from hydatid disease, due, there can be no doubt to the great multitude of dogs met with in that island.

The *Bothriocephalus latus*, a broad tape-worm, appears to be confined to Europe, and is met with especially in the western cantons of Switzerland, the adjacent French districts, Russia, Sweden, Poland, Eastern Prussia, Holland, and Belgium. There is no doubt that this animal is introduced into the body and undergoes development in the same manner as has been already described, but there are still links wanting to complete the chain of evidence. Dr Cobbold inclines to the opinion that for the "host" we should look to certain species of fish, and, especially as suggested by Leuckart, to the salmon and trout family; and the introduction of the parasite is to be referred to eating imperfectly cooked fish, in which the bothriocephalus larvæ have taken up their temporary abode.

Before leaving the tape-worm we quote the following passage which indicates other modes by which the embryos of tape-worms may be introduced into our bodies:—

> "There are other dangers, however, in store for us, even supposing we avoid the tape-worm chances above mentioned. These have reference to the harbouring of the six-hooked embryos, which, when they have gained access to our own bodies, will, as I have previously shown, develop into measles. Our flesh, like pork, thus becomes measled, although certainly not to the spawn-like extent so often seen in the lower animals. A single measle alone, unfortunately, is sufficient to prove fatal, and this ugly contingency, moreover, is one which we can never be absolutely certain of avoiding. We become the 'host' or bearer of the measle by swallowing the fully-developed eggs of the *Tænia solium*. This we may do directly by handling fresh tape-worms, whose almost invisible eggs, by concealment under our nails or in our clothing, may subsequently drop upon or may be brushed into our food, and when swallowed, develop within us accordingly. Even a thorough washing of the hands will not render us absolutely secure from invasion—I mean those of us, especially, who devote ourselves to the experimental investigation of helminthological science. In like manner, our neighbours who complacently devour choice salads made from the stores of the market-gardener, also run a certain amount

of risk, not only as regards this entozoon, but, indeed, as respects several others. The appropriate vegetables may be manured with night-soil containing myriads of tape-worm eggs, or they may be watered with fluid filth, and other abomination into which these eggs have been cast by variously possible ways. In such cases, one or more tape-worm ova may be transferred to our digestive organs, unless the vegetables are carefully cleaned before they appear on the table. In the same way, one perceives how fallen fruits, all sorts of edible plants, as well as pond, canal, and even river water procured from the neighbourhood of human habitations, are liable to harbour the embryos capable of gaining an entrance to our bodies. It thus becomes evident also how one individual suffering from tapeworm may infect a whole neighbourhood, rendering the swine measly; these animals, in their turn, spreading the disease far and wide. Such a person may even prove dangerous to the life of his friends and neighbours, and especially also to members of the same household. A case of this kind has come to my knowledge, and it may be explained as follows:—One or more free proglottides or joints with their eggs had escaped from the bowel, either at night voluntarily on their part, or during the day passively, in some such way, at all events, as secured the liberation of scores, possibly thousands, of ova. One of these escaped eggs had somehow or other come in contact with human food, or possibly had been blown into the mouth, or even, perhaps, had been carried on the legs of some fly into the milk or sugar on the table; in some such manner, at least, the egg found its way to the stomach, and from thence it got into the circulation, ultimately making its passage to the brain, where its presence gave rise to convulsions and death. I offer this as a probable explanation in the case of a young woman whose sister suffered from tape-worm, but I need not say that out of the sixty or seventy deaths recorded from the ascertained presence of this 'worm in the brain' it is more than likely that a similar train of reasoning would afford a true solution of the so-called mystery as to how these entozoa become our brain-guests."

The remaining parasites belong to the order *Nematoda*, or round worms. Our information regarding the development of most of them is still comparatively incomplete, although there can be no reason to doubt that points at present obscure will gradually be cleared up. Want of space prevents us from referring to any but the *Trichina spiralis.* Extraordinary interest has lately been attached to this little parasite. Viewed for many years as a mere curiosity, it has recently been shown to be the cause of a most fatal disease, and the most dangerous probably of all the human parasites. This parasite is introduced into man by eating raw or imperfectly cooked pork, and on the second day after introduction attains full sexual maturity. The eggs of the Trichina are developed into minute embryos, which soon commence their wanderings. They penetrate the walls of the intestine, and pass directly into the muscles of the individual. During this process, grave, sometimes fatal, effects are produced. These are specially, enteritis, peritonitis, fever, pain, great weakness, and œdema of the limbs. Several outbreaks of the trichina disease have been observed in Germany, and though from the habits of the people they are peculiarly exposed to it, there is little reason to doubt that deaths have occurred from it in this country.

In taking leave of Dr Cobbold's work we have only to repeat our strong commendation of it, and to recommend it heartily as containing a very complete exposition of the present condition of Helminthology.

A Treatise on Military Surgery and Hygiene. By FRANK HASTINGS HAMILTON, M.D., Professor of Military Surgery in Bellevue Medical College and Long Island College Hospital, etc. Pp. 640. 127 Engravings. Baillière Brothers, New York: 1865.

THE name of Dr Hastings Hamilton is well and favourably known by the profession here, as well as in America, as the author of a most exhaustive treatise on Fractures and Dislocations. The unwearied diligence, the accuracy and conscientiousness which characterize that work, along with the high-toned courtesy which evidently prompts all Dr Hamilton's dealings with his professional brethren, point him out as one specially suited for the production of a work of the class to which the book under review belongs; for teachers of Military Surgery, and specially of Military Surgery in a state of transition, as it is at present in America, can never be complete if founded on the personal experience (however extensive) of only one man, but demand the combined and collated observations of very many men on very various subjects.

When, at the beginning of the war, the Northern States had to raise rapidly enormous armies, they had also to find surgeons for their regiments; and for their instruction some manual of military surgery was needed, and, thoroughly to satisfy the American mind, such a manual must be indigenous—home-made. Among others, Dr Hamilton published a short treatise, the first edition of which was soon exhausted, and then, as he tells us in his preface, pressing engagements in the public service prevented a revision of its pages, and hence its republication was delayed. The revisal being at length undertaken, it was found that the added experience of four years of war rendered it necessary that it should be completely re-written and very much enlarged: hence the present work.

The author's aim is rather to be practically useful to the young surgeon in field or hospital than to add by elaborate statistics to the general sum of professional knowledge; to describe the *best*, *quickest*, and *simplest* plans of treatment rather than to display erudition and induce confusion by elaborate accounts of *all* devices; to magnify his office, and record the feats and the opinions of his professional brethren-in-arms, rather than establish a reputation for himself.

The fairest and most useful method of reviewing a work of such value and importance is to give a resumé of its contents, with a brief notice of its more curious and exceptional details.

The Introduction treats of the position which military medical officers hold in the army, shows the absurdity of the "non-combatant" theory, and describes briefly the various means for the transport of the wounded to the rear in different nations and campaigns.

Chapter ii. is devoted to the "Examination of Recruits." We read of one man who got his discharge on account of wearing a glass eye, which, however, had succeeded in deceiving the surgeon on his enlistment; and of another poor fellow whose feet were so large that he had been walking on bare feet for eight months, because the quartermaster could not furnish him with shoes; his number was 13, and the largest size issued was 11. Now that substitutes are worth 1000 dollars each, it is probable that the examination of recruits is not quite so strict as formerly.

Chapter iii., containing less than forty pages, on "The General Hygiene of Troops," must necessarily be very brief. The points chiefly insisted on are (1.) the absolute necessity of the occasional use of vegetables to prevent scurvy; (2.) the bad effects of the whisky ration; (3.) the defects of the commissary system, and the peculations attached to it; (4.) the necessity of camp and personal cleanness; (5.) a few words on the dress of troops.

Chapter iv. is a most interesting account of the arrangements adopted for the bivouac of troops, and for their more permanent shelter in tents, huts, and barracks. Our Crimean experience, and the various campaigns in Utah, Mexico, and the present war, furnish ample variety of climate and condition by which to estimate the value of the different abodes.

In chapter v. an account is given of the hospital arrangements, with plans and drawings of some of the larger army general hospitals. The one at Chestnut Hill, near Philadelphia, is supposed to be the largest institution of this kind in the world, containing 2820 beds for patients, and 500 for officers and attendants. The result of experience goes to prove that all hospitals, and specially military ones, ought to be one-story buildings, elevated three or four feet from the ground; and if possible they should consist of numerous separate buildings or pavilions arranged round a central corridor, which should be circular or of a flattened ellipsoidal form. The pavilions must not enclose the central space as a wall does, but touch it with one end only, projecting from it like the radii of a wheel. This construction has very great advantages as ensuring free ventilation, and the various pavilions can all be connected by a railway to each other, and to the administrative buildings, operating-room, and chapel, which may occupy the middle of the central free space. The simplest means of ventilation are found the best,—doors, windows, and an escape for air along the entire length of the ridge of the roof.

Chapters vi. and vii. describe the preparations for the field, and the management of troops on the march.

Chapter viii. is a very brief but thoroughly readable and most interesting account of the different plans for the conveyance of sick and wounded soldiers, from the simplest possible litters, made of two poles and a blanket, up to complicated and expensive ambulance waggons; and, lastly, to the most beautiful and apparently valuable hospital railway cars. Each car contains from 20 to 30 beds suspended on gutta-percha loops, a stove, a dispensary, and a kitchen. Chiefly by these, 75,000 sick and wounded soldiers had been sent north in the first three years of the war, with the effect of greatly relieving the camp hospitals.

Chapter ix., on gunshot wounds, discusses their peculiarities, the characteristics of the effects of the round and conical balls respectively, the question of the frequency of death on the field from hæmorrhage, and the various means of arresting hæmorrhage and dressing wounds. Preference is given to water dressings, either tepid or cold, over any other medicated application, to gunshot wounds. This introduces the subject of gunshot injuries of the several regions; and first, in chapter x., we have gunshot injuries of the head. A series of curious cases are given which illustrate the well-known fact that a man may not be much the worse for the loss of a considerable portion of brain substance, and with proper care may survive even the lodgement of a ball within the calvarium. The non-interference treatment is very properly approved.

In chapter xi. gunshot wounds of the face are briefly discussed, and the great reparative power of its vascular structures, even in comminuted fracture, receives full justice.

Chapter xii., gunshot wounds of the thorax are noted for their danger, both primarily from hæmorrhage and inflammation; and, secondarily, as causing chronic affections of the lungs and pleuræ. As regards their treatment we have here an interesting account of Dr Howard's plan of hermetically sealing all wounds of the chest at once, by paring their edges, stitching with silver sutures, and then closing them with collodion and charpie.

Without criticising Dr Howard's view, the results of which have not yet been fully given to the profession, Dr Hamilton makes an excellent common-sense division between wounds of the chest, which *he* thinks suitable for this plan, and others in which it would be inapplicable.

In punctured and incised wounds of the chest (chapter xiii.), Howard's plans would seem to be more generally useful. With reference to the subject of wounds of the chest, Dr Hamilton records cases in which officers went into battle wearing breastplates, and shirts of chain-armour, recalling the Middle Ages. They proved totally ineffective to resist bullets, except when spent or impinging at an angle. Fortunately such *cases* were not common.

Chapters xiv., xv., and xvi., on gunshot and punctured wounds of the abdomen and pelvis, are good and full, but present no special features of novelty. On the subject of gunshot fractures (chapter xvii.) Dr Hamilton is at home. Such fractures of the thigh have been very numerous, and in two-thirds of the cases attempts have been made to save the limb, with what success is not yet fully known, but in Dr Hamilton's opinion in too indiscriminate a manner. He gives some practical rules to guide the surgeon in his choice of treatment, characterized by his usual common sense.

In the very numerous and complicated apparatus figured and described, we find rather too much prominence given to counter-extending appliances.

Chapters xviii. and xix., on amputations and *exsections* (sic), give brief accounts of the methods of operating in almost every part of a limb and at every joint. In military surgery you must cut your coat according to your cloth, that is, make your flaps where you can best get skin, but still there are certain cases in which amputation by the long anterior flap is quite possible, and, if so, ought certainly to be performed. The omission of any complete notice of this method can be easily remedied in a new edition.

In chapter xx., on arrow wounds, we fancy ourselves at once transported backwards in time to the days of Robin Hood, Crecy, and Poictiers; from Whitworths and Armstrongs to bows and arrows, ay, and to poisoned arrows. The breastplates and coats of mail we read of a little ago would not be so much out of date here.

Bows and arrows are in Indian warfare no child's play, for experienced officers regard them as being in the red man's hands more dangerous than rifles, at distances under sixty yards. They can transfix the trunk or limbs, penetrate bones, and on any attempts to withdraw them, the barbed head separates from the shaft and sticks. The wounds they cause are lacerated. An expert bowman can discharge six arrows in a minute, hence the same man may probably be wounded in several places by the same adversary.

In three soldiers wounded by the Navajoes, Dr Bill counted forty-two wounds, or more than a baker's dozen in each. Arrow wounds penetrating the abdomen are almost invariably fatal from peritonitis. Dr Bill records one in which the liver was wounded in several places; yet the man recovered, after passing bile and sugar in his urine for many days. Wounds from poisoned arrows are rare, though not unknown.

Chapters xxi. to xxiii., treat of traumatic, hospital, and dry gangrene. The paper on hospital gangrene is written by Frank H. Hamilton, junior, and is carefully drawn up; it is chiefly interesting to us, from a statistical comparison of the effects of a local use of a solution of bromine and of nitric acid in arresting the disease. The comparison is greatly in favour of the bromine (strength 1 drachm of pure bromine to 8 ounces of water). The average duration of the disease, after the application, being 6½ days in the cases treated with the bromine, and 16 in the cases treated by nitric acid.

Chapter xxiv., on tetanus, adds little to our power of treating that most terrible complication. With regard to the use of chloroform, which has been tried very largely by the army surgeons, Dr Hamilton thinks it has "not met with a success which in our judgment would warrant its continuance, or, to say the least, its substitution for opium.

Scurvy is briefly described in chapter xxv.

In chapter xxvi. anæsthetics are discussed; and we cannot agree with Dr Hamilton either in his preference for ether over chloroform, or in his opinion that the success in capital operations is less after the use of chloroform and ether than it used to be when we were without these means.

After this full analysis we can now do no more than recommend all surgeons, and specially all army-surgeons, to study this important work, one of the very few good results we have yet seen or heard of which have occurred from the American war. Reading the few selected cases, and then considering some of the general statistics (20,930 wounds reported at the Medical Bureau at Washington), we are enabled in some degree to realize the magnitude of the struggle, and the fearful amount of human suffering it has occasioned. As medical men we read with pride and pleasure the accounts of the bravery and humanity of the medical officers, Northern and Southern alike, emulating each other in their attention to Rebel or Yankee. Dr Hamilton's own bravery and devotion are barely hinted at, but not even his modesty can prevent us from recognising touches of military as well as of professional enthusiasm.

The book is excellently got up, and the illustrations, though some of them are old friends and rude enough, are numerous and intelligible.

The Significance of Dropsy as a Symptom in Renal, Cardiac, and Pulmonary Diseases. The Croonian Lectures for 1864. By W. R. BASHAM, M.D., Senior Physician to the Westminster Hospital. London: Churchill: 1864.

DR BASHAM is already well known to the profession as the author of a good work on dropsy, connected with disease of the kidney.[1] In the present series of lectures, delivered before the Royal College of Physicians of England, he takes a wider field, and treats of dropsy not only as due to renal but to cardiac and pulmonary diseases. The two first lectures treat of renal dropsy, and the following quotation, which forms the conclusion of the second of them, expresses Dr Basham's opinion regarding the nature of this form of dropsy and the principles on which its treatment should be based:—

"I will venture, then, to conclude that renal dropsy—dropsy with albuminous urine—is significant not only of various forms and degrees of kidney degeneration, but of a decadence and consequent failure in function of almost all the other tissues of the body. It would appear to be significant of a wide and all but universal defect in the formative or cell-forming agency.

"If we adopt the physiological view, that healthy cell development depends on the selection of highly nitrogenized material for the nucleus, as well as for the contents of the cell, that cell representing the highest degree of vital activity and energy in which the nitrogenous nucleus is the most concentrated; then viewing the development of less healthy cells, the power of selecting or condensing nitrogenous matter grows weaker and weaker, till in the lowest forms the cell contains but an albuminous fluid with hydrocarbon granules, oftentimes fatty, perhaps amyloid, and consequently totally inefficient for all functional purposes of secretion.

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[1] Second Edition: Churchill: 1862.

"From this point of view I will venture to submit that our methods of treatment should more decidedly embrace the principle of nutrition and support; that our main object should be to invigorate the powers of the organism; that we should call to our aid whatever is calculated to maintain and promote the force of cellular reproduction, so that, by these means, the decaying powers may, if possible, be revived.

"Guided by such principles, many cases, which at first afford but faint prospect of relief, pass from a stage of imminent danger to one of hope and encouragement; and eventually either permanently recover, or at least give assurance that a form of disease which was but lately thought incurable may be brought within the category of those which are remediable and tractable."

Dr Basham's opinion, it will be seen from the above extract, is that renal dropsy is not merely a result of the local disease, but that it is an indication of a widely spread lesion, consisting in a degeneration of many other tissues. As he says in another place: "May we not now appreciate the significance of that form of dropsy which, associated with albuminous urine as one of its earliest and most easily recognised conditions, pervades all the tissues, infiltrates every cell, interferes with and stagnates every function, and oftentimes becomes the immediate cause of death, by literally drowning the individual in his own fluids."

And again, "Even in the acute form of Morbus Brightii cases which have run their fatal course in a few weeks the epithelia cells of other parts present a granular and imperfect appearance. The epithelium of the mouth, throat, and alimentary passages is granular, and sometimes even fatty, the pavement epithelium of the bladder often most prominently so. The epithelial cells of the bronchial mucous membrane are cloudy and granular, and accompanied by evidence of cellular deterioration identical in character to what we witness in the renal tubes.

"Even in the most acute cases structural changes in the heart-fibre are present.

"The small arteries which lead to the epithelial layers are everywhere diseased; and not only in the most advanced forms of renal degeneration, such as that known as the amyloid degeneration, but in the other various forms of renal dropsy, the small arteries of the whole digestive tract, from the buccal cavity to the anus, are similarly affected.[1]

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[1] Virchow, p. 378.

"The inevitable result of this wide-spread decay is, that the parts and cells which are the seat of it become totally unfit and incapable of ministering to the functions either of nutrition or excretion. The gland-cells can no longer perform the function of secretion, and the vessels can no longer supply nutrition to the tissues from which the epithelial gland-cell is formed."

With this view we are by no means prepared to agree. In some cases, no doubt, renal disease may be merely an indication of a general morbid state, but in by far the greater number of cases the disease of the kidney is in the first instance confined to that organ, and the change in other parts is secondary to it or rather to the change effected in the blood. Were Dr Basham's view correct, every case of acute Bright's disease would necessarily have a fatal termination, either immediately or at no remote period. For, supposing the patient to recover from the first effects of the attack, the structural changes in the heart and in the small arteries would necessarily lead to such an embarrassment of the nutritive functions as would not be compatible with much duration of life. Dr Basham's reference to Virchow is scarcely fair. From the text, the reader would suppose that Virchow shared Dr Basham's views, and considered that in every case of renal dropsy there was extensive arterial degeneration. The following, however, is the passage in the *Cellular Pathology*, referred to, and our readers will be at no loss to see that such a construction of it is, to say the least, a very forced one:—"We find, on the one hand, well-marked conditions of cachexia, and on the other, with extreme frequency, dropsy, with the whole complex group of changes which are usually included in the idea we form of Bright's disease. In nearly every instance in which the amyloid affection reaches an advanced stage, the patients are in a state of great marasmus. There are cases where the whole extent of the digestive track, from the buccal cavity to the anus does not contain a single minute artery which is not affected with this disease, and where, in every part of the œsophagus, stomach, small and large intestines, the small arteries of its mucous membrane are found changed in this way." It is evident that Virchow does not here allude to acute Bright's disease, or even to chronic Bright's disease in general, but to advanced cases of that form of it which depend upon amyloid degeneration.

Dr Basham, in the commencement of the second lecture, considers the manner in which the tube-casts and albumen are formed. With regard to the first of these, he is of opinion that, with the exception of blood-casts, they are derived from the metamorphosis or breaking up of the epithelial cells of the renal tubes; and he makes some interesting observations regarding the analogy of renal tube-casts with the sputa in various forms of pulmonary disease. With this opinion we to a certain extent agree, although we believe that, at least in the more acute cases, there is a fibrinous or fibrinoid material poured out, which, in coagulating, entangles the epithelial cells. In reference to the source of the albumen in the urine in cases of Bright's disease, Dr Basham objects to the usual explanation, that it is the result of a percolation of serum from the Malpighian capillaries due to obstructed circulation, on two grounds,—"1*st*, It is too mechanical. 2*dly*, So direct a drain would, it might be supposed, have a corresponding influence in lessening the watery or serous character of the blood in Bright's disease. Yet the opposite is notorious; for the greater the albuminous drain through the kidneys the more watery and serous does the blood become."

The second objection we are absolutely at a loss to understand. How a drain of albumen could render the blood less watery we must leave to Dr Basham to explain. The general opinion, and we believe the correct one, has long been, that the drain of albumen is the main cause of the watery condition of the blood, and the watery condition so produced is a main cause of the dropsy in chronic cases. Dr Basham's opinion is, that the albumen in Bright's disease is derived from the breaking up of the abortive elements of the uriniferous tubes. We do not deny that there is some foundation for this opinion; still, we believe that the greater part of the albumen is derived directly from the blood, in consequence of the congestion of the Malpighian tufts, which results from the obstruction to the renal capillary circulation.

Dr Basham's views of the proper treatment of Bright's disease are founded on his ideas of its pathology. Looking upon it as "a process significant of deterioration and decay," he considers it our duty "to build up and restore, rather than to take from and reduce." Accordingly, he recommends rest, warmth, nutritive stimuli, and blood-forming remedies, especially iron, as the proper treatment. He does not forbid the use of purgatives and diaphoretics, but is strongly opposed to bloodletting, of which he speaks as follows:—"With regard to bloodletting, I unhesitatingly assert that it is injurious; it is manifestly hostile to the fundamental principles on which the treatment of these forms of disease should be based." From this opinion we totally dissent. We consider acute Bright's disease as in the first instance essentially a local lesion, and we know of no remedy so effectual, when there is blood in the urine and other signs of extreme renal congestion, as cupping over the loins. The time for restoratives and tonics comes later, when the acute state is passed, and there are indications of anæmia.

Dr Basham's third lecture is devoted to dropsy resulting from cardiac and pulmonary disease.

In reference to disease of the heart, the author divides these cases into two classes: those which are not and those which are followed by dropsy. Under the first class he includes those cases where the deposit in the valves undergoes calcification; under the second those where the deposit undergoes fatty degeneration. Though we think Dr Basham generalizes too much in making this statement, we on the whole agree with his remarks on dropsy as connected with cardiac disease. We have seen dropsy in connexion with calcification of the valves, and, like all general pathological statements, Dr Basham's rule is liable to some exceptions. We, however, quite agree with him, that so long as the individual and the heart remain well nourished, valve disease may give rise to but little suffering. If there be an obstruction at an orifice, the heart will labour to overcome it, and a compensating hypertrophy will take place. But if the nutrition of the individual and of the organ fail, the heart will be too feeble to overcome the obstruction, dilatation will occur, and will be followed by those sufferings which invariably accompany it. The principle of treatment laid down by Dr Basham is undoubtedly correct "to maintain the nutrition of the body in the state of the highest efficiency."

We must now take leave of Dr Basham, and though we by no means always agree with them, we can recommend his lectures to our readers as worthy of their perusal.

Curvatures of the Spine: Their Causes, Symptoms, Pathology, and Treatment. By BERNARD E. BRODHURST, F.R.C.S. London: Churchill: 1864. Pp. 93.

CURVATURE of the spine ought, by this time, to be thoroughly understood, both by the profession and the public, did the amount of instruction received bear anything like a proportionate ratio to the amount of writing on the subject, or rather to the number of books published. We fear, however, this is not the case, and question very much if the book, the name of which heads these sentences, has added much to our knowledge. Mr Brodhurst calls it "Curvatures of the Spine;" but we trust that it is not intended as an account of all curvatures of the spine, else the pathology is bad, and the treatment worse, but that it is meant to *include* those distortions which are the result of undue weakness of ligaments and muscles, with a yielding condition of the bones, often connected with rickets, but with no actual disease, caries, or loss of substance of the column, and to *exclude* true caries of the vertebræ, which leads to curvature, generally angular, in cases where anchylosis and cure occur.

The subject is introduced by a short chapter on the "Normal or physiological antero-posterior spinal curves," founded chiefly on the observations of the Webers.

The body of the work is divided into three chapters or sections, each of which treats of a special direction of the curvature in space, *i.e.*, lordosis or anterior curvature; cyphosis or posterior, and scoliosis or lateral curvature. Under each of these heads, the causes, pathology, symptoms, and treatment are discussed.

It will be at once seen that such an arrangement is both unscientific, as it is mechanical almost accidental, and inconvenient, as the causes, pathology, and treatment must so slightly differ under the different sections as to involve almost necessarily repetition.

The third chapter, on scoliosis or lateral curvature, is of course by far the longest. Mr Brodhurst divides the progress of the disease into two stages, and his division is one of the most curious instances of the application of what we may call the *chronological method* to surgical nosology we have ever seen. We quote *verbatim* and *in extenso*:—

> "This affection may be divided into two stages or periods, namely, first, incipient curvature; and, secondly, confirmed lateral curvature. It is important that these terms should be rightly understood, and I would therefore define them thus,—incipient curvature is that condition which is removable in the horizontal position, by slight lateral pressure, while a confirmed curve requires long-continued mechanical means to effect the same object."

At page 61 an addition is made to this, by saying that *incipient* is without rotation of the vertebræ, *confirmed*, with rotation. But notwithstanding this, the chapter on the causes of scoliosis is excellent and practical, and we would specially notice with approval the following sentence:—

> "It is probably well known that the muscles of the back of those who wear stays are not red and firm, but that they are for the most part pale and weak, and even that they are atrophic and indistinct. Few will doubt that the constriction to which the figure is thus subjected conduces, through insufficient muscular development, to the formation of curvatures of the spine."

Excellent pathology, doubtless to be followed by no less excellent treatment, but alas for human expectations! At page 90 we find a picture of a "spinal apparatus," consisting of steel plates, bands, levers, and cogged wheels; and on page 91 we are told of a spinal couch, which, "acting with powerful machinery instead of a slight cog-and-pinion motion only, is so powerful to effect the purpose in view that it will remove distortion in cases where the portable instrument is utterly useless."

The constitutional treatment is excellent, the injunctions as to rest and the use and abuse of exercise are practical and sensible; but so long as we read of steel stays and couches for forcibly removing distortion, we must continue to denounce such treatment as barbarous, unscientific, and founded on an entirely mistaken idea both of the disease and its remedy.

One very excellent remark is made on a point too apt to be forgotten, that the spinous processes are not always to be relied on as evidences of the integrity of the bodies, for a very considerable amount of curvature of the bodies may have occurred without the apices of the spinous processes being altered in their relation.—P. 70.

The drawings, which are numerous, are as beautiful and delicate as Mr Baggs' drawings always are, and the publishers part is as usual admirably done.

Part Third.

PERISCOPE.

MATERIA MEDICA AND DIETETICS.

ON DIGITALIS. BY DR WILLIAM MURRAY.

FEW drugs have been the subject of more conflicting statements than digitalis, and yet there are none on whose action the profession has more implicitly relied. The drug has a special action on three separate organs—the brain, the kidneys, and the heart; in proof of this, witness its decided action in delirium tremens, in dropsy, and in various forms of heart disease.

As the action of the medicine upon this latter organ seems to have been little understood, and is still scarcely established, although the observations of Dr Handfield Jones and others have been well put forth, it therefore seems right to add any new facts or observations on the subject.

I have carefully observed from twenty to thirty cases of cardiac disease treated by digitalis, from which the following conclusions have been drawn; some of these will be quoted as very decided cases showing the true action of the medicine. As the proposed explanation of the action of digitalis accords fully with the ingenious and well-supported theory put forth by Dr Anstie in his work on "Stimulants and Narcotics," it may be well to say that he holds that all true stimulants tend to bring tissue action to its normal standard; that when such action is in excess a true stimulant will reduce it, and when below par will bring it to its proper level; he proposes "that the word stimulant be restricted to agents which, by their direct action, tend to rectify some deficient or too redundant natural action or tendency." I believe that the action of digitalis when properly investigated will confirm these views; and at the same time the investigation of its action will be materially assisted by bearing them in mind. I shall show—

1*st*, That digitalis will stimulate and strengthen a weak heart, and that the weaker the muscular tissue of the heart the safer will be the administration of the medicine.

2*d*, That in a hypertrophied heart it will fail to reduce the pulse either in frequency or strength, and in such cases will prove dangerous.

3*d*, That in a weak organ, acting because of its weakness with great rapidity, it will reduce the number of its contractions, and, as it were, strengthen or tone them down. To strengthen and quicken the action of a weak slowly acting heart, and to reduce the number of the rapid strokes of a feeble heart is, according to Anstie, to do the work of a true stimulant, bringing action up to the normal standard on the one hand, and reducing it to that level on the other.

I. The following case is one of many in which the remedy has removed irregularity and intermission of the pulse, has given power to the heart when exceedingly weak, and increased the rapidity of its action:—

A lady, aged 50, had suffered for nine years from cardiac disease, and was in an extremity of suffering when I first saw her, having a very largely dilated and weak heart whose cavities were overladen with blood, and considerable disease of both aortic and mitral valves. A confused tumbling and heaving impulse was all that could be felt in the cardiac region, and feeble blowing murmurs supplanted the natural sounds of the organ. There was no pulse at the wrist, and the large vessels in the limbs had scarcely a pulsation in them.

A dropsy of the legs and belly was making fast progress, and the usual stimulants and diuretics had failed to give relief. Although thus pulseless and faint, with the heart in so weak and burthened a condition, I prescribed digitalis in doses of ten drops and upwards. In a day or two the organ began to beat more forcibly, the pulse came again at the wrist, the turbulent or confused action became replaced by a quiet steady stroke, the breathing, before laboured, became easy, and the dropsy began to disappear. In a week the patient was out of bed, and very shortly she so far recovered as to go about in her usual way. After this she lived for more than a year in comparative comfort and good health, when suddenly the organ again failed, and before digitalis could be administered she died.

I might add to this cases of aortic regurgitation with dropsy; many cases in which both aortic and mitral orifices were diseased with dilatation of the walls of the heart and a weak irregular pulse at the wrist; cases of simple weakness of the organ without appreciable disease; cases of dyspnœa from an emphysematous state of the lungs and sluggish pulmonary circulation—all of which were most decidedly benefited by digitalis. I would especially direct the attention of the profession to the use of the drug in cases of bronchitis depending on sluggish circulation with weakness of the heart's action and emphysema of the lung.

I have alternated the use of digitalis with the strongest of our stimulants in some of these cases, and have been astonished at the superiority it possesses as a stimulant. From the observation of a few cases in which the drug has been given continuously for a lengthened period, I do not find anything like failure of the heart's action to occur under a month or six weeks, and then a few days of intermission will clear the way for a fresh course of the medicine. In dyspnœa the relief obtained is sometimes most marked. I remember a poor girl with frightful disease of the mitral orifice, having to rest on almost every doorstep on her way to my house, who was so relieved by digitalis as to walk to and fro with ease. I do not think, after trying it in almost all the affections of the heart, that there is a single disease or condition of that organ *attended by weakness* in which it may not be given with safety and benefit.

II. The next condition to which I would refer is that of hypertrophy. In this my experience is not large; but in one uncomplicated case of hypertrophy of the left ventricle without valvular disease, in a man who had an almost herculean frame, digitalis was fairly and carefully tried. The heart's action in this case was rapid, and at the same time very forcible; the digitalis not only failed to reduce this, but it caused an increase in the symptoms, and the patient almost fainted after trying it for one week, so that the medicine had to be changed to tinc. veratr. virid., from which decided benefit was derived. I might say that a converse experiment was tried in a boy with mitral disease and rapid action of the heart. Here tinct. verat. virid. failed to give the relief which was most effectually derived from digitalis.

III. Lastly, as a cardiac stimulant, digitalis ought to reduce too great rapidity of action *when this depends on weakness*. In this respect the drug has been well tried, and almost universal testimony to its value has been given.

In inflammatory diseases, in fevers, in phthisis, it is acknowledged to reduce the rapid pulse. In one case of phthisis at present under my care, it does this, and most materially lowers the temperature of the body and relieves the daily hectic paroxysm. While, however, it checks the excessive rapidity of the heart's action, it does not, therefore, weaken its force; so that, as a true stimulant, it brings the organ down to a slower rate of action, but at the same time affords it more power to do its work.

Thus it appears that the *physiological* action of digitalis is that of a stimulant, in Dr Anstie's sense of that term; and that in its *therapeutic* properties it is especially useful in cardiac weakness, whether that weakness be accompanied by extremely slow or extremely rapid action. Further, its physiological action as a stimulant may be explained by supposing that in the case of the slow heart it improves the molecular arrangement of the sarcous elements, or that it excites the nerve centres from which the nervous power of the heart is derived; and in the case of the weak but rapid heart, it acts by strengthening that regulating or restraining (vital) influence which, while maintaining the activity of tissues at a normal rate, checks undue and riotous action in the same (Radcliffe). Lastly, let me say, that as a diuretic it is at once the safest and best we possess, and the dose may vary from ten drops to half an ounce. This very day I have given nearly half an ounce in fifteen-drop doses every two hours to a child three years old, and by so doing have subdued a rapidly-developing general dropsy, which was threatening the little sufferer's life.

P.S.—In some cases of mitral regurgitation, accompanied by a loud distinct murmur, I have felt satisfied that digitalis reduced the intensity of the murmur most decidedly, and in one case it seemed to disappear altogether.—*Medical Times and Gazette.*

ON SOME NEW FACTS RELATIVE TO THE THERAPEUTICAL APPLICATIONS OF THE ALKALOIDS OF OPIUM.

THE recent researches of M. Claude Bernard have at length thrown light on that chaos of contradictions with which the therapeutical history of the alkaloids of opium was filled. We are now able, by isolating the soporific, the exciting, or the convulsing action, to refer to such or such of the alkaloids of opium one or several of these properties. Take, for instance, narceine; it is the most soporific of the derivatives from opium. In equal doses narceine puts to sleep animals more soundly than codeia would do. "What particularly characterizes narceine is profound calm and absence of excitability to noise." What is also found with morphia, and especially with codeia, animals put to sleep by narceine return very quickly to their natural state. Narceine is, then, of all the products derived from opium that which is best adapted to produce *physiological sleep*. Trials have been made upon this point, not only with adults, but with children. A very remarkable instance of this was reported in a recent number of the *Bulletin de Thérapeutique*. The case in question was that of a little girl, ten years old, affected with phthisis. For a long time she had had very little sleep; the cough was incessant, and followed by vomiting. Various soporifics had been used without advantage, when narceine was ordered in the dose of the seventh of a grain daily (a tablespoonful of a syrup containing, in sixteen ounces of vehicle, four grains of narceine). In less than a week the child had no more of the talkative sub-delirium which had wearied her and deprived her of sleep; she slept almost the whole night; the attacks of cough were less violent. What we would here particularly allude to is not only the good effects produced in this case by narceine, but the absence of the nausea and vomiting observed by M. Behier, in the trials of this medicine which he made upon adults. These trials should be repeated, especially with regard to the soporific rather than to the sedative action, upon which we should count less.

Since M. Behier has popularized in France hypodermic injections, this method has entered into almost daily use, and there are few physicians who have not derived good results from it. This method, simple and easy, free from all danger, is destined, by its very precision, to take precedence, sooner or later, of the other modes of administering energetic alkaloids. It is, then, interesting to bring together all the facts which have a bearing on the practice of injections. Atropine exerts upon pain only a *local action*. On the contrary, hydrochlorate of morphia acts principally by absorption, which renders it very efficacious in deep-seated or inaccessible pains. We have not, as in the case of atropine, to dread accidents; tolerance is *almost* complete. We seldom see more than a little dryness of the mouth, temporal constriction, sweating, rarely vomiting. The facts recorded in the recent thesis of Dr Piedvache show that morphia *in injection* has not the inconvenience of rendering the system less susceptible of its effects. It is not necessary, in order to produce a determined sedative effect, to gradually increase the dose. But a remark of great importance is, that the sleep produced by morphia is, as pointed out by Claude Bernard, characterized by great excitability. The slightest noise should therefore be guarded against; the least excitation, by effecting a sudden wakening, would counteract the action of the remedy.—*Gazette Médicale de Paris*, March 11.

ON EXTRACT OF MEAT. BY PROF. VON LIEBIG.

SINCE my investigations upon meat in the year 1847 (see these "Annals," lxii.), I have constantly endeavoured to promote the fabrication of extract of meat, according to the method described by me, in countries where beef is cheaper than it is with us.

Since the introduction of this extract of meat (which is not to be confounded with the so-called consommé or soup-tablets) into the Bavarian Pharmacopœia, its great efficacy in cases of disturbed nutrition, of impaired digestion, and of debility, has been practically confirmed, and it will perhaps suffice to give an idea of the extensive use made of the extract of meat as a remedy, to mention that in the court-pharmacy here nearly 5000 pounds weight of beef are annually employed for this object. It may be worthy of remark that a great part of the extract of meat in the Bavarian pharmacies is sold by retail—that is, without a physician's prescription—an indubitable proof that it has become a domestic remedy, to which those persons who have experienced its beneficial effects in the form of medicine resort spontaneously in similar derangements of health; these are often very poor people, who are least inclined to spend money upon medicine, and whom the high price of this remedy (1 fl. 12 kr. per oz.) does not deter.

In the hospitals and infirmaries, in which it is well known that the good soup prepared there is only too often confiscated by the nurses and attendants, the prescribing physician will, by means of the extract of meat, be enabled to give his patients a soup, quite free from fat, and of any strength he may desire.

Parmentier and Proust have for many years most strongly recommended the extract of meat for use in the French army. "In the train of a regiment," says Parmentier, "the extract of meat supplies the severely wounded soldier with a remedy which, with a little wine, immediately restores his strength, enfeebled by great loss of blood, and places him in a position to bear being transported to the nearest camp-hospital."

"No more happy application," says Proust, "can be imagined. What more strengthening remedy, what more powerful panacea, than a dose of the genuine extract of meat dissolved in a glass of generous wine! The elaborate tit-bits of gastronomy are all for the spoiled children of wealth! Should we, then, have nothing in our field-hospitals for the unfortunate whom his destiny condemns to suffer for us a long death-struggle in the snow and mire of marshes?"

A pound of extract of meat is sufficient, when boiled with slices of bread, potatoes, and some salt, to afford a meat soup for 128 soldiers, such as is not to be had of equal strength in the best hotels. Coffee and tea, although valuable in themselves, are in the end to be considered as only imperfect substitutes for the extract of meat. In fortresses and in the navy, where the men are thrown upon salted and smoked meat, the extract is the only means of supplying the important constituents which are removed from the meat in the process of salting, and of restoring to it the complete nutritive power of fresh meat; in like manner the employment of the extract of meat would be of the highest importance to travellers, and particularly to families in the country as well as in towns, particularly in Germany, where soups are indispensable; in Germany meat would be eaten in very much greater plenty and better dressed, and the soup would be prepared from the extract, if the high price of the latter did not present a difficulty which with us is almost insuperable.

The introduction into Europe of the extract of meat at half or a third of its present price from countries where meat is of scarcely any value, would be a true blessing to the European population. I had, in Podolia, Buenos Ayres, and Australia, with great earnestness directed attention to the fabrication of extract of meat, and was ever ready to make persons who seemed inclined to the project, acquainted with the mode of proceeding, and to assist them with my advice; my efforts were for fifteen years unattended with success, until two years ago a better prospect presented itself of carrying out my wishes. In the spring of 1862, I had a visit from Herr Giebert of Hamburgh, an engineer, who, engaged about roads and other works, has spent many years in South America, and among other places in Uruguay, where hundreds of thousands of sheep and oxen are killed solely on account of their skins and fat; he told me how painful it was to him, thinking of Europe, to witness the waste of the flesh of these animals, of which only a very small portion was salted, while the rest was mostly thrown into the rivers, and that he had constantly felt the most ardent wish to utilise the same. Having subsequently seen my chemical letters, in which the extract of meat is described, he travelled to Munich, and determined, if he could learn the mode of its manufacture, to return to South America, to found an institution for producing it. The probability that the stone must once again roll in vain, did not restrain me from entering earnestly into Herr Giebert's views and making him acquainted with everything connected with the preparation of the extract; for practically learning the process he had come to the best place, as scarcely anywhere else would he have such an opportunity as the court-pharmacy here affords, where the extract is prepared weekly. I introduced Herr Giebert to the manager, my friend Professor Dr Pettenkofer, who most willingly gave him access to the laboratory, and made him acquainted with the minutest details of the process. Herr Giebert was in earnest with his design; he returned in the summer of 1863 to Uruguay, but it was nearly a year before he could, with the apparatus made in Berlin, overcome the many difficulties which attended the arrangement and introduction of a new branch of industry, and commence the manufacture. I have scarcely ever felt greater joy than that afforded me by a letter received from him a month ago, in which he informed me that the first produce of his manufacture of extract of meat had been sent off to Europe.

Herr Giebert had expressed a wish to me, that he might designate his extract of meat by my name, as it was prepared after my method. I agreed to this, but at the same time remarked, that if his product contained the slightest trace of fat (whereby it acquires a rancid condition), or if gelatinous matter was predominant as in the ordinary soup tablets or consommé (whereby it becomes inclined to mould and loses the unalterability at high temperatures and in damp air[1] which characterizes the genuine extract), I would be the first publicly to notify the uselessness of the same. On the other hand, Dr Pettenkofer and I promised him, if he would send his entire produce of extract of meat to Munich, that we would, without fee or reward, subject each of his remittances to analysis, and in case they satisfied the requirements of science, that we would warrant their genuineness, on the condition that he would bring the pound of extract of meat into commerce at a third of its present price in Europe and no higher. To prop up a mere money speculation we would not lend our names. This proposal was, of course, to apply only to the first period of the introduction of the extract into Europe, as it may be supposed that when the public were once acquainted with the characters of the genuine extract of meat, they would no longer require the certificate of the chemist to enable them to form their own opinion.

The first sample of extract, of about eighty pounds of extract of beef, and thirty of that of mutton, arrived in Munich some days ago, and we have the great satisfaction to be able to say that its quality is excellent, as was to be expected from the flesh of half-wild sheep and oxen. We hope that the other condition, of which we may signify our approval—namely, the price (a third of the present price in Europe), may likewise meet our expectations.

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[1] A number of facts testify to the unchangeability of the extract of meat under the most unfavourable circumstances in damp unoccupied cellars, and in moist warm air; if the product is pure, it is not at all inclined to mould, and I have specimens before me from the court-pharmacy, and from Principal Friedel (of the Sanitary Society), which were kept from eight to fifteen years, closed with a loose cork and paper, and on which not a trace of injurious change is perceptible.

The following is Professor Liebig's process for the preparation of the extract referred to at the beginning of this article:—

"From the above-described behaviour, the best process for preparing, in the short space of a few minutes, the strongest and most aromatic soup, is at once evident, and any one can, by the simplest experiments, satisfy himself of the truth of Proust's statement, that the constituents of the soup, on which its taste and its other properties depend, exist ready formed in the meat, and are by no means the product of cooking.

"If one pound of lean beef, free from fat and bone, finely chopped as when used in sausages, be equally mixed with its own weight of cold water and slowly heated to the boiling point, the fluid being then, after boiling for some minutes, separated by expression through a napkin, from the coagulated albumen and from the fibrin which has become hard and horny, we obtain an equal weight of the most aromatic broth, such as cannot be obtained even by boiling for hours from a piece of meat. Mixed with some salt and the other adjuncts with which soup is seasoned, and coloured somewhat darker with roasted onions or burnt sugar, this broth furnishes the best soup which can be prepared from a pound of meat. The influence which the (brownish) colour of the soup exercises on the taste, in consequence of the ideas connected with the colour, is easily proved. Soup coloured with a little caramel (roasted sugar) has, in the opinion of all persons, a much stronger taste than the same soup when not coloured, although the caramel, in fact, in no way enhances the taste.

"If we allow the meat to boil longer with the water, or evaporate the broth at a boiling temperature, it spontaneously acquires on concentration a brownish colour and a delicate taste of roast meat. If we evaporate it to dryness in a water-bath, or, if possible, at a still lower temperature, we obtain a dark-brown soft mass, of which half an ounce is sufficient to convert a pound of water, to which a little salt is added, into a strong and palatable soup.

"This extract of meat is not to be classed with the so-called soup or broth tablets prepared in England and France, for the latter are not made of meat, but consist of gelatin in a state of greater or less purity, which is distinguished from bone-glue only by its high price.

"From thirty-two pounds of lean beef, free from bone and fat (consisting of eight pounds of dry meat and twenty-four pounds of water), we obtain one pound of this extract, which, on account of its high price, can scarcely become an object of trade; but if the experience of military surgeons agrees with that of Parmentier, according to which 'the dry extract of meat in the train of a corps, supplies the severely wounded soldier with a restorative, which, with a little wine, immediately raises his strength exhausted by great loss of blood, and enables him to bear being transported to the nearest hospital,'[1] it seems to me to be a pure matter of conscience to recommend Parmentier's and Proust's proposal to the attention of governments.

"Now, if the composition of this extract be somewhat more precisely known, any skilful apothecary can easily distinguish the genuine from the false preparation. Of the true extract of meat nearly 80 per cent. dissolve in spirit of wine 85 per cent., while rarely more than from 4 to 5 per cent. of the ordinary soup tablets are dissolved by the same fluid. The amount of creatin and creatinin, which latter is immediately recognised in the spirituous solution by means of chloride of zinc, and the nature of the salts remaining after incineration, consisting principally of soluble phosphates, furnish sufficient resting points for estimating the goodness of the genuine extract.

"I consider this extract to be not less important for provisioning ships and fortresses, to preserve the health of the men in those cases where there is a want of fresh meat and vegetables, and where the men are dependent on salt meat.

"It is generally known that in salting meat the latter is rubbed and sprinkled with common salt, and that in the points where the meat and salt come in contact, a brine is formed, amounting to a third or to half of the fluid which formed a constituent of the fresh meat.

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[1] See Proust, Annales de Chimie et Physique, 3d series, t. xviii. p. 177.

"I have found that this brine contains the principal constituents of a concentrated soup, that, therefore, in salting, the composition of the meat is altered, and in a still greater degree than occurs in cooking. In cooking, the highly nutritious albumen remains in a coagulated state in the meat, but in salting, albumen separates from the meat; from the brine when heated to the boiling point a quantity of albumen separates as a coagulum. The brine has an acid reaction; it gives with ammonia, on the addition of a salt of magnesia, copious precipitate of ammoniaco-magnesian phosphate; it contains lactic acid, a large quantity of potash; and that it contains creatin, which I was unable to separate from the great excess of common salt, may be undoubtedly inferred from the amount of creatinin present. Thus, the brine neutralized with lime yields, after the crystallization of the common salt, a ley, from which, after some time, on the addition of alcohol and then of chloride of zinc, the double salt of creatinin, which I have already frequently mentioned, is deposited.

"Hence, it is quite clear, that in salting meat, if the latter be rubbed so much that a brine is formed, a number of matters are withdrawn by the separation of the fluid of meat which are necessary to its constitution, and that in proportion to this loss its nutritive properties are impaired; if these constituents are not replaced from other sources, it is evident that a portion of the meat becomes a respiratory matter certainly not conductive to health. It is further certain that by salted meat, if its quantity be not increased, the health of an individual cannot be permanently maintained, inasmuch as by its constituents, the parts of the body removed in the metamorphosis of tissue are not fully replaced, and the fluid diffused through the whole body (fluid of meat) is not kept in its normal composition. A change in the nature of the gastric juice, and consequently in that of the products of digestion, must be looked upon as a result of the long-continued use of salt meat; and if, during digestion, the matters which are indispensable to its progress are taken from other parts of the body, the latter must lose their normal condition."—Translated from the *Annalen der Chemie und Pharmacie* for January 1865, by William Daniel Moore, M.D.Dub., in *Dublin Medical Press.*

MIDWIFERY.

CURE OF A CASE OF SPONTANEOUS RUPTURE OF THE UTERUS.

BY DR BAUMANN.

THE patient was a countrywoman, pregnant of her fourth child, who at the time of the doctor's arrival felt a sharp pain in the right side of the abdomen, which was a little tumefied. The accoucheur wishing to practise version with the left hand, the left shoulder presenting, had soon to give up the attempt in consequence of arriving at a longitudinal tear of the uterus which led into the abdominal cavity. The right hand introduced in its turn readily completed the version, speedily followed by the extraction of the child, which was of small size, and already dead. When the womb had been emptied of its contents the physician had to return into the abdominal cavity a considerable coil of small intestine which had passed into the uterus by an opening four inches long. The vagina was then filled with sponges as high as possible. The uterus contracted energetically and the patient felt comparatively little pain; the treatment consisted in cold fomentations, large doses of morphia, luke-warm enemata, and, later, fomentations of hot wine. For the first few days everything went on well. On the fifth day vomiting occurred, which ceased on the discontinuance of the morphia; on the sixth, after repeated enemata there was copious fecal evacuation, which sensibly diminished the meteorism which had hitherto existed. From this time everything went on well, and at the end of six weeks the woman was able to return to her ordinary employments.—*Würtem. Med. Corresp. Blatt. and Révue de Thérapeutique.*

TUMOUR OF THE KIDNEY LYING IN THE ABDOMEN IN FRONT OF THE INTESTINES, MISTAKEN FOR AN OVARIAN TUMOUR; OPERATION; DEATH; POST-MORTEM.

(Under the care of Dr HALL DAVIS and Mr GEORGE LAWSON.)

ELIZABETH P., a widow, aged forty-nine, the mother of three children, was admitted into Prudhoe ward on 27th December 1864, on account of a suspected tumour of the left ovary. Her family history is good. Her mother died at seventy-six years of age, and her father at fifty-three. The patient herself always had good health up to August 1860. Her story was, that about that time she had a fall, and shortly after she felt pain in the left side. She thinks it was then that she first noticed a small tumour, about the size of a walnut, in the left inguinal region; but whether this was really the abdominal tumour for which she was admitted into the hospital or the small hernia she now has in the left groin, it is difficult to say, as her account of her past sufferings was very indistinct. Menstruation ceased two years ago last September; but for the two years preceding that date it was very irregular. She has suffered from vaginal pruritis and vaginal leucorrhœa for the last two years. She has never passed any blood in her urine, nor observed any unusual appearance in it.

On admission, she looked pale and anxious. She said she often slept badly at night on account of abdominal pain. Has constant desire to micturate, and when walking cannot retain her urine. On looking at the abdomen, there was seen a bulging forwards below the umbilicus on the left of the middle line. In the left lumbar region anteriorly was felt a tumour apparently of the standard size of a fœtal head at full time. Its upper margin was one inch and a-half below and to the left of the umbilicus. There was dulness on percussion in the left lumbar region, whilst the umbilical and right lumbar regions were fully resonant. The tumour presented an irregular surface, was somewhat elastic on compression, but gave no sense of fluctuation to the taxis. It was very movable over the subjacent structures, and the abdominal parietes were freely movable over it. She had at times considerable pain in it, and seldom passed a whole day or night without suffering. Her urine was pale and limpid, specific gravity 1006, slightly acid, and contained no albumen. Her pulse was not accelerated; tongue clean; chest-sounds good. Dr Hall Davis examined the uterus with a uterine sound, and thus ascertained that it was small and freely movable, independently of the tumour, and in its natural position. She was ordered by Dr Davis a generous diet with two ounces of port wine daily, and a quinine mixture with sulphuric ether.

January 7th.—Has passed sixty ounces of urine in the twenty-four hours, of the specific gravity above indicated, and quite clear. She says she feels stronger and better than when she first came into the hospital.

13th.—Complains of much pain at the lower part of the abdomen, particularly towards the left iliac, and inguinal regions. The patient has, on account of her bad nights from frequent pains in the lumbar, iliac, and inguinal regions, and total inability to work for her livelihood, expressed a strong wish to have the tumour removed. Believing that the tumour was ovarian, and concluding that, from its great mobility and the absence of adhesions, its removal would be easy, an operation was promised; but its dangers were very fully explained to the patient, both by Dr Davis and Mr Lawson, and she was recommended to leave the hospital for a week to consider the subject well over with her friends. She was accordingly discharged on the 16th January.

24th.—The patient again presented herself for admission, and expressed a strong determination to have the tumour removed, urging that in her present state she was quite incapable of work, and was thus unable to maintain herself. It was therefore arranged that an operation should be performed.

26th.—An empty, well ventilated, and well warmed ward was prepared, and supplied with warm vapour. The patient having been placed under the influence of chloroform, a catheter was introduced, and the small quantity of water in the bladder drawn off. Mr Lawson then opened the abdomen by an incision, about four inches in length, below the umbilicus, when the tumour, with the pressure merely of the weight of the hand, immediately presented itself lying in front of the intestines, and looking very like a small ovarian tumour. It was evidently covered by a layer of peritoneum, which had not the appearance of omentum. Mr Lawson then introduced his two fingers, to explore round the tumour and ascertain the extent of its pedicle, if it had any; but he found that it had no pedicle, and that its peritoneal covering was continuous with the peritoneal lining of the abdominal walls. The tumour was not within the true abdominal cavity, but behind the peritoneum, which it had carried forwards, and now formed one of its coverings. A small incision was made through the peritoneal coat on the surface of the tumour to examine its nature, and a dark-coloured elastic mass presented itself. This he punctured with a fine exploratory trocar, but only a drop of blood escaped. It was now a question what was the nature of the tumour? It was now suggested that it might be a movable kidney, completely altered by disease; and as this supposition seemed to be the most probable, Mr Lawson determined to proceed no further, and at once closed the wound, having first, however, satisfied himself that the uterus and ovaries were *in situ* and normal. The edges of the wound were now brought together with harelip-pins, and the wire suture between them. One of Mr Salmon's obstetric bandages, lined with flannel, was placed round the abdomen, and the patient returned to bed. Ordered one grain of opium every four hours. The operation was performed at nine o'clock in the morning, and the report at six o'clock states that she had been quiet, and slept a little since the operation. She has no pain or tension of the belly; pulse 96.

27*th.*—She passed five ounces of urine during the night, and vomited once; has no pain or tenderness of the abdomen; pulse 108. During the morning she vomited several times, and in the afternoon the sickness increased in frequency. She was ordered soap-pill with opium, ten grains, as a suppository. After the suppository she slept for two hours; but on waking the vomiting again returned. Without quoting *verbatim* from the hospital notes, it is sufficient to say that the vomiting continued more or less until she expired. The abdomen became tense and tympanitic on the afternoon of the 28th, and she died on the evening of the 29th. From the time of the operation she suffered but little pain, and maintained consciousness to within two or three hours of her death.

The following is an extract from the report of the post-mortem examination by Dr Cayley:—

Abdomen: There was slight general peritonitis; a little soft yellow lymph was smeared over the coils of intestine and the abdominal walls. It was most abundant near the incision. On the left side of the abdomen was a large oval tumour, attached to and apparently forming part of the left kidney, from the outer and lower part of which it appeared to project. The kidney and tumour were enveloped on both sides by peritoneum, which formed a kind of mesentery for them, so that they were freely movable upwards to the diaphragm, downwards as far as the brim of the pelvis, and inwards to the mesial line. The left renal artery was much elongated, and ran obliquely downwards and outwards. The tumour had a fluctuating feel. The kidney was turned so that its hilus looked backwards and to the left, and its convex border forwards. On making a section through the tumour and kidney, the former was found to be of the consistence of very soft brain-matter, of a pinkish-white colour, with large venous tracks running through it. It was enveloped in a fibrous capsule continuous with that of the kidney, from the substance of which organ it was quite separated. The posterior surface of the kidney was hollowed out by the tumour, and its greatest mass lay in the hilus, which was much altered in shape; it formed a projection into the pelvis of the kidney, which was somewhat dilated. The ureter was normal. The supra-renal capsule was distant an inch and a half from the kidney. The descending colon and sigmoid flexure lay *behind* the tumour and kidney. The long diameter of the tumour was four inches and a-half, and the short diameter three inches and a-half; the circumference nearly 12 inches. The ovaries were small and tough, and much cicatrized. The uterus was of a moderate size. The walls of the lower part of the body were very flaccid, and the fundus was bent backwards, so as to form an angle with the lower part of the body. The right kidney was normal. The liver also was normal. The spleen was of moderate size, and very soft. The lumbar glands on the left side were somewhat enlarged, and infiltrated with medullary matter. On microscopical examination, the tumour was found to consist almost entirely of round and oval nuclei, about the size of blood-globules, with a small quantity of very delicate fibres and long slender fibre-cells. There was no mark of the tumour having been punctured.

In this case the extraordinary nature and position of the tumour are sufficient to account for the error of diagnosis. Certainly the last thing one would have expected to find on opening the abdominal cavity would be the kidney lying as it did on the surface of the intestines. The only way in which the unusual position can be accounted for is, that the weight of the tumour gradually bore the kidney downwards and forwards, and that the continued rolling movements of the intestines on it gradually urged it forwards, and, dragging on the peritoneum, it formed for itself a mesentery, which became elongated as the tumour increased in size and weight. It might be said that the mobility of the tumour upwards would have determined that it was not ovarian; but during life motion in this direction was limited by the transverse colon and stomach, and it was only when these were removed at the post-mortem examination that it could be made to reach the diaphragm. So far from the kidney being suspected as the seat of the disease, the urine examined on two or three occasions was ascertained to be healthy, with the exception of being of a low specific gravity. There was no cachectic appearance nor wasting of the tissues to suggest a malignant growth, but the woman was well nourished and appeared in fair health and well suited for an operation. The case is important as it adds another fact to the history of the difficulties in the diagnosis of ovarian disease, and it is recorded with the sincere hope that it may be an aid towards elucidating the diagnosis of tumours apparently within the cavity of the abdomen.—*Lancet.*

RETENTION OF URINE MISTAKEN FOR LABOUR. BY R. L. JOHNSON, F.R.C.S.

To the young practitioner the following case may prove of interest, as it illustrates a " point " in practice :—

In the absence of the "recognised" family medical attendant, I was called on to visit Mrs T., of Stanmore Street, and informed that her case was most urgent. Forty hours previously symptoms of labour had commenced, and had, since that time, increased hourly, and as the family medical attendant, who had on three occasions seen Mrs T. since "labour commenced," was from home, when sharp and continuous pains seemed to denote instant delivery, and as he was not expected to return home for some hours, I was informed that my presence was promptly required.

Mrs T. was, of course, in her bedroom in *dishabille*, and in much agony. A monthly nurse was in attendance, also Mrs T.'s mother (who was the mother of sixteen children), a fire blazed in the bedroom grate, before which was placed a "clothes-horse," heavily laden with the recognised freight of "swaddling clothes," etc.

Mrs T. is 35 years of age, below the middle height, and of a weak habit of body. She has had four children, all alive, and are strong and healthy. She complained of periodic "bearing-down" pains, accompanied by a discharge, that she could not walk, nor lie on either side, and the only relief she experienced was when on her back and with her extremities semiflexed, but in this position she could not for any length of time continue on account of "shooting pains" in her back, and the sense of suffocation she experienced.

On examination I found her pulse 90, her tongue typhoid, her skin hot and clammy, and during pains copious perspiration visible on her head, face, and palms of her hands. She slept but little, had strange dreams, had occasional mental wanderings when awake. She could form no opinion as to about the time of her expected confinement, as "matters had gone wrong with her for two years previous;" but she had considered her *size to have increased* very much within the *last few days.* Her stomach was irritable; her appetite poor indeed; her bowels were constipated; and she had insatiable thirst. I was shown a small quantity of her urine; it was high coloured and contained pus, and I was told that each movement she made, and during the pains, a *considerable* quantity of urine passed involuntarily from her. I informed her of my belief that she *was not in labour*, but suffered from *retention of urine.* I asked to be permitted to examine the surface of her abdomen; the privilege was reluctantly allowed me, for "her own doctor" had examined her per vaginam, and had pronounced her labour to be a slow one but fraughtless of danger." Indeed, my "inexperience" was hinted at by her mother and by the nurse, and the very notion of my patient having retention of urine regarded as absurd, as a proof of which absurdity a part of the bedroom carpet was raised to convince me that urine had been passed in such quantity as to leave unmistakable signs of its presence on the boards beneath. On examination, I found the surface of her abdomen tense, hard, and unyielding, and a little below and to the left of the umbilicus, dulness on percussion; whilst the remaining portion of the surface of her abdomen was tympanitic, but exhibited between either side of the mesian line and the recti muscles, and just above the pubes, thumb-like prominences, which were very painful to touch and semi-elastic.

From the conversation I had with my patient, and from this last fact, I felt convinced that my diagnosis was correct; and, although labour might be at hand, retention was undoubtedly its precursor. I desired to pass a catheter, but would not be permitted to do so; and, as I would not be permitted to treat the case in my own fashion, and not being disposed to pronounce "labour" at hand, I cautioned my patient and her friends to get immediate medical aid and not lose a moment in its procuration, and then left the house.

Three hours from the time of my withdrawal from Mrs T.'s case I was again sent for, and *carte blanche* given me to do as I thought proper, and I had the satisfaction to remove from my patient more than *nine pints* of urine! During its removal I had to apply a swathe, as in the operation for ascites, and to administer stimulants on account of extreme prostration.

May I add that, from the time of the withrawal of the urine from Mrs T., all "bearing down pains" ceased for three months! Then they returned, "short, sharp, and decisive," and brought with them a natural labour, a healthy female child, and not accompanied with, nor followed by retention of urine.—*Dublin Medical Press.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLIV.—MEETING V.

1*st March* 1865.—Professor MACLAGAN, *President* of the Society, in the Chair.

I. SCROFULOUS CARIES OF THE SPINE LEADING TO PARAPLEGIA.

Dr Haldane showed a specimen of caries of the bodies of the ninth and tenth dorsal vertebræ. The body of the ninth vertebra was softened and of almost cheesy consistence, that of the tenth in an earlier stage of the same condition. The spinal cord in the corresponding situation was softened, but it had not yet been microscopically examined, as it was hardening at present. The patient from whom the preparation was obtained was an Irish labourer, 35 years of age. He had been admitted into the Royal Infirmary on the 4th November last, complaining of pain in the lumbar region. The pain seemed of the character of lumbago, being much increased on attempting to stoop, or when pressure was made over the part. The patient ascribed it to exposure to cold and wet. Under dry cupping and the use of turpentine stupes the pain was abated, so that stooping could be accomplished without much uneasiness. Soon, however, the patient began to complain of loss of power in the lower limbs, which gradually increased until complete paraplegia was established. The sensibility and temperature of the legs remained unaltered, the urine was natural, and there was complete power over the bladder and rectum. A little uneasiness was referred to the back, but there was no sensation of girding round the body. The idea entertained of the case was that very chronic softening of the cord was in progress. A suspicion that there might be cancerous or tubercular disease of the vertebræ was abandoned on the ground that there was no appearance of cachexy; that pain was trifling, and that there was little tenderness on pressure; that there was no angular projection nor any indication of disease in other regions. About the middle of February a slough began to form over the sacral region; from this time he rapidly sank, and died on the 22d February. On post-mortem examination a few nodules of tubercule in an apparently quiescent condition were found in the lungs and liver. The case was interesting both from the comparatively advanced age at which the disease had manifested itself, and from the great obscurity of the diagnosis.

II. INJURY OF THE HEAD.

Mr Annandale brought before the Society a boy who, last autumn, had met with a severe injury of the head, which rendered it necessary to remove a portion of bone larger than half-a-crown. The boy had recovered perfectly, and was evidently in the enjoyment of robust health. His intellectual faculties were unimpaired.

III. CANCER OF THE BRAIN.

Dr Sanders showed a cancerous tumour of the brain. It was about one inch in diameter, and was exclusively confined to the white matter of the right hemisphere. It was situated near the upper surface of the superior and external part, corresponding to the junction of the anterior and middle lobes. It was ascertained that it did not involve the gray matter, or to a very slight extent, being completely surrounded by white matter. All the rest of the brain was healthy. There was a good deal of serum in the ventricles. The subject of the observation, a woman 55 years of age, was admitted into the hospital about three weeks ago suffering from hemiplegia of the left side. The loss of power in the left arm was complete; the muscles were nearly flaccid. There was some power in the left leg. There was incomplete paralysis of the left side of the face. Dr Sanders remarked that the case was interesting on account of paralysis having been produced by a tumour which involved neither the corpus striatum, the optic thalamus, nor the gray matter. It was possible that a certain degree of pressure had been exerted on the left hemisphere, as the presence of the tumour had pushed it somewhat beyond the middle line. The patient had had no sensory paralysis or hyperæsthesia; the intellectual faculties were entire. Cancer had also been found in the cervix and part of the body of the uterus and in the neighbouring glands. Dr Sanders observed that Dr Todd had referred facial hemiplegia to paralysis of the fifth, not of the seventh nerve, because the orbicularis palpibrarum was not paralyzed. Dr Sanders had tested this statement on this and other occasions, but had found nothing to justify Dr Todd's opinion. It was impossible to understand how drooping of the features and imperfect closure of the mouth could depend upon paralysis of the fifth nerve; moreover, there was no twisting of the jaw as if the motor root of the fifth was affected. Dr Sanders had observed that the same idea had been repeated both in the second and third editions of Dr Aitken's work on the Practice of Medicine, and in some other text books, and he had now alluded to it as likely to lead to error.

Dr Grainger Stewart had minutely examined the tumour, and found it to contain cells of a distinctly cancerous character; many of the cells contained several nuclei. The deposits in the uterus and neighbouring glands presented the same characters.

IV. MINER'S LUNG.

Dr Grainger Stewart showed the lungs and spleen from a miner who had been a patient in the Royal Infirmary under Dr Warburton Begbie. The lungs were everywhere of a black colour, and in some places were quite consolidated from the abundance of the carbonaceous matter. There were cavities in the lungs, but no tubercle. On microscopic examination, black pigment was found both in the free state and contained within the epithelial cells. An interesting feature of the case was, that a large quantity of pigment was found in the spleen, and in the substance of some of the serous membranes.

V. ON THE NATURE AND TREATMENT OF URÆMIC CONVULSIONS.

Dr Haldane read a paper on this subject, which will be found at p. 882 of this number of the Journal.

Professor Simpson said, that, with regard to bleeding in cases of uræmic and puerperal convulsions, he was inclined to doubt the propriety of the practice. It had been carried to an enormous extent in Edinburgh, particularly by Dr Hamilton, who said he had never seen a patient die who had been bled sufficiently freely. Dr Simpson had himself seen 80 ounces of blood drawn within a few hours. Bleeding had formerly been considered the great panacea, but now opinion had changed; the poison which produced the convulsions was not one which could be removed by bleeding. One was no doubt inclined to use the lancet in cases of puerperal convulsions, not with a view to depurate the system of the poison, but to stay some of the bad effects of the convulsions, such as encephalic congestion, and even rupture of bloodvessels, which sometimes occurred. Dr Simpson had been asked to go to England, two or three days ago, to see a case of puerperal convulsions at the seventh month. As it was not convenient to go at that time, he telegraphed back, and, among other things, suggested chloroform. Being, however, again sent for, he went and saw the lady, within fifty hours from the commencement of the attack. The patient, a stout, healthy-looking person, had got cold; this was followed by œdema, and the result was six severe fits, coming on suddenly. On asking her medical attendant, a most accomplished medical man, if he had bled her, he emphatically replied that he never bled now in almost any kind of case, and had no faith in it. Chloroform had been administered, and the patient kept under its influence for eighteen hours. There was no recurrence of the fits. The case had presented many obstetric difficulties, but into these he would not enter. Dr Haldane, in speaking of the pathology and mechanism of uræmic convulsions, had discarded Frerichs' view, that they were occasioned by the conversion of urea into carbonate of ammonia. It was pretty certain that that opinion was untenable; but were we correct in coming back upon urea? Dr Simpson believed that we ought to say that we knew nothing quite certain in the present state of pathological and chemical knowledge about the morbific cause of these convulsions. An excess of carbonate of ammonia in the blood did not produce the true phenomena of puerperal convulsions; neither did an excess of urea. Such was at least the result of direct experiments on the lower animals, and of clinical observations on the human subject. In his own mind he had no doubt that some kind of poison was present in the blood, which acted like a vegetable alkaloid, and he believed it would probably turn out to be an alkaloidal poison generated in the animal system. Therefore he considered it right, in the treatment of these convulsions, to give an acid, especially acetic acid, because the acetates of most alkaloids readily passed out of the body; and he thought he had seen good results in such cases from the administration of large doses of vinegar. Patients, when liable to fits of convulsions, were in much the same state as if strychnia had been administered to them. If kept very quiet, they remained well, but if touched or otherwise irritated, they were thrown into convulsions. Hence, in puerperal convulsions, if all causes of irritation were promptly removed, there was more chance of the patient's safety. Dr Simpson had not seen chloroform used in adults in non-puerperal cases of uræmic convulsions, but he had often employed it in the convulsions of children with albuminous urine. In the case of a child six weeks old, whom he had seen with Dr Combe, he had recommended treatment by chloroform. The child was put under chloroform, and kept under its influence for some hours. On recovering from the anæsthesia the convulsions returned. The disposition to convulsions remained for a whole fortnight, and during all this time the child was kept under chloroform, being only waked up at intervals to be fed. The child recovered perfectly. Dr Simpson thought that perhaps even in adults, if the anæsthetic were kept up long enough, good results might be obtained. It seemed to destroy that increased polarity of the nervous system, which was the result of the poison in the blood; but, to ensure its full effect, its administration required to be kept up for a considerable length of time.

Dr Argyll Robertson demurred to the view that the amount of congestion of the brain was indicated by the state of the pupil. He did not deny the accuracy of the experiments referred to by Dr Haldane, still it was certain that we might have congestion of the brain while the pupils remained uncontracted. Also, the condition of the pupil varied with the power of accommodation in particular eyes. In far-sighted persons the pupil was small; in near-sighted, large. Dr Robertson held that the condition of the pupil did not depend on increased vascularity, but on nervous influences. We knew that the iris was moved by two sets of muscles, the contracting fibres (under the influence of the third pair), and the dilating fibres (regulated by the sympathetic). He therefore held that when contraction of the pupil occurred in cases such as the one narrated by Dr Haldane, it was due not to congestion of the cerebral system, but to some condition of the nervous system such as was produced by the administration of opium.

Dr Arthur Gamgee would say a few words regarding the delicacy of Kühne's method of testing for ammonia. He had himself repeated all the experiments, and had completely corroborated the results arrived at. He had used much larger quantities of blood than Kühne. Kühne had used only 6 or 7 ounces of blood. But as Dr Gamgee was anxious to entirely refute Dr Richardson's theory, he had on one occasion used 140 ounces of blood, taken by means of a tube from the carotid of a horse, and conducted into a flask through which pure hydrogen (previously tested) was bubbling. No trace of ammonia was detected. In another case he had tied the renal vessels of a dog; and when the first symptoms of uræmia showed themselves the animal was bled to death, the blood was examined, but no ammonia detected. The test employed in these cases was Nessler's reagent, which was an exceedingly delicate one, both for ammonia and for the compound ammonias. Richardson had said in his work that he believed that it was ordinary ammonia which was present in the blood, still he did not commit himself. Any one who had examined the compound ammonias must know that their smell was very like that of ammonia. Dr Gamgee had made experiments with ethylamine, tricthylamine, methylamine, aniline, etc., and found that they were most readily recognised by Nessler's reagent. He thought, therefore, we might say that not only was ammonia not present in the blood, but that probably none of the compound ammonias were. In one experiment he had added to 140 ounces of blood, proved to contain no ammonia, ·55 grain of that alkali, and unequivocal proofs of the presence of ammonia were obtained. Dr Richardson had said that one part of ammonia was sufficient to hold in solution the fibrine of 8000 parts of blood. This blood, therefore, should have contained about 8 grains of ammonia.

VI. ON THE PHYSIOLOGY AND PATHOLOGY OF CERTAIN FORMS OF DILATED PUPIL.

Dr Joseph Bell read a paper on this subject, which will be found at page 917 of the present number of this Journal.

GLASGOW MEDICO-CHIRURGICAL SOCIETY.

At a Meeting of the Society, held in the Faculty Hall, on Tuesday the 14th of March, the following gentlemen were elected office-bearers for the present year:—*President*—Dr Ritchie. *Vice-Presidents*—Dr S. Orr; Dr Robert Paterson. *Council*—Dr J. G. Wilson; Dr Prichard, Partick; Dr Maclaren; Dr Naismyth, Hamilton; Dr W. T. Gairdner; Dr Yeaman. *Secretaries*—Dr Adams; Dr Macleod. *Treasurer*—Dr Coats.

ARMY MEDICAL SERVICE.—ASSISTANT SURGEONS.

The Director-General of the Army Medical Department presents his compliments to the Editor of the *Edinburgh Medical Journal*, and begs to enclose a List of the Candidates who were successful at the Competitive Examination in August last, and who have passed through a Course at the Army Medical School, showing the combined results of the Examinations.

Names.	*Studied at*	*Marks.*
W. H. Jameson,	Edinburgh,	5247
R. Harvey,	Aberd. & Glas.	5020
J. H. C. Whipple,	London,	4813
J. Cleghorn,	Edinburgh,	4640
W. W. Tomlinson,	Dublin,	4545
W. S. M. Price,	Dublin,	4497
G. F. Duffy,	Dublin,	4490
J. Bennet,	London,	4369
T. F. O'Dwyer,	Cork,	4310
J. R. H. Boileau,	Dublin,	4305
H. Cook,	Cork,	4302
W. Carpenter,	Dub., Galway,	4295
G. M'I. Campbell,	Aberdeen,	4202
J. F. Foster,	London,	4190
R. A. Farquharson,	Aberdeen,	4160
W. Keir,	Aberdeen,	4040
L. Corban,	Cork,	3925
F. Tuite,	Dublin,	3925
W. Taylor,	Glasgow,	3895
P. A. Smith,	Cork,	3887
P. Shepherd,	Aberdeen,	3882
J. M'K. Maclean,	Edinburgh,	3855
P. G. Power,	Cork,	3855
W. T. Hedley,	Edinburgh,	3793
T. A. Purcell,	Dublin,	3782
R. D. Kemp,	Aberdeen,	3756
S. W. Handy,	Dublin,	3705
E. O'Sullivan,	Dub. & Cork,	3699
A. E. Hale,	Birmingham,	3690
J. MacCreery,	Dublin,	3646
H. C. Gillespie,	Cork,	3630
C. Spurway,	London,	3530
W. MacConnell,	Dub. & Belfast,	3528
J. Hector,	Aberdeen,	3520
W. L. Eames,	Cork & Edin.	3512
C. E. M. Shaw,	London,	3494
A. Dunn,	Dublin,	3490
V. Stone,	Edinburgh,	3469
K. Macaw,	Belf. & Glas.	3460
W. E. Riordan,	Dublin,	3426
T. Bourke,	Dublin,	3417
J. Murray,	Aberdeen,	3405
J. B. Kelly,	Dublin,	3399
D. C. G. Bourns,	Dublin,	3397
J. F. Welsh,	Edinburgh,	3396
J. H. Hughes,	Gal. & Dub.	3337
J. W. Jones,	Dublin,	3331
S. E. Walker,	Birmingham,	3292
J. F. Blake,	Dublin,	3290
A. B. Robinson,	Dublin,	3284
D. A. S. Thorburn,	Edinburgh,	3247
E. R. O'Brien,	Cork & Dub.	3178
J. MacCully,	Belf. & Dub.	3170
W. H. Jagoe,	Dublin,	3170
W. Blake,	Dublin,	3163
C. Smith,	Dublin,	3135
J. Duhy,	Dublin,	3116
E. T. Haward,	London,	3097
D. C. Grooe,	Dublin,	3040
F. F. Dickinson,	Gal. & Dub.	3040
A. Barrie,	Glasgow,	3038
S. Doake,	Belfast,	3027
S. Flood,	Dublin,	3000
G. B. West,	Dublin,	2993
D. J. Canny,	Dublin,	2975
E. Vallance,	London,	2969
J. Candy,	London,	2861
J. B. Ryan,	Dublin,	2785
C. F. Hutchinson,	Dublin,	2735
E. Eustace,	Dublin,	2705
F. J. Peatfield,	London,	2693
J. R. R. Coulten,	Dublin,	2619
C. Backhouse,	Dublin,	2600
E. Ward,	Dublin,	2581
J. Maturin,	Dublin,	2537
D. Renton,	Edinburgh,	2455
C. Healy,	Dublin,	2392

CONVERSAZIONE OF THE ROYAL COLLEGE OF SURGEONS.

On Friday the 24th February, a Conversazione of the Royal College of Surgeons was held in their Hall, Nicolson Street, when Professor Douglas Maclagan delivered a Lecture on "Civil Incapacity," which will be found at page 865 of the present number of this Journal. On the motion of Lord Deas, seconded by Professor Blackie, a vote of thanks was accorded to Dr Maclagan.

MORTALITY IN PARIS.

The following statistics of the diminished mortality in Paris extend over a period of twenty-four years. In 1841 the population of twelve parishes amounted to 935,000 persons, and one death in 36 is proved. In 1864 the number of deaths was one in 40. Wide streets and open boulevards have replaced the narrow passages and crowded courts of old Paris. Also there is an immense increase in the quantity of water. In 1840 65,000 cubic metres were distributed in twenty-four hours, whereas in 1863 133,258 cubic metres were supplied. In 1840 there were 36,000 metres of sewers, whereas in 1863 the sewers of Paris attained the surprising length of 350,000 metres—that is, 90 leagues. Another cause of the increased salubrity of Paris is the immense number of squares and open gardens created for the use of the people.

PUBLICATIONS RECEIVED.

Adams,—Lectures on Curvature of the Spine. By W. Adams, F.R.C.S., etc. London, 1865.

Hawkins,—The Works of Sir Benjamin Collins Brodie, Bart., with an Autobiography. Collected and Arranged by Charles Hawkins, F.R.C.S.E. In three volumes. London, 1865.

Jones,—The Spirit of Nursing. By Harry Jones, M.A. London, 1865.

M'Donnell,—The Functions of the Liver. By Robert M'Donnell, M.D., etc. Dublin, 1865.

Manchester Eye Hospital Annual Report.

Playfair,—Handbook of Obstetric Operations. By W. S. Playfair, M.D. London, 1865.

Richardson,—For and against Tobacco. By B. W. Richardson, M.D., etc. London, 1865.

Routh,—Fibrous Tumours of the Womb. By C. H. F. Routh, M.D., etc. London, 1864.

Schmid and Milner,—Inhalation the most Rational Treatment for Diseases of the Respiratory Organs. By C. T. Schmid, M.D., and C. Milner, M.D. 1865.

Smith,—Inhalation of Gases and Medicated Vapours in the Treatment of Consumption. By W. Abbots Smith, M.D., etc.

Waring,—Manual of Practical Therapeutics. By E. J. Waring, F.R.C.S., etc. Second Edition. London, 1865.

PERIODICALS RECEIVED.

American Journal of the Medical Sciences,—January 1865.

Berliner klinische Wochenschrift,—Nos. 9, 10, 11, 12.

Births, Deaths, and Marriages, Monthly Return of, for February 1865.

British Medical Journal,—Mar. 4 to 25, 1865.

Dublin Medical Press,—Mar. 1, 8, 15, 22, 1865.

Gazette des Hôpitaux,—Nos. 23 to 32, 1865.

Gazette Hebdomadaire de Médecine, etc.,—Nos. 8 to 12, with titles, etc. Paris, 1865.

Gazette Médicale de Paris,—Nos. 8, 9, 10, 11.

Medical Times and Gazette,—Mar. 4 to 25, 1865.

Revue de Thérapeutique Medico-Chirurgicale,—March 15. Paris, 1865.

Virchow's Archiv,—Vol. 32, Nos. 1 and 2. Berlin, 1865.

Würzburger Medicinsche Zeitschrift,—Vol. 5: Nos. 4, 5, and 6. 1864.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Notes on Fetid Bronchitis, and other Lung-Diseases with Fetid Breath.* By THOMAS LAYCOCK, M.D., etc. etc., Professor of the Practice of Medicine and Clinical Medicine, and Lecturer on Mental Diseases, in the University of Edinburgh.

I OBSERVE in the March number of the Edinburgh Medical Journal a paper on the chemical composition of the sputa in cases of fetid bronchitis and pulmonary gangrene. The conclusions drawn seem to me to indicate that the writer has not observed a case of fetid bronchitis, nor even mastered the literature of the subject. And in reference to my own humble researches, I may say that I do not hold the views which he seems to attribute to me, probably from misapprehension or ignorance of what I think on the matter. I will therefore beg leave to occupy your pages with some elucidatory notes on fetid bronchitis and other pulmonary diseases with fetor of the breath.

My first observations were made twenty-eight years ago, and were published in the London Medical Gazette for December 1837.[1] In that paper I compared a well-marked case of fetid bronchitis with another of pulmonary gangrene (both which I had carefully observed), with especially reference to the origin, nature, and significance of the fetor in certain kinds of bronchitis. As to these points, I am not aware that anything of moment has been added to what I then wrote, except in my Clinical Lecture, published nearly eight years ago,[2] and in which the results of a chemical examination of the sputa were published for the first time. The lecture attracted attention and remarks both in France and Germany; and I think I am now able to throw some additional light on this group of pulmonary diseases.

There are three kinds of pulmonary affection in which there is fetor *sui generis* of the breath, and which is mistaken for that of pulmonary gangrene.

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[1] Two Cases of Pulmonary Disease, with Remarks, by T. Laycock.

[2] Medical Times and Gazette, May 1857.

I. Affections in which there is no *cognisable* lung-disease, and no fetid sputa.

II. Affections in which there is a fetid bronchitis, bronchorrhœa, or broncho-pneumonia, with or without dilated bronchi.

III. Affections resembling a consumption, in which there is a fetid cavity, abscess, or vomica, lined with a pyogenic membrane, and situated in a portion of lung which has undergone fibroid degeneration or condensation.

I shall consider these three kinds in succession.

I. *Pulmonary Fetor of the Breath, without Cognisable Lung-Disease.*

I have observed that there may be pulmonary fetor of the breath without *cognisable* lung-disease. In cases of this kind it is necessary to determine that the breath-fetor is really of pulmonary origin; for it is often caused by morbid conditions of the nostrils, mouth, and throat. Of this kind are ozæna, caries of the teeth, gangrene of the mouth, and certain diseases of the tonsils and glands of the pharynx. The pulmonary halitus in a state of health seems to be rather of an agreeable odour than otherwise. It is changed, however, into one more or less disagreeable under various constitutional changes, as habitual constipation, pregnancy, scorbutus, alcoholism, or even from long abstinence from food. In these and other instances we recognise the pulmonary origin of the fetor, partly from the history of the case, partly from the character of the odour, but chiefly from a careful examination of the nostrils, mouth, and throat, including the pharynx.

Now, according to my experience, pulmonary breath-fetor, closely resembling that which characterizes fetid bronchitis, may occur without any manifest pulmonary disease whatever, and be a source of great misery and loss. A middle-aged lady (a governess) consulted me for an odour of the breath, which excluded her from society, and incapacitated her from following her profession, in which she stood high. I recognised the peculiar character of the odour, but was surprised to find no pulmonary signs or symptoms whatever. The tonsils sometimes are diseased in such a way that their crypts contain a cheesy fluid, having a very bad smell, which closely resembles that of fetid bronchitis, but is less fecal in character, and in this way the breath is tainted. In these cases the fetid stuff can be squeezed from the tonsils by pressure upon them. There is also a chronic morbid condition of the pharynx, in which a sort of fetid purulent stuff exudes, and renders the breath offensive. In the case of this lady I could find nothing the matter in these respects. The nostrils, teeth, and gums had been already carefully attended to. I could only conclude, therefore, that the fetid halitus was pulmonary in its origin, yet without any cognisable pulmonary lesion or symptom. The patient was dyspeptic and cachectic-looking, and had very much the complexion of persons with chronic rheumatism. No drugs she had tried had afforded more than a temporary relief from the fetor; but I was in hopes that a course of the Vichy alkaline waters would be beneficial. Since my attention was directed, by this case, to the fact that the peculiar odour of fetid bronchitis might be evolved from the lungs without structural disease either of the bronchi or parenchyma, I have noted other cases,—of a less decisive character, however, because of the less intensity of the fetor. The conclusion is supported indirectly by what I have observed in cases of undoubted lung-affections with the peculiar odour; and I use the phrase "peculiar odour" advisedly, for it seems necessary to state explicitly that it is *sui generis*, as compared with other pulmonary fetors.

II. *Fetid Bronchitis, Bronchorrhœa, or Broncho-Pneumonia, with or without Dilated Bronchi.*

There are cases of fetid pulmonary affections in which the cause of the fetor and of the fetid sputa is in the bronchial exudations or excretions. As compared with the next class they are of little gravity, although apt to become suddenly serious, and even fatal. In some there is only a bronchorrhœa; in others a chronic bronchitis; and in others an acute or subacute bronchitis, or bronchorrhœa, with a localized affection of the lung-tissue—a broncho-pneumonia of a peculiar kind. Such was my first-recorded case, in which the bronchorrhœa was so copious that forty ounces of sputa were expectorated in a few hours.[1] These cases have attracted much attention, because of the discrepancy between the apparent gravity of the symptoms and the course and termination. Van Swieten observed some such cases, for he declares, in opposition to the opinions of "various illustrious and ancient authors," that he has seen persons who expectorated the most fetid sputa, and yet lived long, and were able to transact their ordinary business. He mentions especially the case of a youth who, as the sequel to an illness, began to expectorate sputa so stinking that he (Van Swieten) could hardly endure the stench, although by no means delicate in such matters. The expectoration continued for two years, without the general health being affected, when it suddenly increased in quantity, the patient became emaciated, and death quickly followed.[2] Several of our best modern observers have confirmed this observation of Van Swieten; of these the illustrious discoverer of mediate auscultation was the first, to be followed by Elliotson, Andral, Briquet, Stokes, and others. Laennec observes that the expectoration in chronic mucous catarrh sometimes becomes more or less fetid, and occasionally approaches the gangrenous odour. In discussing "gangrenous eschar," he mentions that in two or three cases in which there was a gangrenous fetor, he found after death nothing which could account for it, unless it were the rapidity with which the body generally, and the mucous membrane of the lungs more particularly, ran into putrefaction. From the result of several cases of recovery, he was "tempted to believe" that the odour had its origin in the mucous secretion of the bronchi.[1] Dr Elliotson notices the same kind of affection. He says he has seen persons with such a fetid expectoration that their sisters could not bear to sleep with them, and yet no sign of danger whatever presented itself, and the patients did exceedingly well. They were going about without any particular ailment, except that they had a copious expectoration. In colour it was sometimes bluish, sometimes black, yellow, green, brown, and even reddish, from having a little blood in it. Then as to quantity, it may vary from a few ounces to two or three pints in the twenty-four hours.[2]

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[1] I reprint this case in an Appendix (Case I.), page 977.

[2] Comment. apud Boerhaave; 2d Ed. Leyden: tom. iv. p. 72.

The anatomical changes in the bronchial mucous membrane are those of the so-called chronic bronchitis. It is thickened, swollen, and of a dull red colour. This conclusion I draw from recorded dissections. Andral relates the case of a man-cook, who was admitted to the hospital La Charité, coughing much, and expectorating a great quantity of *very fetid* greenish sputa, which flowed out in one sheet when the vessel was tilted. One would have thought that the liquid came from a pleuritic sac or a vast tubercular cavity. The patient said he had had a similar expectoration for several years. The chest sounded well everywhere on percussion. He died with œdema of the brain and lungs. The lungs were found to be melanotic and firm at points; the large bronchi (full of a fluid like that expectorated as to its extreme fetor) were white in their inner surface; but the smaller bronchi (also full of the fetid fluid) had the mucous membrane of a deep red. These were therefore the source of the sputum; there were no cavities.[3] The coincidence of the melanotic firm lung with the bronchial change is noteworthy. Cruvelhier records a somewhat similar case. He states that he attended a youth who, for two years previously, had expectorated every morning a "verre" of horribly fetid pus, and who died of serous effusion into the ventricles of the brain. On making a *post-mortem* examination, no pulmonary abscess or cavity was found, but the bronchi of the upper lobe of the *left* lung were observed to be full of pus, and their mucous membrane very red, and much thickened. The lobe itself was softened and infiltrated, and had none of the characters of pulmonary tissue.[4] In this case there was manifestly an etiological connexion between the condition of the bronchi and the lung with which they were connected.

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[1] A Treatise on Diseases of the Chest, etc. Translated by John Forbes, M.D. 3d Ed., p. 231.

[2] Lectures on the Theory and Practice of Medicine. Edited by Drs Cook and Thompson. P. 487.

[3] Clinique Medicale, 1824, tom. ii. p. 52.

[4] Anatomie Pathol.: Maladies du Poumon, tom. i. liv. 3, p. 6.

Dilatations of the Bronchi are often concomitants of chronic bronchitis, and have been found after death in bad cases of pulmonary fetor. M. Briquet of the hospital Charité concluded, twenty-five years ago, that in certain cases of pulmonary fetor, the extremities of the bronchial tubes became dilated and gangrenous, independently of any other portion of lung-tissue, and that the gangrene was the result of either a general bronchitis, or of a bronchitis limited to the affected bronchi. He ably details two cases he treated in the Hospital Cochin.[1] One of these, in a male, was of the class to be next considered, for the dilated bronchi were complicated with a fetid pyogenic cavity in the left fibroid lung; the other was of a wholly different kind. It was that of a pregnant female who had fetid abscess in the right labium, and a gangrenous (pyæmic ?) cavity, with pleurisy of the right lung. This had no communication with the bronchi, and there was no fetor of the breath or sputa. More recently, M. Lasègue has published an able essay, in which he examines this class of affections, indicating more particularly their recurrent and curable nature, and the need of differentiating them from true pulmonary gangrene. He mentions a case under the care of M. Behier, of a woman who, after an attack of a pernicious intermittent, had for nearly a year frequent paroxysm of violent cough and shortness of breath, with very copious expectoration of a fetor almost intolerable. She perfectly recovered.[2] He quotes also a similar case recorded in The Lancet for May 1854, and cases by Professor Skoda and Dr Helm. M. Lasègue rightly doubts whether these cases are gangrene of the lung at all, but leans to the opinion expressed by Laennec, that the odour is due to a putrescence of the bronchial secretion. Their catarrhal characters, whether as to the nature of the expectoration, the course, and the termination, are undoubted. Professor Diettrich of Erlangen, connecting the affection with dilated bronchi, as M. Briquet had done already, thinks that the fetor and fetid sputa are due to the putrescence of the fluids which accumulate therein, and that these irritate the bronchi.[3] I need not examine these theories; it is enough to say here, that cases occur without dilated bronchi, and dilated bronchi are very common without any putrescence.

The preceding instances may be held to be chiefly or wholly bronchial; there are others, however, in which the physical signs point to a *localized broncho-pneumonia.* The signs of consolidation are commonly limited to a single lobe, and that usually a lobe of the left lung, and are not unfrequently combined with the signs and symptoms of a localized pleurisy in the same region. I have subjoined the details of a case of this kind I had under treatment in the Royal Infirmary.[1] At the commencement the disease may be exclusively bronchial; there is no dulness on percussion, and moist sounds only are heard. But if the local or "lobar" pneumonia be not manifested at first, it is subsequently, for the disease advances by recurrent attacks of acute pulmonary affection, cough with pain—sometimes acute and even distressing in the region of the affected lobe—and copious fetid expectoration. These continue for from seven to twenty-one days, when the symptoms abate, the fetor disappears, and a state of comparative health is experienced. With each recurrent attack the signs of consolidation are apt to become more marked, and finally there may be those of a cavity, when the disease, if not fatal, becomes chronic. In this stage it belongs to the next class of cases; still, however, manifesting exacerbations. Dr Stokes has elucidated what I term the fetid broncho-pneumonias in an able monograph.[2] He describes two cases of a more acute character than mine, in one of which (that of a lady) the physical signs never indicated disease over a space of more than about four fingers in breadth and three in depth in the postero-inferior portion of the *left* lung. The course of the disorder was similar to that of other recorded cases. There were recurrent attacks of high fever, and much constitutional disturbance, with violent cough, localized pain, and extreme fetor of the breath and sputa, continuing for about a week, when the symptoms subsided, the fetor disappeared, and the patient apparently recovered. In the male case there was hæmoptysis—in the case of the lady an expectoration of great quantities of muco-purulent fluid often tinged with blood, and the most agonizing pain in the lower portion of the left side, where, however, there was no stethoscopic indication beyond a slight and diffused mucous râle, but clear percussion. The male patient died at Rome of acute pneumonia; the female fell into a permanently febrile state, in which, however, the remittent character of the local disease continued manifest, and died. Within a few weeks of her death signs of a cavern were noted, the mucous râle having gradually changed into obscure gurgling. The diagnosis of "local gangrenous disease" of the lung in these cases rested chiefly on the fetor, as there was no post-mortem examination. And I think Dr Stokes uses the phrase rather nosologically than pathologically, for it is clear that he thinks there is rather a putrescence of the fluids than a gangrene of the tissues. He thinks the expectoration of fetid stuff is no proof of a cavity, because he has known it to occur so shortly after the operation of the exciting cause, that it is difficult to conceive a cavity could be formed so rapidly. Besides, the surface which pours forth the putrid fluid must be extensive, because of the quantity; while physical signs show that the portion of the lung which suffers death must, at the first, be insignificant in extent. And he thinks it almost certain that the fetid fluid is originally poured out in a putrid condition. In short, he looks upon the disease as being, in the first instance, a disorder of the bronchial secretion, and considers that the "local gangrenous disease" with which the case ends is consecutive thereto. It is certain, however, that in some of the cases the bronchitis and pneumonia begin together. There is, in fact, a distinct form of "lobar" broncho-pneumonia not noted by systematic writers, which resembles the fetid kind in all particulars except the odour, and which is by no means rare, although confounded with pneumonias in general.

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[1] Mémoire sur un Mode de Gangrène du Poumon, dependant de la Mortification des Extremités dilatées des Bronches: Archives Generales, May 1841.

[2] Des Gangrènes curables du Poumon, par le Dr Ch. Lasègue: Archives Generales, July 1857: p. 26.

[3] Ueber Lungenbrand, in Folge von Bronchien Erweiterung: Erlangen, 1850.

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[1] See Case III., in Appendix, p. 981. Also Case I., of S. Battley. Andral reports an example in Clin. Médicale, tom. iii. p. 152.

[2] Clinical Researches on Gangrene of the Lung; by Wm. Stokes, M.D., etc. etc.; Dublin Quarterly Journal of Medicine, February 1850, p. 1.

Hæmoptysis may be as predominant a symptom as fetor. The most curious example of this kind I find amongst the older writers is recorded by the celebrated Willis, amongst his "Examples of those that spit blood." It is the case of the Rev. Dr Berwick, Dean of St Paul's. "That most reverend divine," Willis says, "fifteen years before he died, laboured with a most obstinate cough, and sometimes with a bloody and salt spittle, with a grievous breath, stinking like hell; by which being made lean by a pining away of the body, he wanted but little of being almost extinguished by a consumption. As often as his spitting of blood intermitted, the rankness of breath and spittle ceased also; afterwards, the return hereof declared constantly that other effect to be presently attendant." He was thrown into prison for political causes, where he drank "meer water instead of ordinary drink," recovered his health, and "so remained indifferently healthful for above ten years' space." He afterwards had a return of the "cough, with the bloody and stinking spittle," and died. On post-mortem examination no cause could be found for the "stinking breath and spittle, for there was no collection of any filthy or stinking and putrid matter, nor any cavity in the lungs made by an ulcer or wound." One lobe, or rather the whole left side, was, however, formed of "stony hardness,"—had undergone fibroid degeneration.[1]

III. *Pulmonary Fetor with a Pyogenic Lung-Cavity* ("*Fetid Abscess,*" *Fibroid Vomica*).

In the clinical lecture already referred to, I called attention to the case of a man (Edgar) admitted into Ward I. of the Edinburgh Royal Infirmary, in May 1856. He was a carter, aged 66, and affirmed that up to six weeks before admission he had had good health. At that date he was affected with rigors, thirst, feverishness, and slight cough, and shortness of breath. He got thinner, and began to spit up a fetid spit with violent coughing. After admission he had recurrent attacks of pain in the chest, spitting of blood, and fetid expectoration, from which he recovered so much that sometimes there was little cough or fetid sputa. He gradually declined in health, however, getting more emaciated and cachectic (his complexion becoming remarkable sallow), until he died at last suddenly from atrophy and softening of the cerebellum, due to atheroma and clot of the left cerebellar artery. Dr Haldane made the post-mortem examination. The *right* lung was found to be œdematous; the *left* presented the characteristic changes of this form of fetid disease. It was adherent to the ribs posteriorly, where the adhesions were firmly cartilaginous and nearly an inch thick. The upper lobe was completely and firmly consolidated with an exudation of a simply fibrous character. No trace existed of cancerous or tubercular disease. In the centre of the affected lobe there was a cavity about the size of a walnut, containing a very fetid thick fluid, reddish in colour.[1] The physical signs corresponded very closely to the pulmonary lesion. In two points this case corresponds with my other cases of pulmonary fetor, classed with broncho-pneumonia, namely, in the periodic recurrence of hæmoptysis, fetid spit, pain and febrile disturbance, and in the physical signs, such as localized dulness on percussion, feeble breathing, increased vocal resonance. As to its anatomy and termination, however, it differs greatly. In these respects it belongs to that form of rheumatic consumption which I have already differentiated from the scrofulous and tubercular, and specially described.[2] This kind of lung-disease, with fetor, seems to be the same as that termed by Laennec "partial gangrene," and "gangrenous eschar," by Dr Stokes "local gangrenous disease," and generally by more recent writers "circumscribed gangrene" of the lungs. Bayle, I think, describes also this form of phthisis under the term "Ulcerous consumption." The cough from the first, he says, is accompanied by expectoration, and there is almost always a very fetid smell. The disease, he adds, may be distinguished from pleuritic abscess by the greater resonance of the chest on percussion. It is a rare form, for out of 900 cases of consumption, Bayle noted only fourteen of this "ulcerous" kind.

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[1] Willis' Works, translated by S. Pordage. Part II. Pharmac. Rationalis, sec. 1, chap. 8.

It is remarkable that the disease should be so constantly described as "gangrene" of the lung, but it is very obvious that the diagnosis of gangrene, even *post-mortem*, is often as vague as during life, for it is too often established solely from the presence of a fetid sanious fluid contained in a cavity in the lung, as in my case. Now, as the expectoration of such a fluid is not only no proof of a gangrene, but not even of a cavity, so the fetid fluid, which is only a peculiar kind of pus that has undergone a fermentation proper to it, is no proof of gangrene. It is only when putrid *tissue* is found in the cavity or in the sputum that we can certainly say that the morbid change is a gangrene.

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[1] This case is detailed at length in the report of my Clinical Lectures, Medical Times and Gazette, May 1857; more briefly in Appendix (Case IV.), p. 983.

[2] See "RHEUMATIC CONSUMPTION," in my Lectures on the Physiognomical Diagnosis of Disease, Medical Times and Gazette, 3d May 1862, p. 451.

These fetid pyogenic cavities or vomicæ in a solid fibroid lung may, however, be complicated with cavities constituted by dilated bronchi, or with a truly gangrenous cavern. The case of Dr Haldane, reported by Dr Gamgee, was of this latter kind; a "fetid abscess" was found to be associated with a gangrenous excavation. Andral relates a case of "gangrenous pneumonia," which was evidently a pyogenic cavity or "fetid abscess" in a fibroid lung conjoined with dilated bronchi. The patient, a young man, aged 21, had symptoms of pneumonia eighteen months previously to admission to La Charité; chest-symptoms continued until three weeks previously, when he had a profuse spitting of blood. On admission, febrile disturbance; great emaciation; breathing not perceptibly embarrassed; breath fetid; sputa very profuse, and of a very disagreeable odour. In three weeks a change was observed in the expectoration; it became still more copious and fetid, and contained small grayish clots, exhaling a very fetid odour; the breath also smelt horribly. He died suddenly. The left lung was found to be almost exclusively the seat of disease. There were firm pleural adhesions; the lung itself dry, firm, and resistant, showing, when cut into, an immense number of yellowish granulations and lines of fibrous tissue of a dull white colour, constituted of thickened cellular tissue, and marking out the pulmonary lobules. In the centre of the upper lobe there was an empty cavity, of the size of a large nut, emitting a "gangrenous" odour, and lined with a thin layer of greenish matter, beneath which was the pulmonary tissue, red and hard. Several considerable bronchi opened into it. The bronchial mucous membrane was red; the parietes of several bronchi were evidently hypertrophied, and in some parts dilated into small cavities, which contained a fetid fluid, like the sputa. These were most numerous in the inferior lobe of the lung (left).[1]

M. Briquet details a still more illustrative case of this kind in that of Joseph Von Malden, aged 64, professor of music. For three or four years before his admission into the Hospital Cochin, he had had recurrent exacerbations of a bronchitis which was "habitual," characterized by severe violent cough, pain in the lower part of the *left* side, and copious very fetid sputa of a reddish colour. Each attack passed off in a few days. When admitted suffering with the attack which proved fatal, he reported that in the first instance only a portion of the sputa expectorated was fetid, and he had little cough; but the symptoms gradually became more severe, and, in particular, the sputa were so acrid that they burnt his tongue. But as the tongue was gray and moist, the sensation was probably due to a hyperæsthesia of the organ. He experienced a dull pain in the lower part of the *left* side; and in the *left* postero-inferior region there was dulness on percussion, mucous râles, and bronchophony. He died eight days after admission. The *post-mortem* appearances were similar to those described in Andral's case. The two pleuræ at the lower lobe of the *left* lung were firmly adherent. In the centre of the lobe was a fetid cavity, the size of a fist, containing a fetid pultaceous stuff, more like altered blood than the detritus of tissue. It was lined with a dense false membrane, presenting spots like ecchymoses. The lung around it was indurated, and dilated bronchi opened into it, having their lining membrane thickened, and of a deep red colour. The right lung was strongly adherent at the apex; the bronchi, especially those of the upper lobe, dilated; the middle and lower lobes interspersed with gangrenous cavities, the size of a filbert, and communicating with bronchi.[1]

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[1] Andral: Clinique Medicale, tom. ii. p. 299, 64e Observation.

It is generally concluded that these cavities are the result of circumscribed gangrenous sloughing, and that the periodic attacks of febrile disturbance, pain in thorax, cough, and fetid expectoration, are obviously due to the accumulation of gangrenous fluid within them. There is not, however, any solid ground for this opinion. On the contrary, as I showed in 1837,[2] there is abundant reason to conclude that the fluid comes chiefly from the bronchi. In fact, the quantity is so greatly disproportionate to the size of the cavity, that all who have carefully examined these cases either come to this conclusion or express doubt as to the correctness of the current theory. Dr Stokes discusses this question with his usual clear insight, and in especial shows the difficulty of distinguishing in this respect between "localized gangrene" and "fetid abscess,"[3] which is the name given by pathological anatomists to these fetid pyogenic cavities.

In judging of these cases we must take into consideration that they are essentially chronic and really occupy a period of time extending over, at shortest, several months, and sometimes over many years; resembling phthisis in this and other respects, and governed as to their course by similar laws. In the case of Edgar there was a constitutional condition upon which was grafted a localized affection of the left lung. This was probably a lobar broncho-pneumonia in the first instance, like those just described, with pleurisy, the result of which was a cartilaginous degeneration of the pleura and a fibroid degeneration of the lung. We may look upon the fetid cavity, therefore, as consecutive to those fibroid degenerations, just as in ordinary phthisis a vomica is consecutive to tuberculization. And I think that when we find such cavities in a highly fibroid lung, we may reasonably conclude that they are formed like other ulcerous solutions of continuity of tissues in the state of fibroid degeneration, and are to be classed with rodent fibroid ulcers in the skin and cervix uteri or elsewhere. Hence the cavity is the result of rodent fibroid ulceration. In its lining pyogenic membrane there is a tissue analogous to that of the bronchial mucous membrane in cases of chronic bronchitis. Since the structural disease is constitutional, it is fair to conclude that the predisposing causes of the peculiar fetid excretion are constitutional, and are linked etiologically with the fibroid change of tissue. It is a special characteristic of fibroid and cartilaginous ulcerations to be fetid. This is well shown in ozæna, which is a deep ulceration of the fibrous and cartilaginous tissues of the nose, and also in rodent fibroid ulcers in general. The smell of ozæna is, in fact, very similar to that observed in examples of fetid fibroid vomicæ; and M. Lasègue correctly remarks the same of the fetor in fetid bronchitis. Yet it is not otherwise described by observers in general than as "gangrenous." The characters will doubtless differ in different cases, according to the complications,—in one there may be dilated bronchi; in another, a gangrenous cavern; in another, blood may be exuded and decompose in the cavity under the same kind of fermentation as that which modifies the pus and bronchial secretion. The smell might well be termed an ozæna-odour.

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[1] Archives Generales, May 1841, p. 9.
[2] London Medical Gazette, Dec. 1837, p. 459.
[3] Opere cit., p. 15.

The Nature and Origin of the Pulmonary Fetor in Fetid Bronchitis and Bronchorrhœa.

In the last group of cases I have endeavoured to show that the peculiar fetor is probably similar in origin to ozæna; but it is not like that odour in all cases; nor will the explanation apply to the fetor of acute and chronic bronchitis, or of bronchorrhœa, or broncho-pneumonia, for there is no proof of a rodent ulcerous cavity in these. Now, it is to be observed that the odour is *sui in generis* in them, as compared with other pulmonary fetors, and has therefore its own characters. 1. In its typical form, according to my experience, when it comes directly from the lung, it is always more or less fæcal. In the first case that I observed, the breath and sputa rendered a spacious ward odious as a privy, with the stench of fresh *fœces*. As to this fact there can be no mistake. Now this is not the odour of the breath in cases of fetid abscess or ozæna. 2. After the sputa has been exposed to the air for a while, however, it gives off an odour wholly different. In my first case I could only liken it to the delicate scent of a decaying apple.[1] In the case of Oliver S. (reported by my friend Dr Low of King's Lynn, together with my clinical remarks), the breath and sputa were noted as of an odour "rather fæcal than gangrenous;" while that of the sputa became like the scent of the apple or hawthorn blossom.[2]

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[1] See reprint of this case in the Appendix (Case I.).

[2] See abstract of case in Appendix (Case II.).

And in the case of C. A., the sputum is correctly described as "of a muco-purulent character, with a disagreeable fæcal odour, which is most intense when just expectorated, and which, on standing exposed to the air, becomes changed,—sometimes into a wet-mortar smell, but more generally into what simulates apple-blossom very closely."[1] Now, in these characteristics the odour of fetid bronchitis differs essentially from that of fetid abscess and of true pulmonary gangrene. In the case of pulmonary gangrene with which I compared my first example of fetid bronchitis, the breath was so horrible that I was made sick to vomiting when using the stethoscope. It was wholly unlike that of fæces. All writers speak of this dreadful smell in true pulmonary gangrene;[2] but none ever describe it as fæcal except M. Louis. He informed M. Valleix that he had observed exceptions to this horrible putrid smell in certain cases, in which the odour resembled that of fæces, although after death there was nothing more observed in the lung than what is usually found.[3] I suspect, however, that these cases were examples of pyogenic cavities, or dilated bronchi, and not gangrene; for it is probable these pyogenic membranes give forth the same purulent stuff as the bronchial mucous membrane in cases of fetid bronchitis. Dr Elliotson remarks incidentally, that in phthisis the expectoration sometimes smells "like fæces." I must express a doubt, however, that these were cases of *tubercular* phthisis, for I have never observed the *fæcal* odour in any such cases. Although fetid pus is not uncommon, pulmonary fetor, *sui generis*, is by no means a common symptom. I found it in only two of twelve hundred patients of the York County Hospital, and it can only be expected to occur in the rarer forms of consumption (using the term in its general sense), such as the rheumatic consumption I have described, and the bronchial or "pituitous consumption" of Gilchrist,[4] in which there is a copious purulent expectoration derived solely from the smaller bronchi, although the lungs themselves are flabby, or emphysematous, or œdematous.

It follows, from these facts, that there are three distinct kinds of pulmonary fetor, namely, that of fæces, that of ozæna, and that of gangrene; and it remains to ascertain how each arises.

The odour of pulmonary gangrene is obviously due to putrescent decomposition of the dead pulmonary tissue; it is the odour of putrefaction. Dr Law has expressed the opinion that the blood previously effused into the lung putrefies. This may be the case when the lung itself is gangrenous, but blood rarely thus putrefies *per se* —perhaps never—without the action of a septic "ferment." It is well known that putrid meat will quickly induce putrescence in fresh meat when brought near to it or in contact with it; and it is therefore conceivable that the effused blood in cases of pneumonia and pulmonary apoplexy of a low type may receive a septic "ferment" from without, and thus the putrid fermentation be set up in it. Or it is possible that some of the infusorial organisms which seem to be associated with hospital gangrene and other kinds of infectious putrescence may take effect on the blood or pulmonary tissue. In pulmonary mouldiness (the pneumono-mycosis of Virchow) a parasitic bronchitis seems to result from the irritation of an aspergillus in the bronchi. These have been found also surrounding the margin of a gangrenous cavity in the lung.[1] The pulmonary moulds are not unknown to phthisis, and it seems probable that the sporules may multiply in the bronchi, and be diffused by the breath of the patient, so as to excite a bronchitis in those persons who inhale them who have weak lungs. Hence, perhaps, the widespread belief, that it is not safe to sleep with a phthisical person, and that phthisis is a dangerously infectious complaint, as is the opinion in Italy. The putrescent infusoria, such as the vibrios of hospital gangrene and the bacterides of carbuncular gangrene, have not, I think, been looked for in cases of pulmonary gangrene. The bacterides may, however, be a cause of those kinds in which the gangrenous cavity has no contact with the external air and there is no fetor, inasmuch as they are abundantly present in the blood in carbuncular gangrene, and which sometimes affects the lungs.

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[1] See case in Appendix (Case III.), p. 981.

[2] Cruveilhier thus describes it, in a case he has delineated:—"Autour de son lit règne une atmosphère horriblement fétide, l'odeur de la macèration la plus infecte." And again:—"La colonne d'air qui sortait de sa poitrine était d'une fétidité intolèrable,"—even when the patient was only breathing.—Anatomie Pathologique, etc., liv. 3, pl. ii.

[3] "Il existait une *odeur tres forte de matières fécales*."—In Valleix, Guide du Médicin practicien, 1853, tom. i. p. 476.

[4] On the Use of Sea Voyages in Medicine. 2d Ed.

Since Laennec first mooted the opinion, the ozæna-odour in certain forms of bronchorrhœa and bronchitis has been attributed by several observers to a putrescence of the bronchial excretions. Granting this, the inquiry naturally follows, why they become putrescent in the particular class of cases described, amongst the many thousands who suffer from purulent bronchial secretion? Besides, the odour evolved is not at all like that of the fetid pus expectorated in cases of empyema, in which the cavity communicates with a bronchus, so that if the purulent bronchial excretion do undergo change, there must be something in its composition in these cases of a special character. That it does undergo decomposition seems certain; but it does not follow that the change is putrefaction. In ozæna from ulceration of the nasal cartilages, and in rodent fibroid ulcers, there is a most offensive discharge, but the odour is not like the smell of decomposing pus from ordinary ulcers. The pus of fibroid pyogenic cavities and of fetid bronchitis undergoes a similar change. We are therefore led to the conclusion that in all these cases there is some special constituent of the morbid products which either undergoes its own peculiar decomposition or acts as a ferment in exciting such in the other constituents. I do not think we need go beyond observed facts for instances as to this point. In so far as anatomical changes have been observed in these fetid pulmonary diseases, they are manifestly those of fibrinous exudation and degeneration,—are, in short, those which are observed in acute and chronic tissue-changes of rheumatic origin. Now, we know that in such changes lactic acid plays an important part, either as a *cause*, if we adopt the conclusions of Dr B. W. Richardson and others, or as a *result* of the morbid tissue-metamorphosis, if the ordinary theory be objected to. And it seems farther probable, according to the physiological chemists, that it is decomposed in the lungs, under ordinary circumstances, into carbonic acid and water. If, then, we suppose that there is an excess of lactic acid in the pulmonary excretions in these cases, or of some other acid of the class, or that the ordinary series of decompositions is interrupted and other products result in virtue of vital changes, an irritant is brought into direct contact with the bronchial mucous membrane. If this be sufficiently powerful, the same consequences will follow as with other powerful irritants (such as the vapours of iodine or chlorine), and thus a bronchitis with a copious muco-purulent discharge be excited. How easily lactic acid or acids or the lactates may be transformed within the capillary bronchi and air-cells, or elsewhere, wherever a suitable ferment is introduced or produced, is shown by the fact that when lactates are left in contact with putrefying matter, at a temperature of 86° to 95°, butyric acid is formed.

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[1] Dr Friederich, Virchow's Archiv, vol. x. (1856), p. 570; and Drs Von Dusch and Pagenstecker, ibid., vol. xi. p. 561.

That a rheumatic condition is an important element in these cases is shown by collateral evidence, although the morbid product need not necessarily be lactic acid, and certainly is not in all cases of pulmonary fetor. It is incidentally mentioned that in Dr Haldane's case tube-casts of the smaller bronchi were at first mistaken for disintegrating lung-tissue. Now these cases of "plastic bronchitis" run a course almost identical with that of cases of fetid broncho-pneumonia. There is the same frequency of hæmorrhage, violent cough, and other important symptoms, occurring in paroxysms, lasting for a week or two, but sometimes for several weeks, with the same tendency to recur for weeks or years. They also end in a very similar manner, either in a form of phthisis or in a dangerous pulmonary complication,[1] and they are about equally rare. In short, substitute for plastic expectoration of tube-casts a fetid muco-purulent expectoration, and the diseases would be identical,—so that the constitutional conditions are apparently the same. The difference is as to the susceptibility of the morbid pulmonary excretions and exudations to undergo decomposition. These cases of plastic bronchitis shade off also into that special kind of lobar broncho-pneumonia, closely resembling the fetid kind in symptoms and course, and occurring in rheumatic subjects, to which I have already referred. A case of this kind is now under my care in the Infirmary, in which the seat of lesion is (as is most common) in the left lung, with persistent pain there, and tingling in the palm of the left hand. The patient (a man) has had profuse strongly-smelling sweats, like those of the rheumatic, and rheumatic corpuscles in his urine.

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[1] Dr Peacock's paper in vol. v. of Trans. of Pathol. Society of London.

Fetor of the breath closely resembling that under consideration is observed in pregnancy, scorbutus, and cachectic states of a rheumatic character; in all these the blood is highly fibrinous and embolism not uncommon. Since the pulmonary artery has been found plugged in cases of pulmonary gangrene and rodent pulmonary ulcer simulating it, the plugging has been usually considered as the cause of the morbid change. There seems to be, however, just as solid ground for the opposite theory, namely, that the plugging is caused by the pulmonary disease. But, however caused, the excess of fibrin is probably a predisposing or exciting cause of the fetor.

These remarks refer chiefly to cases of pulmonary fetor in which changes of structure have been observed. But in one class of these affections there is no cognisable disease, and yet a very characteristic odour (the fæcal) is exhaled. But these cases, when examined, only serve to show more clearly its rheumatic origin. In offensive perspiration of the feet a peculiar cabbage-like stench is given off. It has been roundly asserted that this is due simply to want of cleanliness, just as the stench of fetid bronchitis has been attributed to gangrene; but the theory is certainly not sound. Cases occur in which the most scrupulous cleanliness fails to prevent the smell. Indeed, I know no more intractable malady, for even when the stockings and shoes are changed two or three times a-day and powerful scents used, the morbid stench is still diffused. It seems probable that the perspiration contains an element which undergoes decomposition at a low temperature or on exposure to the air. I have been informed that a surgeon in extensive practice in the north of England committed suicide a few years ago because of this offensive malady. A patient soon infects the air of a large room. In the cases that have come under my observation I could trace distinctly a rheumatic habit, and sometimes rheumatism was a complication. Allied to this is the offensive odour in some forms of rheumatic fever and in cases of acute recurrent attacks of leprous inflammation of the skin, in which the paroxysm terminates by a copious plastic exudation into the cutaneous cellular tissue. The urine sometimes becomes fetid in like manner. Dr Low, of King's Lynn, had communicated to me the particulars of a case of this kind in a pregnant female, and which ceased with delivery.

Speculations have naturally arisen as to the immediate origin and composition of the volatile matter which causes the stench; and as the blood is easily impregnated with volatile stuff, it is readily concluded that it comes from the blood. Perhaps it may be so as to some; but I think it equally probable as to others, that they first become what we find them on exposure to the air in the lungs at a blood-heat, or on the surface of the body, as in cases of fetid perspiration. The change in smell which the fetid sputa undergoes on exposure to the air shows how easily decomposition takes place. Possibly the fæcal odour may be an excretion, but if it were so in my first observed case, it did not originate in any so-called metastasis, for I noted the state of the fæces as to odour and observed no change. Pus from abscesses in the abdominal parietes has been found to be highly fæcal in smell, although the most careful examination failed to detect any connexion between the abscess and the intestines. I have no doubt that sulphur is a predominant, albeit not exclusive constituent of these fetid volatile changes. The odour itself is a sufficient proof, independently of distillations of sputa at a temperature of 230° Fahr.; and I may add that ordinary pulmonary vapour itself, if collected and kept a while, quickly becomes fetid,—even the condensed vapours on the windows of rooms crowded with people smells offensively although the highest temperature cannot exceed 90°. Dr Gamgee has, however, demonstrated, what was already plain enough to all acquainted with the facts, that we can get stinks from any pulmonary excretion whatever, if we subject them to the same chemical torments of distillation, acidification, neutralization, redistillation, and the like. This fact is no ground, however, for the conclusions, that there is nothing special in the odour of fetid bronchitis or its causes, and that we must abandon that term as perplexing, unscientific, and unphilosophical, for the more correct phrase "bronchitis with fetid expectoration." I have, however, subjoined reports of the chemical analysis of the sputum in two cases under my care.

The Treatment.

The treatment of these fetid cases is palliative and curative. The stench is of itself a dreadful infliction on the patient and all about him; then there are, commonly, violent cough and severe localized pain. All the terebinthinate drugs combined with henbane, belladonna, or opium, may be administered in the chronic bronchorrhœal forms with advantage; as chian turpentine, copaiba, the compound tincture of benzoin, in doses of m. xv., medicinal naphtha, creosote. Inhalations of turpentine, iodine, or chlorine may be used with advantage (but care is necessary), and volatilized iodine diffused through the room. As to the curative treatment, it must have a twofold object in regard to the nervous and rheumatic elements. I have for some time been convinced that in certain kinds of bronchitis and pneumonia the vagus system or the system of the pulmonary sympathetic is chiefly at fault in the first instance, in consequence of which there is defective tissue-metamorphosis and capillary action in the lung-tissue. In short, these diseases originate as neuroses of nutrition. This, I think, is more especially the case in the fetid forms under consideration, so that they belong (as I showed in my clinical lecture) to the same class of affections as diabetes mellitus, nervous fluxes, and other diseases of nutrition and transformation. This view is not weakened by the fact, that the morbid products belong to the rheumatic class, for I have long taught that the place of election of rheumatic disease is very commonly determined by states of the nerves and nerve-centres in connexion with the part affected. The change of morbid action from one part to another, known as *metastasis,* depends upon this action of the nerves and nerve-centres.[1] The left lung is constantly associated with the heart in certain kinds of rheumatic affections, and the remarkable preference of the left lung to the right in these rheumatic kinds of broncho-pneumonia seems to me to be due, in a certain proportion of cases, to the same cause as that which determines the heart-affection, namely, centric nervous action. Hence, in these fetid cases, I administer bitter drugs, as quinine, strychnine, and bitters generally, together with metallic tonics. As to the rheumatic element, the treatment should have reference both to the state of the blood and the tissues. Attention is now being directed more particularly to the nervous element in rheumatic cases; but the solids and fluids must not be forgotten, and the well-tried methods of treating acute and chronic rheumatism should be adapted to each case. I say nothing of the diagnosis and prognosis of fetid pulmonary affections, having already illustrated these very amply.

APPENDIX.

I subjoin a series of cases which have come under my observation and care, as an appendix to my notes on these pulmonary diseases. They illustrate all the more common forms, as met with in practice.

CASE I.—*Convulsive Cough, having first a Quotidian, then a Tertian Type; Sputa very copious, and of a highly fæcal odour. Polydipsia. Recovery.*

Sarah Battley, aged 20 years, wife of Hospital-Sergeant Battley, of the 10th Hussars, was attacked, in January 1837, with a pain under the right scapula, extending round the side to the anterior lateral region of the chest; it was aggravated when she coughed or breathed, catching suddenly. She was relieved by bleeding, and suffered from nothing but a slight cough until 17th April. She was then exposed to cold, and experienced some fatigue and anxiety. The pain now recurred and extended to the right mamma. She had daily pyrexial paroxysms, commencing about noon, and not terminating in sweat. Her cough also became more severe, the expectoration more profuse; and on the 23d she observed the sputa to have a very disagreeable smell, which gradually became more perceptible, and at last highly offensive. The thirst she had suffered from since the commencement of the attack now became excessive. She was admitted into the York County Hospital on 27th April. The following was her state:—She suckled her first child for twelve months, and weaned it six weeks ago; milk can be squeezed from the nipples. Complains of violent cough, attacking her in paroxysms, which continue from fifteen minutes to two hours, causing great pain in her head and accompanied with a profuse expectoration of a muco-purulent fluid, having a distinctly fæcal and highly offensive smell. Her bowels have not been moved for two days; she is pale, and has an anxious expression of countenance. Her friends suppose her to be in a *galloping consumption.* There is bronchophony in each upper and anterior region of the chest, most marked on the right; pulse 80, steady and feeble; tongue clean and moist; appetite impaired and fastidious; thirst excessive, the patient drinking two or three gallons of fluid every day; temper irritable and desponding.

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[1] See my Principles and Methods of Medical Observation and Research, second edition, p. 283.

Apply a blister to the upper part of the right side of the chest. To take a dose of calomel and colocynth extract immediately; and every four hours two tablespoonfuls of the following mixture:—℞. Infusi. digitalis, liq. ammon. acet. ā ā ℥iv., mist. camphoræ, ℥viij.—M.

27*th April, Evening.*—She is obliged to sit up in bed with her body slightly bent forward. Seeing her in this unusual position, and perceiving a strongly fæcal smell, I concluded the purgative was operating, and walked away. I was much surprised, on returning for the purpose of inspecting the evacuations, to be informed that her bowels had not been opened, and that the stench I perceived was caused by the breath and sputa.

28*th, Evening.*—Cough still severe, dyspnœa great; and if she attempt to lie down, is so much increased as to threaten suffocation. Ordered ℞. ext. hyoscyami, ext. conii., ā ā gr. iiss., pulv. ipecacu., pulv. scillæ, ā ā gr. ss.—M.; fiat pil. tales xij. To take a pill with each dose of the mixture, and to have dilute nitric acid for common drink to any extent.

29*th, Morning.*—Breath and sputa have still the fæcal odour; the sputa is of a whitish-gray colour, and muco-purulent consistence. Cough relieved; pulse 78, full and steady; tongue clean; thirst excessive,—to allay it she has a gallon pitcher at her bedside, full of water acidulated with nitric acid, of which she constantly drinks; bowels constipated. There is an enlarged and painful gland near the right sterno-clavicular articulation. To have two or three leeches applied to the inflamed gland. Let her continue her remedies, take ℈ij. of the compound powder of senna, and have a solution of chloride of lime about her bed to destroy the offensive smell. *Evening.*—Bowels have been moved once, the fæces of the normal colour and odour; tongue moist, and moderately clean; appetite improved; thirst diminished; pulse 96, round, incompressible; cough much less frequent; sputa in small quantity, and very slightly offensive. To have a senna draught.

30*th, Noon.*—Slept last night, but coughed incessantly, in paroxysms like hooping-cough, from six to half-past eight o'clock this morning, during which period she expectorated two pints and a half of a muco-purulent, dirty gray fluid, having a peculiarly offensive odour, like that of some kinds of discharges from the bowels. Breath not offensive; other symptoms unchanged. *Evening.*—Blister irritable, and ordered to be poulticed. Sputa have the odour of decayed apple, and are in small quantity; no fetor of the breath.

1*st May, Evening.*—Sputa of more fæcal odour, but cough and expectoration less.

2*d, Morning.*—Pain in the right anterior and upper portion of the chest; in the same region puerile respiration and pectoriloquy. On the opposite side puerile respiration and bronchophony; *no rattle on either side;* sputa and breath of a more fæcal odour; pulse 90, full and steady; tongue clean; appetite good; has slept tolerably well. *Evening.*—Has vomited to-day. Complains of being very weak, and sweats; pulse 104, small and soft; tongue moist and clean; fæces of the natural odour. Has coughed very much to-day, and the sputa are so offensive as to make the ward smell like a foul privy; her breath has a similar smell when she coughs; the sputa are copious, gray, muco-purulent, and swim on water. Cannot lie supine, or on her right side. Appetite moderately good; pulse 90, steady.

5th, Evening.—Better in every respect; pulse 114, soft; sputa much less offensive, and (as she says) of a sweet taste; breath offensive; pectoriloquy on the right side; bronchophony on the left.

6th, Evening.—Has had a very severe fit of coughing to-day, with a copious and very offensive expectoration; pulse 108, tongue clean. Left the hospital. I learned a few weeks after this date that she soon and rapidly recovered, and joined her husband at Nottingham in good health and spirits. The tertian type which the disease assumed (a peculiarity I noticed only when copying the case for the press) is not less remarkable than the quantity and fetor of the expectorated matter.—*From the London Medical Gazette for* 16*th December* 1837.[1]

CASE II.—*Broncho-pneumonia of Left Lung; Recurrent Hæmoptysis; Fetid Breath and copious Fetid Expectoration; Polydipsia. Recovery.* (Reported by Dr Low of King's Lynn.)

Oliver S., aged 37, single, residing in the Canongate, admitted, under Dr Laycock, into Ward I. of the Royal Infirmary on 17th February 1857.

Patient is 5 ft. 6 in. in height, well formed and tolerably robust. Has the appearance of having been very stout, but the muscular system is now flabby. Diathesis, lymphatic. Hair dark; features broad and massive; forehead prominent; conjunctivæ anæmic; eyes grey; nose short and thick, alæ nasi expanded; malar bones not prominent; upper lip tumid; mucous membrane of lips and gums pale; teeth small and regular, enamel good. Voice hoarse and whispering; breath gives off a peculiarly fetid odour. Sternal end of left clavicle is higher than the right; manubrium sterni depressed. On left side there is a prominence of the third and fourth ribs at their junction with the cartilages. Abdomen rather large and flabby.

History.—Is one of a large family, seven of whom, viz., four brothers and three sisters, are dead. Does not know of what diseases they died. Patient states that in his youth he was very healthy and temperate. Until the age of twenty-five years he followed the occupations of a tailor and a hawker, which he relinquished at this time for that of a beershop-keeper. For five years subsequently he continued well, and though indulging occasionally in liquor was not, he considers, on the whole intemperate. Being unfortunate in this line of business, he was compelled to sell his house and resume his prior occupation of hawking. Owing to the depression resulting from his misfortunes, he became very intemperate, and five years ago had a severe attack of delirium tremens, for the treatment of which he became an inmate of the Infirmary. After remaining there a month, he was discharged, and he returned at once to his habits of dissipation. Twice subsequently, while in a state of intoxication, he received injuries on the chest, which caused the alterations in its form above noticed. In other respects he continued well until a year since, when he had a second attack of delirium tremens, and was again an inmate of the Infirmary for eight days.

The present illness commenced three months ago, after exposure to severe cold and wet. The first symptom noticed was a troublesome cough, which, however, was unattended by pain or expectoration. This continued until two months ago, when, after repeated exposures to cold and wet, it became more urgent, though still without pain, and with only slight expectoration. Between three weeks and a month before admission, the cough increased in violence; there was severe pain in the left side, and the sputa were streaked with blood. He noticed now, for the first time, that his breath was very offensive. Since that time the cough has continued unabated; the pain in his side is much increased, and the sputa have been occasionally tinged with blood.

He has not been under medical treatment. Has had no feverishness or thirst from the commencement of the present attack, until four days ago. States that he has lived well during the past seven years. Feels tolerably well except as to the cough.

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[1] The notes of the physical signs are not to be considered as entirely accurate; physical diagnosis was only just coming into general use at this date.

Examination on admission.

Respiratory System.—Thoracic expansion is somewhat restricted. The sternal end of the left clavicle is dislocated; manubrium sterni depressed, and the prominence of third and fourth ribs appears to have resulted from an old fracture.

On percussion, anteriorly, the right side of the chest is resonant, as also the upper two-thirds of the left side; the lower third is dull both anteriorly and laterally. On auscultation over right side, inspiration is found to be harsh, expiration prolonged. On the left side, inspiration is sibilant, expiration prolonged, and attended by fine moist crepitation superiorly, but over lower third by loud snoring. Posteriorly, percussion is normal. The respiratory sounds are slightly exaggerated on both sides, and at the base of left lung there is fine crepitus, with expiration. The cough is very troublesome; sputa copious (about a pint in twenty-four hours), muco-purulent, viscid and fetid, but much less so than the breath; some of the masses are tinged with blood. No lung-substance is observable under the microscope; but there are abundant pus-globules.

Circulatory System.—Cardiac dulness two and a half inches transversely at nipple; impulse felt between fifth and sixth ribs. There is a slight blowing murmur at the close of systole, heard at the apex. Pulse 68, full and firm.

Tongue furred and moist. Patient complains of great thirst. There is no hepatic or splenic enlargement observable on percussion. Bowels open. Urine, sp. gr. 1·032; deposit a copious sediment of urate of ammonia and purpurates. Chlorides abundant.

Patient was ordered m. x. of medicinal naphtha, and m. xv. of liquor of muriate of morphia thrice daily. On the 20th February, he complained much of pain in the lower portion of left side; the thirst was excessive; sputa more copious. A blister relieved the pain. On the 23d, crepitus had disappeared from left side, the sputa had increased to two pints (40 oz.) in the twenty-four hours, and exhaled an odour resembling that of the hawthorn blossom. Breath still very fetid; smell rather fæcal than gangrenous. On the 25th, there was a return of hæmoptysis; the sputa were deeply covered with blood. Patient had no pain, and expressed himself as feeling well in other respects, but the snoring and crepitus were again heard in left base of thorax. Pulse 60, very feeble. Appetite good; thirst less. Was ordered the tincture of the sesquichloride of iron. On 5th March, began to take gr. $\frac{1}{30}$ of strychnia thrice daily, which was increased on the 9th to gr. $\frac{1}{20}$. Next day, slight return of hæmoptysis, and the cough very urgent, but no pain felt. For several days up to the 2d April, the sputa continued slightly tinged with blood, but rapidly diminished in quantity to 1 oz. daily, and then entirely ceased. On the 19th March, the fragrant scent had entirely disappeared, and the breath had almost lost its fetor.

An examination of the fetid sputa was made at the laboratory of the University, under the superintendence of Professor Gregory, and the subjoined report sent.

Examination of Sputa.—A pint of this fluid was diluted with distilled water, acidulated with sulphuric acid, and then subjected to distillation in a sand-bath. A second pint was treated in like manner, and the products of the two operations, amounting to four ounces, were mixed together. This liquid was distinctly acid, and had a somewhat fetid odour. After being neutralized by carbonate of potash, it was evaporated to dryness. The residuum, which was crystalline, after treatment with alcohol and sulphuric acid, gave off a decided odour of acetic ether, and this after standing for a time was replaced by one of pine-apple.

The original *acids*, therefore, so far as can be judged by the odour alone, appear to have been acetic, with a trace of butyric and a faint trace, probably, of propylic acid.

The residuum left in the retort, after the first of the above operations, was rendered alkaline by carb. potash. Two ounces of an alkaline fluid were distilled off, and after being neutralized by dilute sulphuric acid, evaporated to dryness. The crystalline residuum, was treated with caustic potash, when a peculiar odour, betwixt that of putrid urine and decomposing fish, was evolved.

The residue of the second pint of sputa, after the first distillation, was next made alkaline by caustic potash, and again subjected to distillation, the vapour being received into water which was slightly acidulated with hydrochloric acid. The product, after being evaporated to dryness, was treated with absolute alcohol at a boiling heat. The alcohol was then poured off, filtered and evaporated. The residuum on the addition of caustic potash gave off a strong odour, which Dr Gregory recognised as that of methylamine.

CASE III.—*Recurrent Broncho-Pneumonia of Left Lung, slight recurrent Hæmoptysis, Fetid Breath and Sputum.* (Reported by Mr Wickwire and Mr Thompson.)

Catherine A., aged 27, married, resident in the Canongate, Edinburgh. Admitted October 12, 1863, under Dr Laycock, into Ward XI., Edinburgh Royal Infirmary.

History.—Patient states that she was quite well until three weeks ago, when one day she had a shivering, which was followed the next day by a severe pain in the left side, which came on suddenly while going up stairs. This pain increased in intensity, and mustard poultices were applied, which did not afford any relief. Four leeches were afterwards applied, which eased the pain greatly; but it has never entirely disappeared. Patient states that she has had a little cough ever since she became ill, which, however, was not bad enough to cause her much trouble; but two or three days ago, she was seized with a violent fit of coughing, and expectorated more than a pint of matter, which, for the first time, she observed had a peculiar disagreeable odour. She has had these coughing fits ever since, several times during the day, at intervals of four or five hours.

State on Admission.—Respiratory System: On *percussion*, there is good resonance all over the chest *anteriorly; posteriorly* there is dulness over lower one-fourth of both lungs. On *auscultation*, there is feeble breathing at base of left lung *anteriorly; posteriorly*, in *left supra-scapular region*, breathing is harsh: in *scapular region*, inspiration is coarse; expiration prolonged; over lower fourth of *left side* there is very feeble, indistinct breathing, and distant crepitation; vocal resonance increased: in *right supra-scapular region*, breathing coarse; vocal resonance increased: in *scapular region* the same. At the *base* there is feeble breathing, with occasional crepitation.

The patient expectorates profusely, but not frequently. The sputum is of a muco-purulent character, with a disagreeable fæcal odour, which is most intense when the sputum is first expectorated, and which, on its standing exposed to the air, becomes changed, sometimes into a wet-mortar smell, but more generally into what simulates apple-blossom very closely. Tongue covered with a white fur; appetite poor. Systole loud, and prolonged at apex; no murmur. Catamenia regular; no albumen in urine. Dull gnawing pain in left postero-inferior region of chest; sleeps badly; otherwise normal.

13*th Oct.*—Was to-day ordered—℞ Sodii chloridi, ℥ss.; tr. ferri mur. ʒi.; mist. camph. ℥xij.—M. Cochlearia dua ampla tertiis horis.

17*th.*—Pharynx much congested. Has spat up several mouthfuls of blood during the night, probably from the throat. Pulse 112. There was to-day found well marked signs of consolidation of apex of right lung and base of left. Was ordered—℞. Naphthæ med. ʒiij.; syrupi papaveris, ʒvj.; mistura camph. ℥xj.—M. Two tablespoonfuls three or four times a-day.

18*th.*—Pulse 120. Moderate volume, and soft. Slept better last night than for five weeks previously. Has not had a fit of coughing since mid-day yesterday.

19*th.*—On *auscultation*, *anteriorly* on *right* side, there is at *apex*, loud, harsh, vesicular breathing. In *mammary region*, there is loud vesicular breathing, with sibilus at the end of inspiration; vocal resonance increased. In *supra-mammary region*, the same. In *axillary region*, the sounds are less marked; in *lateral* they are less marked still. In *left subclavian region*, bronchial breathing and bronchophony. In *mammary region*, harsh inspiratory murmur; resonance natural. There is great pain, on percussion, in left supra-mammary and lateral regions. *Posteriorly*, on *percussion*, there is good resonance all down the right side; dulness all down the left side, particularly in lateral region. On *auscultation*, there is coarse inspiration; no moist sounds; and vocal resonance increased in *right supra-scapular region.* In *left supra-scapular region*, there is vesicular breathing, with occasional sibilations at the end of inspiration; vocal resonance increased.

20*th.*—Patient has not had a fit of coughing since last night at 5 o'clock. Has spat up very little in the meantime. The sputum which is expectorated ordinarily possesses little or none of disagreeable odour.

23*d.*—Patient thinks that she is rapidly improving. Has not had a fit of violent coughing since the evening of the 19th.

24*th.*—Has not had an attack of coughing since last report. On *auscultation*, *anteriorly*, there is good vesicular breathing; no moist sounds; vocal resonance not increased. *Posteriorly*, on *right side*, the breathing is good, loud and vesicular. In *left supra-scapular region*, there is a slight blowing; no moist sounds. At the angle of scapula on same side, there is medium crepitation, with inspiration; vocal resonance increased. There is fine crepitation in *postero-lateral region*, diminishing as we pass down the back.

26*th.*—Had slight paroxysm of cough yesterday, and brought up some bad-smelling sputum—the first since the 19th. Previous mixture to be discontinued, and the following instead:—℞. Quiniæ sulphatis, ʒss.; acidi sulph. dil. ʒjss.; liq. morph. mur. ʒij.; infusi rosæ acid; mistur. camph. āā f. ℥vi.—M.; ft. mist. Two tablespoonfuls every three or four hours.

28*th.*—Had slight hæmoptysis this morning.

11*th Nov.*—Since last report patient has made great progress. The expectoration is very slight now, and has not the offensive odour it had when she came in; neither is there any blood in it. Patient is up greater part of the day, and does not feel fatigued with the exertion. With the exception of complaining of a slight pain in left base, posteriorly, nothing happened till the 22d, when a feverish attack began; and on the 26th she expectorated dirty, brownish-gray sputum, which has a strong odour, and which, when inhaled, *diluted*, somewhat resembled apple-blossom. The pain at left base still continued.

3*d Dec.*—On *percussion*, some dulness *in front*, on *left* side, from fourth rib to base of chest. *Behind*, dulness more marked, from spine of scapula down. On *auscultation*, behind, there is fine crepitation, with vesicular respiration, over region of dulness. In front, the same, but less distinctly heard; respiration, generally, not loud on left side; right side normal.

On 6*th Dec.* patient was exposed to cold, and had a rigor.

On the morning of the 7*th*, the cough became troublesome, the sputum was copious and like coffee in appearance, but had no smell. About 11 P.M. the coffee-like appearance ceased, and it became muco-purulent and nummular.

On the evening of the 9*th*, patient got up, and sat by the fire for about three hours. On going to bed the pain in left side returned; and along with it the sputum began to have a more purulent appearance, and the appleblossom-smell returned. A mustard poultice was applied, which relieved the pain.

When she awoke on the morning of the 10*th*, she felt very sick. Shortly after waking she got a cup of coffee, which was rejected in about ten minutes. The sickness continued, and about 11 o'clock she tried to take some breakfast, which was immediately rejected. After the first vomiting, the sputum ceased to have the characteristic smell, and what has been spat up since is odourless. Patient feels a "girding pain," or feeling of tightness all round the lower part of the thorax, which is increased when a long breath is taken. It is also increased on pressure. Previous mixture to be discontinued, and the following instead:—℞. Potass. iodid. ʒss.; liq. morph. mur. ʒjss.; inf. quassiæ, āā ℥viij.—M.; ft. mist. Two tablespoonfuls three times daily. Also, ℥iv. port wine daily.

On 11*th*, the smell of sputum was scarcely perceptible.

On 13*th*, while sitting by the fire, the patient shivered. She then went to bed, and coughed a great deal. The sputum was very copious, and very offensive. It was of a dirty-gray colour, tenacious, and in flocculent masses.

On 20*th* and 21*st*, had slight hæmoptysis. Was ordered to return to the quinia mixture; to take gutt. xx. ter die vin. ipecac., and to discontinue the iodide of potassium mixture.

Patient continued much in the same state, the expectoration being diminished in quantity, until the 4th January 1864, when, on account of her husband having been killed, she left the hospital.

Dr Gamgee kindly undertook a chemical examination of some of the sputa, and reported to me as subjoined:—

"A few days since I received from Dr Duckworth a stoppered bottle, containing the sputum of a patient in Ward XI. It had a very fetid odour, resembling very closely the smell of rotten cabbage. Although the single analysis which I have made does not enable me to give a very full account of the chemical composition of the fluid, I think the results which I have obtained may not prove uninteresting. The sputum had an alkaline reaction. It was placed in a retort joined to a Liebig's condenser, and the temperature was raised, by means of a chloride-of-calcium bath, to 230° Fahr. By this means a distillate was obtained, which possessed in a concentrated form the smell of the sputum. It had a very decidedly alkaline reaction. After the distillation had been carried on for some time, the substance in the retort had lost all its disagreeable smell. A little water was then added, and then some sulphuric acid, until a strong acid reaction was produced. The retort was then heated to 270°—280° Fahr., when a fluid having a full acid reaction, and a decided odour of butyric acid, distilled over. I compared the fluid with some pure butyric acid which I prepared. The resemblance of smell was very decided. In the distillation from the sputum there was, however, a slight odour superadded to that of butyric acid. In order to obtain the pure acid, I neutralized a portion of the acid distillate with carbonate of soda, and concentrated it. I then added an excess of sulphuric acid, and redistilled, when a fluid having exactly the odour of weak butyric acid was obtained.

"The only acids which could have been obtained from the sputum by the process which I adopted are, as far as I am aware, butyric acid, acetic acid, formic acid, proponic acid. With regard to acetic acid and formic acid, I can state positively that they were not present. The distillate must therefore have contained butyric or proponic acid, or both. The positive evidence is almost conclusive as to the presence of the former.

"What is the composition of the highly fetid volatile substance which had an alkaline reaction? I distilled some of it with sulphuric acid, but found that it passed over unchanged, and still with an alkaline reaction. It is therefore not a butyrate. The only other fact with which I am acquainted with regard to it is, that it is, as one would expect, a sulphur compound.—*Dec.* 24, 1863."

CASE IV.—*Fetid Bronchitis or Broncho-pneumonia; Recurrent Hæmoptysis; Polydipsia; Death; Fibroid Degeneration of Left Lung, with Fetid Pyogenic Cavity; Arterial Atheroma; Atrophy and Softening of the Left Lobe of the Cerebellum from Emboli.*

John Edgar, 66, single, a carter, admitted under my care into the Royal Infirmary, May 28, 1856. The patient enjoyed good health up to the time of present attack, which commenced six weeks ago, with rigors and slight dyspnœa, followed by thirst, feverishness, and cough. Subsequently he lost flesh; the cough became more violent, and was attended by copious expectoration of fetid matter.

On admission, a bulging was found over the cardiac region. Percussion sounds rather flatter over left apex than right, anteriorly; otherwise normal; at the same point respiration is exaggerated; expiration prolonged. Posteriorly mucous râles are heard at left base and over middle third, on forced inspiration. Expectoration abundant, partly purulent, with very offensive odour. Over the base of the heart a murmur is audible with the diastole; it is heard also at xyphoid cartilage and second right costal cartilage, but faintly at the apex. The arteries at the wrist are very tortuous; the pulsation of the arteries in both arms and forearms, as well as of both carotids, can be distinctly perceived. Pulse 88.

4th June.—Patient has not improved much. Complains of thirst, and a little pain in left infra-mammary region. Expectoration more abundant and purulent. Skin hot and dry. *11th.*—"A little improvement;" appetite good; skin cool, that covering the face of a yellow tint; abundant moist râles over whole of left side, posteriorly; vocal resonance increased; percussion equal on both sides. *24th.*—The odour of breath and sputa less offensive; the sputa less abundant, still muco-purulent; appetite much improved. *29th.*—Coughed up a teaspoonful of florid blood; small quantities continued to be expectorated during the day; sputa frothy. *30th.*—The fetor of sputa is gone to-day, and no odour is perceptible in the breath. Dulness on percussion over left apex, anteriorly, extends down to second rib; cardiac dulness cannot be ascertained, that portion of the chest being as resonant as elsewhere; a murmur with the first sound is audible at the apex; also at base over sternum and under both clavicles. On percussion over left lung, posteriorly, the upper two-thirds are found to be duller than on the right side; lower third is resonant; the colour of the face is less sallow, and patient expresses himself as somewhat stronger.

15th July.—The dulness on the left side extends below the nipple, anteriorly and laterally. Respiration over the dull region is tubular; towards the lower part it is faint, and inspiration is attended by a sub-crepitant râle. At the base, friction sounds are audible. Vocal resonance muffled. The sputa abundant; the upper part is frothy and white; in one or two places fawn-coloured; somewhat fetid; the lower part is muco-purulent and tenacious. Second cardiac sound is rough and prolonged; over the sternum both sounds have a metallic character. Pulse 92, full and regular. Patient does not think himself in any way worse, except as regards the cough; skin has a more decidedly icteric tint since last report; conjunctivæ slightly yellow.

21st.—No change in physical signs, except that a cracked-pot sound is elicited, on percussion, over second and third ribs on the left side. Sputa retain their fetor, which is of a fæcal character. Patient is gradually getting weaker, though he says there is no change. The yellow tinge of the skin has been diminishing for a few days past, and the lips have acquired an anæmic paleness. He complained in the evening of pain in the left chest, which a blister relieved.

On the 27*th July*, a month from last date, blood again appeared with the sputa, and on 7*th August* it came up in mouthfuls, of a bright red colour; heart's action was increased, and a diastolic murmur was heard at the base.

8th August.—Got up to stool this morning about 7 o'clock, and was found unable to get back again; was lifted into bed; pulse by no means very weak, 84 per minute. He said he had no pain, and could not tell what was wrong. Had some ammonia and brandy administered to him, but did not improve. In the evening, seemed to be quite sensible, but could not speak distinctly. Pulse full, 96 per minute.

9th.—The pulse somewhat weaker, 104 per minute. At nine in the morning, was apparently sensible, but could not articulate. His tongue lay to the right side in his mouth, but could be moved about easily when he tried. Pulse getting weaker and rather quicker. About five P.M., had a convulsive fit, in which his right side was alone affected, the arm and leg violently, and the mouth being drawn to the right side, without foaming. He had six similar ones before half-past eight P.M., in all of which the right side was most affected; but in the last the left was considerably affected also. About nine P.M., he had one, in which the left arm and leg were violently convulsed, and the right hand, but not above the elbow. The mouth was at first drawn to the left, but, during the fit, changed to the right, and continued so until the fit ended. The pulse was almost gone, and he seemed nearly asphyxiated; but whenever the convulsions ceased the pulse began to gain in power, and very soon was nearly at its former strength, and 104 per minute. He had four other fits before midnight, at which time (being unable to swallow) he had brandy and an enema of beef-tea administered. From that time till nine next morning, 10th August, he had sixteen other fits. This morning he lies on his back, breathing with some difficulty. Pulse 120, weaker. At ten A.M., he had a final fit, a very violent one, in which the whole body was convulsed. After this he lay on his back, breathing with difficulty and stertorously, until about 5.30 P.M., when he died quietly.

Sectio Cadaveris, by Dr Haldane, forty-five hours after death.—The body was not emaciated to any great extent. The skin of the face was of a dusky yellowish colour, which did not, however, extend to the integument of any other part of the body. On removing the skull-cap the brain was seen to present a very uniform smooth appearance, owing to an effusion, partly serous, partly gelatinous, on the surface of the hemispheres. The brain itself was somewhat œdematous, and very soft; the lateral ventricles were rather enlarged, and contained about an ounce of fluid. The arteries at the base of the brain were very atheromatous, especially the right middle cerebral and the left inferior cerebellar, which last was completely occluded about an inch from its origin. The left lobe of the cerebellum was both softened and atrophied, and, under the microscope, was seen to be crowded with exudation-corpuscles. The *pericardium* contained a good deal of serous fluid. The *heart* itself was quite healthy, with the exception of a slight incompetency of the aortic valves, caused by a swelling, about the size of a pea, between two of them. The aorta was dilated and rough immediately above the valves, and was, to a slight degree, atheromatous. The *left lung* was adherent to the ribs, especially posteriorly, where the adhesions were quite cartilaginous, and nearly an inch thick. The upper lobe was completely consolidated, with an exudation of a simply fibrous character. No trace existed of either cancerous or tubercular deposit. In the centre of the lung there was a fetid, disintegrating cavity, about the size of a walnut. The *right lung* was very œdematous, especially in the upper lobe, with some pneumonic consolidation and a few emphysematous patches along the anterior border. The *liver* was normal; the *gall-bladder* elongated, with an hour-glass contraction in the middle. *Kidneys* contained a few cysts. *Supra-renal capsules* rather larger than natural, but normal in structure. *Spleen* normal. *Testes* the same. All the arteries in the body, as far as they were examined, presented here and there patches of atheromatous deposits.

CASE V.—*Pneumonia; Pultaceous, Grey, and excessively Fetid Expectoration. Death in twenty-eight days. Gangrene of the Left Lung.*

John Elder, aged 45 years, was admitted into the York County Hospital on the 27th September 1837, with symptoms of pneumonia. He had severe pain in the left side of the chest (as I was informed), violent cough, and gelatinous adhesive expectoration. On the 2d of October, the expectoration suddenly increased in quantity, and became decidedly puriform. On the 5th, there were pectoriloquy and a distinct blowing, as of air into a cavity. The breath and sputa exhaled a highly disagreeable fetor, so that I was nauseated while using the stethoscope; the strength was prostrated, and the patient constantly demanded nourishing diet, which he loathed when offered him.

On the 16th October, all the symptoms, which had gradually increased in severity, became much aggravated; the sputa were very copious, of a yellowish grey colour, and excessively offensive. Amphoric resonance could be heard over the whole chest, but very loudly over the second and third ribs. The moment the stethoscope touched the ear, and before it was adjusted, the sound could be *felt*. His strength now rapidly failed. On the 22d, he lay supine, a grey, pultaceous, and horribly stinking fluid flowing from his mouth. On the 24th, he died.

Examination of the Body.—While cutting through the cartilages and intercostal muscles of the ribs mentioned, eighteen or nineteen hours after death, the knife slipped into a cavity in the left lung, and let out a quantity of abominably stinking gas. Raising the sternum, this cavity was found to occupy nearly the whole of the lung. Anteriorly, it was bounded by the ribs and softened intercostal muscles; a portion of the third rib near its cartilage was denuded of its periosteum. The cavity was half-filled by a grey, pulpy, stinking fluid, resembling that expectorated; the lung was of a similar colour, was for the most part softened to the consistence of pap, and when sliced where most solid, the cut surface appeared grey, glairy, and homogeneous. The right lung was dotted with black spots, but in other respects, excepting a few small tubercles, was healthy.

Laennec and Andral give seven cases of gangrene of the lung; in five the left lung was affected, as it was in this case.—*From the London Medical Gazette*, 16*th December* 1837.

CASE VI.—*Gangrenous Bronchitis (?) and Gangrene of Trachea simulating Pulmonary Gangrene. Tuberculosis. Death.*

— Y., aged about 40, was admitted under my care into Ward I. of the Edinburgh Royal Infirmary, on 14th December 1860. He was extremely prostrated, and coughed much, bringing up a copious muco-purulent greyish sputum. On left side (chiefly), resistant dulness on percussion, tubular whiffling, and cavernous flapping; these signs, together with the character of the sputum, led to the diagnosis that there was a gangrenous cavern in the left lung. His extreme weakness rendered physical examination unadvisable, and he shortly died.

Examination of the body, by Dr Haldane.—The heart and pericardium were healthy. The lungs were voluminous, more especially the left, which was also very dense. They had a knotty feel, which, on inspection, was found to depend upon small masses of tubercle scattered through their substance. Some had softened and constituted small vomicæ, but there was no cavity of any considerable size. The lung tissue was otherwise natural. On examining the bronchi, their lining membrane was found to be of a dirty brownish colour, and to be coated with mucus having a very offensive, almost gangrenous, odour. On examining the neck an abscess filled with most offensive purulent matter was found deep down on the right side. It communicated with an irregular opening in the trachea, about an inch in length. The edges of this opening were in a gangrenous condition, and portions of five or six rings were quite gone, while the ends remaining were left bare. The lining membrane of the trachea both above and below the opening was of a dark brownish-red colour, and was coated with a muco-purulent fluid which had a characteristically gangrenous odour. The larynx was quite healthy. A few scattered masses of tubercle were found in the mucous membrane of the small intestine.

This case is interesting as an example of gangrene of the air-passages. It is also instructive as to another source of fallacy in the diagnosis of pulmonary gangrene, for the flapping tracheal sounds were evidently propagated along the bronchi to the voluminous dense lung, compressed closely against the thoracic wall, and so the signs of a gangrenous cavern were induced; while the bronchial exudation was of a putrescent odour, and the discharges from the necrosed trachea gangrenous.

4 RUTLAND STREET, EDINBURGH, 6*th April* 1865.

ARTICLE II.—*Case of an Unusual Form of Nervous Disease, Dystaxia or Pseudo-Paralysis Agitans, with Remarks.* By WILLIAM R. SANDERS, M.D., F.R.C.P.E., Physician, and Lecturer on Clinical Medicine at the Royal Infirmary of Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 5th April* 1865.)

THE case which I bring before the Society is characterized by a singular disorder of the voluntary movements. When the patient is sitting tranquil and at rest, nothing peculiar, or at most a little unsteadiness, is observed; but so soon as he becomes excited, or when he performs any muscular action, immediately violent shaking movements take place, occurring usually first in the limbs, and rapidly extending to the whole body, which is thrown into extraordinary commotion. The head keeps nodding rapidly or shaking from side to side; the trunk is agitated; and the arms and legs are violently shaken to and fro. These rhythmical spasmodic movements are entirely beyond the patient's control; they disable him from walking or even standing; he cannot use his hands to feed himself, to wash his face, or put on his clothes; he is completely incapacitated for any employment. The shaking movements soon subside when the patient is at rest, and they disappear during sleep; but any exertion, even turning in bed, is sufficient to renew them. The particulars of the case, derived from the reports of Dr James Carmichael and Mr Keith Anderson, who have acted as resident physicians in my wards in the Royal Infirmary, are as follow:—

John Conolly, aged 35, a bricklayer's labourer, was admitted to the Edinburgh Royal Infirmary on 7th October 1864.

The patient is of middle height, of rather spare habit, not anæmic nor cachectic, nor presenting anything remarkable in his external appearance. He is a native of Ireland, but since 1847 has resided chiefly in Yorkshire. He has usually enjoyed good health, but about fourteen years ago he suffered from ague every spring for several seasons. He reports also an attack of rheumatism nine years ago, and states that the ague ceased entirely in the spring following his recovery from the rheumatism. Since that period till the commencement of his present ailments his health had been excellent. The nervous disorder under which he now labours is traceable to an accident which he met with about a year before admission to the Infirmary. By the giving way of a scaffolding on which he was working, he was suddenly thrown down to the ground from a height of twelve feet, and some planks of wood and bricks, falling upon him, injured him severely, causing fracture of the right leg and wounds of the left knee, of the back near the fourth dorsal vertebra, and of the head, attended with considerable loss of blood. Between four and five months elapsed before he recovered sufficiently to resume his employment. At this period no shaking movements had occurred; and, on careful investigation, it appears evident that the injuries then sustained in the head and spine were confined to the soft and superficial parts, and did not involve the bones nor the contents of the cranium or spine. After recovery from these injuries he had returned to work for eleven days, when unfortunately the planks slipped on which he was walking, and he fell down on his back upon them. He received no injury by this fall, but he states that he experienced a "great fright." He describes that, while lying on the planks, and before assistance reached him, he suddenly commenced to shake; his right arm flew upwards involuntarily, and began beating the top of his head, and all his limbs shook, so that the bystanders thought he was in a fit. He was perfectly conscious at the time, and aware of his condition, but he was unable by any effort to control the commotions of his limbs. The liability to these shaking movements, interfering with voluntary action, has continued ever since; they have sometimes been very violent, at other times they have been almost completely cured; but, on the whole, for some months before admission they had increased in extent and in the frequency of their occurrence. The limbs and trunk were first affected; since last July the muscles of the neck began to jerk, causing the nodding and shaking of the head, and quite recently the speech has been interfered with by the irregular action of the muscles of respiration.

He was under treatment for his injuries in the Newcastle and Hull Infirmaries, and afterwards came to Edinburgh for further advice. Here the disordered movements becoming much worse, he applied for admission to the Royal Infirmary.

In regard to his medical history, it has been ascertained that he comes of a healthy family, that he never took mercury, that he has never worked with mercury, lead, arsenic, or any of the metals, and that he never was addicted to intoxicating drinks, nor to venereal excesses.

Report on his condition, chiefly as he appeared on admission.—The marked symptom, as already mentioned, which the patient presents, is the tendency of the body and limbs to shake violently under mental excitement or voluntary effort.

Recumbent Posture.—When lying flat in bed, as he was first seen at visit, he was quite still, some twitching of the adductors of the thigh was the only symptom observed; but so soon as he made any movement, the shaking at once began. When he raised and extended the arms, they were agitated violently and rapidly with a lateral to-and-fro motion, and if brought near the chest or head they beat against these parts, inflicting blows which the patient was unable to prevent. Both arms, when extended forwards, shook synchronously, and if sufficiently near, the hands were dashed to and fro against each other. If he attempted to feed himself, the hand and arm oscillated so vehemently that he struck his face with quickly-repeated blows, and he failed entirely to convey food to his mouth. While lying on his back, if he raised the feet from the bed, the flexors and extensors of the foot began to jerk, and when he elevated both legs, rapidly alternating abduction and adduction took place, and the knees were knocked forcibly against each other. On tickling the soles of the feet the agitations in the legs became greatly increased, and speedily extended to the whole body.

In the recumbent posture the muscles of the trunk are usually unaffected; but when the commotions in the limbs are violent, jerking is observed to ensue in the abdominal and lumbar muscles. In the same way the breathing is in general performed naturally; but during a general attack of the shaking, the thoracic muscles act intermittently and spasmodically, and produce a catching or panting of the breathing, and a jerking interruption of the voice in speaking.

The head when first raised from the pillow nods rhythmically, or shakes sideways, and the movements are with difficulty stopped by laying it down again. The lower jaw is also sometimes seen to move up and down by the rapid intermittent action of the masseter and temporals; but it is remarkable that the facial muscles of expression are exempt from the affection, and no distortion or grimacing of the features is ever produced. The tongue is also unaffected.

In the sitting posture similar movements, but usually more energetic, occur. The shaking begins in the legs and arms, and spreads to the head and trunk. The arms are usually drawn to and fro across the chest. The paroxysms of shaking do not come on unless he is excited by being inspected, or unless he begins to move. The paroxysms last only a minute or two if he continues seated; while they last they make the chair shake under him. After they pass off, he remains quiet for a shorter or longer time, till some slight cause again renews them, often somewhat suddenly. Thus, when seated near the wall of the ward, he has more than once knocked his head against it, in consequence of the oscillations beginning before he had time to remove himself to a safe distance.

Standing and Walking.—On rising from bed, as soon as the feet touch the ground, the shaking of the limbs and trunk begins, accompanied at first by jerking of the extensors of the feet, so that he seems in danger of being thrown forwards on the ground. He succeeds, however, usually in maintaining his balance in spite of the violence of the jactitations. When he walks, he widens his legs to give a larger basis of support; he raises and sets down the feet almost naturally, only occasionally with somewhat of a spring from the jerk of the extensors. In consequence of the commotions of the legs and body, he presents, in walking, a singular staggering appearance. As each leg is lifted up, it oscillates as if it had no fixed direction, and the whole body is shaken so forcibly that he seems in danger of falling at every step. When the spasmodic movements are very strong, he makes for the nearest pillar in the ward, and often, unable to proceed, supports himself against it, his arms and legs jerking, and his body oscillating violently, nearly every muscle of his frame being thrown into intermittent action. Sometimes he is quite unable to attempt further progress. On other occasions, if he can overcome the difficulties of first starting, at which time the shaking is always worst, he improves by perseverance, and, after accomplishing one or more turns along the ward, he walks with much greater confidence, sometimes almost quite steadily. Any excitement or emotion of mind uniformly increases the disorder of his movements, in spite of all his efforts to the contrary. Thus, during the daily visit his symptoms were always more developed, and Conolly, after being nearly steady all morning, and feeling quite elated at the improvement in his condition, would, to his great disappointment, shake as violently as ever, so soon as the physician and students entered the ward. The death of any patient in the ward always made him worse, by the impression produced on his feelings. It was observed likewise that cold weather aggravated his symptoms. Thus, on the occurrence of frost in the end of February, after he had been much better for some weeks previously, he was obliged to take to bed, owing to the increase of the disordered movements. A constipated state of the bowels also affected him injuriously till relieved by purgatives.

He is not usually confined to bed, except when the shaking is so extreme as to render sitting or standing impossible, for the recumbent posture renders the shaking worse when he begins to move; while, on the contrary, change of posture and moderate exercise increase his control over the irregular actions.

From the patient's statement that he was unable to stand or walk, the case was supposed to be one of true palsy, of the kind sometimes called shaking palsy or paralysis agitans; but, on investigation, it was at once discovered that the voluntary motor power was not sensibly impaired. Notwithstanding the shaking, he could flex or extend his arms or legs voluntarily with perfect facility and with a normal amount of force; for when, to test their power, the flexion or extension was opposed by the effort of a bystander of ordinary strength, it was found that Conolly could successfully resist, and, in spite of opposition, could flex or extend his limbs, or prevent their forced flexion or extension, just as well as an individual in full possession of his muscular powers. He can grasp or squeeze the hand firmly, and clench or open the hand with perfect freedom, although the arm is shaking all the time; and, although he walks so unsteadily himself as to seem in constant danger of falling, he had strength sufficient to carry a fellow-patient on his back through the ward. Other particulars worthy of note are, that, according to his own statement, he can run more easily than he can walk (although to the onlooker the shaking in both cases appears about the same); he always walks straight, not swerving to right or left; he can turn himself without difficulty; and when he shuts his eyes the unsteadiness is not increased, he can balance himself, and walk as well blind as with his eyes open. Farther, he can go up stairs tolerably well, but he is afraid to come down for fear of falling. From these circumstances there can be no doubt that, although Conolly exhibits, in consequence of spasmodic interference, a deficieney or incapacity of sustained action, there is, properly speaking, no paralysis of voluntary motion, and no decided loss of muscular power. In fact, his movements seem to be performed by a double mechanism. The voluntary motor apparatus acts as usual, but its effects are interrupted by the involuntary and spasmodic agitation of those muscles which ought to support and sustain the voluntary movements.

On the application of galvanism, the muscular contractibility and sensibility appeared natural; neither increased nor diminished.

On 3d February, Conolly was put under chloroform. When completely insensible, the shaking movements entirely ceased. As he partially recovered consciousness, the agitations still remained in abeyance, and, to the surprise of every one, he was observed to move his limbs with perfect steadiness; thus he repeatedly raised his legs from the bed and crossed his knees, flexed and extended his arms, etc., without the slightest shaking. On tickling the soles of the feet, or applying heat or cold to them, no decided effects ensued, no marked reflex movements were produced. When he had completely recovered from the effects of the chloroform the shaking movements returned, but were for some time less violent than before its administration.

It is remarkable that, with so much disturbance of motion, the sensory functions are not in the least affected. He never had suffered pain at any time, and there is no defect nor perversion of common sensibility, nor of the sense of touch or of temperature in any part. There is no tenderness of the spine, except that on a single occasion he complained of pain at one spot over the second lumbar vertebra, and that not decidedly. He has never experienced any feeling of constriction in the thorax or abdomen, and has perfect control over the rectum and bladder. There have never been any signs of cerebral or cerebellar affection; no headache, giddiness, nor vomiting; mental faculties ordinary; and sleep natural. No symptoms of the cranial nerves being involved. The pupils are large, but this condition appears natural; the sight is good, and the other senses normal.

The other organs and functions present nothing worthy of note. No disease of heart or lungs. Pulse 70, regular, rather soft; it rises to 80 during the shaking. Skin of natural temperature;

appetite good; bowels rather confined; urine, 40 ounces daily, limpid, acid, sp. gr. 1017, not albuminous, holding phosphates in solution, no oxalates.

The *treatment* has been conducted on the tonic plan, together with narcotics; the principal means employed were, good diet, steel, quinine, zinc, arsenic, iodide of potassium (which last made him worse), belladonna, henbane. Warm poultices to the spine were of marked benefit; counter-irritation did no good. The cold douche to the back was tried while he was in a warm bath, but it occasioned such sudden increase of the shaking that the patient injured himself against the sides of the bath, and was worse for some time after. The weather lately has been of too low a temperature for repeating any tonic application of cold. On the whole, no single remedy except belladonna and chloroform has exerted any marked beneficial effect, but the general tonic treatment has been of advantage. He has often improved greatly, but is apt to relapse. In the first week of March, in particular, he was better than at any period since admission; he could feed himself without assistance, and could stand and walk with such perfect steadiness that he no longer looked like the same individual. For a few days he seemed cured; unfortunately he relapsed again into his former condition, owing perhaps to over-exertion, more probably to the return of the cold weather.

Remarks.—I shall confine myself to some observations on the nature of this case, and the name by which it should designated.

When Conolly is observed walking in the ward the irregular jerking and shaking movements naturally suggest chorea (St Vitus's dance), which his disorder resembles not only in affecting the voluntary muscular apparatus, but also in the antecedent rheumatism, in being occasioned by a fright, and in the cessation of the movements during sleep; but, on examination, his disordered movements are seen to differ entirely from those of true chorea. They are shaking or oscillatory movements, to and fro, by the alternate action of antagonistic muscles; they repeat themselves rhythmically when once begun, and are usually symmetrical on the two sides of the body. They present nothing of the irregular and rapidly-changing, unsymmetrical dancing and gesticulating character of the true choreic movements. The grimaces of the features so usual in chorea, are entirely absent in Conolly, and his age and general appearance are different from those of the usual subjects of St Vitus's dance.

But although not the true Chorea Sancti Viti of Sydenham, Conolly's disorder belongs to the class of nervous affections allied to chorea, which have been distinguished by nosologists under the name Tremores or the Trembles. It is true that the word "tremor" does not sufficiently express the violence of the commotions in Conolly's case, for in the cases of tremors the movements are commonly slight and tremulous merely, not shaking and tumultuous; but, on the other hand, shaking movements are really nothing but an exaggeration of trembling, and, in point of fact, in certain extreme cases described as tremors, the movements appear to have been of the same nature and as vehement as those of Conolly.

The class Tremores, admitted by Galen, Sylvius de le Boe, and Sauvages, was rejected by Cullen,[1] who considered nervous trembling merely as a symptom depending on asthenia, paralysis, or convulsion, and included it under the genus paralysis. Among recent authorities, however, Romberg[2] has reinstated the Tremores as a distinct division of the neuroses of motility, "forming," as he says, "the bridge which conducts from the region of convulsions to the paralyses." He assigns, as the chief causes, advanced age, poisoning with mercurial vapour or lead, alcohol, venereal excesses, fevers, mental emotion, and debility during the period of convalescence and after profuse discharges. Passing over, with the short mention of a few lines, the tremor potatorum, the tremor senilis, and the tremor febrilis, Romberg treats, as of greater importance, the mercurial tremor and the paralysis agitans, which alone present any correspondence with our present case.

The symptoms of "mercurial tremors," in their more aggravated forms, closely resemble those presented by Conolly. Thus, the case of John Chattin, described by Dr Watson in his thirty-eighth lecture on the Practice of Medicine, is the counterpart of Conolly's in its principal features, with the exception that in Dr Watson's patient the tongue was affected, and there was more decided motor weakness or paralysis than in Conolly. But while Chattin had, in his employment as a "water-gilder," been poisoned by the fumes of mercury, in the case of Conolly there has been no source whatever of mercurial infection (not even in the shape of medicine); nor can his shaking disorder be ascribed to lead or alcoholism, or the action of any similar nervine poison.

In regard to paralysis agitans, it is necessary to remark that this name is sometimes loosely and inaccurately applied. In its strict and classical sense, the paralysis agitans of Parkinson[3] (who first clearly distinguished and described it, and whose account has been adopted by succeeding authors, Romberg, Todd, Watson, and others) is a special disease, peculiar to old age, and consists of the combination of two pathognomonic symptoms. The first of these is tremor—the "tremor coactus." This tremor, or involuntary tremulous motion, comes on very gradually, first in the arms, and after a considerable interval in the legs and body; it is accompanied by "lessened voluntary muscular power," and it occurs "in parts not in action, and even supported." The second pathognomic symptom, which must be associated with the preceding in order to constitute paralysis agitans, is a propensity to bend the trunk forwards, "and to pass from a running to a walking pace,"—a propensity to forward movement to which Sauvages gave the name of scelotyrbe festinans, seu festinia. The combination of tremor coactus with scelotyrbe festinans, therefore, constitutes the true paralysis agitans of Parkinson. Further, the paralysis agitans so constituted is invariably fatal, and depends on some organic lesion of the nervous centres, which, although Parkinson specially invited the attention of pathological anatomists to it, has not yet been investigated.[1] It is hardly necessary to say that Conolly's case does not in any way correspond with the disease described by Parkinson. Conolly is in the prime of life; his shaking is more violent than the tremors of old age or paralysis agitans senilis; his disordered movements soon cease when the limbs are supported; there is no failure of voluntary muscular power; there is no irresistible tendency to move or fall forwards, and no reason to apprehend an inevitably fatal result.

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[1] "Tremor. Artus alterna per itus et reditus frequentes motitatio."—Tremorem, utpote semper symptomaticum in numerum generum recipere nollem; species autem à Sauvagesio recensitas, prout mihi Astheniæ, vel Paralysios, vel convulsionis symptomata esse videntur, hic subjungam. 1. Species asthenicæ. Tremor a debilitate; senilis; a coffea; a pathemate, metu, ira, gaudio, libidine; typhodes. 2. Species paralyticæ. Tremor paralytodes; vertiginosus; a plethora; a saburra; a hydrocephalo; scorbuticus; rheumatismalis; temulentus; metallurgorum; traumaticus. 3. Species convulsivæ. Tremor coactus; tendinum; palpitans.—*Cullen. Synopsis Nosologiæ Methodicæ—Neuroses.* Edinburgi, 1785. Thomson's Edition, vol. i. p. 297.

[2] Romberg. Nervous Diseases, translated by Sieveking. Syd. Soc. ii. p. 230.

[3] An Essay on the Shaking Palsy, by James Parkinson, London, 1817.

It may be questioned, however, whether the term paralysis agitans ought to be confined to the senile form of it, associated with the propensity to fall or move forwards, described by Parkinson. I think, that it would be more useful to reserve for Parkinson's disease the specific name of paralysis agitans festinia, or senilis, or Parkinsonii, and thus leave us free to extend the general name of paralysis agitans to other cases occurring at various ages, and not attended by the irresistible impulse forwards. Certainly there are many instances of true paralysis (especially spinal paralysis and paraplegia) which are accompanied by jerking and shaking movements without any tendency to fall forward, and which require a designation that shall not confound them with Parkinson's disease. Such a case I had in my ward three years ago in a man, John M'Lachlan, aged between 30 and 40, who was markedly paraplegic, and who presented in a high degree irregular jerking and shaking movements, in many respects resembling those of Conolly, only that they presented more of the jerking and less of the shaking than in Conolly. In M'Lachlan's case, lesions both of motion and sensation which were present clearly proved the existence of organic disease of the spinal cord, and the spasmodic jerking appeared to be due to excessive excitability of the reflex motor actions of the spinal cord, together with some deficiency in its co-ordinating power.

This case, which was repeatedly in hospital both under my care and that of my colleagues, Drs Warburton Begbie and Haldane, was named "paralysis agitans" (non-senilis), or "choreic" (more properly choreiform) "paralysis." Since the time when M'Lachlan was under my care, the study of the irregular movements in paralysis and in disease of the nervous centres generally has excited great attention, in consequence of the researches of Duchenne,[1] who, in 1858, first described what he considered a new form of nervous disorder under the name of "ataxie locomotrice progressive." By ataxia is meant not motor paralysis, but a want of co-ordination in the muscular movements; it usually precedes or is accompanied by motor paralysis, but is distinct from it. The term ataxia was first, I believe, used in this sense, many years ago, by Professor Bouillaud,[2] who applied it especially to designate the disordered movements occasioned by injury or disease of the cerebellum. In addition to these, however, Bouillaud included among the ataxiæ, not only chorea and other affections of the voluntary movements, but also irregular action of involuntary muscles, *e.g.*, irregular action of the heart. The term ataxia afterwards fell into disuse, till it was revived by Duchenne. Duchenne was the first who clearly separated ataxia from paralysis, and the progressive form of ataxia described by him was certainly not at all or very imperfectly known before his researches, having been confounded with many others in the ill-defined group called tabes dorsalis. As this subject does not, however, more immediately concern our present inquiry, I need only observe, in relation to ataxia, that two things require to be distinguished: *first*, Ataxia in general, considered as a symptom which is common to several forms of disease of the nervous centres (especially disease of the posterior columns of the spinal cord); and, *second*, The particular disease, "progressive locomotor ataxia." The latter, as described by Duchenne, appears to be a distinct nosological species, and, besides its progressive course, is characterized by a combination of at least three orders of symptoms; namely, 1*st*, Affections of the cranial nerves, as, paralysis, usually transient, of the sixth pair, double vision, amaurosis; 2*d*, Affection of the sensibility, especially paroxysms of intense deep-seated pain, shooting like lightning (douleurs fulgurantes), and ultimately impaired cutaneous and muscular sensibility; and, 3*d*, Ataxy, or want of co-ordination in the muscular actions of the limbs, attended or not by motor paralysis. It is plain that this complex and peculiar affection is altogether *sui generis*, and cannot be confounded with any other. But the ataxia disorder of movements may occur in diseases of the spinal cord (as Todd long ago indicated) quite independently of affections of the cranial nerves, which form an essential part of the grave and almost incurable malady described by Duchenne. And these simpler cases of ataxic paralysis are not unfrequently attended by shaking movements of the limbs, and might be, and indeed have sometimes, both before Duchenne's researches and since, been designated as paralysis agitans.[1] I have already alluded to M'Lachlan's case, which was an instance of paraplegia ataxica agitans; and while writing these remarks, I have, by the kindness of Dr James Hunter, had an opportunity of seeing a case of spinal disease, paraplegic in character, in which the arms are partially affected as well as the legs, the paralysis is greater on the left side (hemiplegic paraplegia), the left leg is flexed and rigid, and pains, with some amount of anæsthesia, are present in both limbs. This patient, a female aged between 30 and 40, still exhibits shaking movements much like Conolly's, though not so violent; and Dr Hunter informs me that a year ago the shaking was much more extensive than it is now. It still prevents her feeding herself. It is difficult to estimate the amount of ataxia from the weak condition of the patient.

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[1] Parkinson considers it probable that the cervical portion of the spinal cord is affected; he found it indurated in a case of shaking palsy.

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[1] Duchenne, Electrisation localisée, 2d Ed. 1861. Trousseau, Clinique Méd. de l'Hôtel Dieu, 2d vol. 1st Ed. 1863; 2d Ed. 1865. Meryon on Paralysis, 1864, p. 192. It was by Trousseau's Lectures that my attention was first directed to Duchenne's researches on the Progressive Locomotor Ataxy.

[2] Bouillaud. Nosographie Médicale. 1846. Vol. v.

If, then, we admit a paralysis agitans non-senilis, whether accompanied by ataxia or not, does Conolly's case belong to that form of disease? In ordinary parlance, I have no doubt many would be inclined to call it so; but if we are to attempt accuracy in technical names at all, and unquestionably accurate nomenclature is of great moment, it is impossible to apply the term paralysis agitans to it without some marked qualification; for, in point of fact, an essential and most important feature of Conolly's case is the absence of motor paralysis, so that one-half only of the name would be correct: it is morbus agitans, but it is not paralysis. It is this which so widely distinguishes his disease from that of M'Lachlan, with which it ought in no way to be confounded, and renders the title paralysis agitans, in Conolly's case, a misnomer.

In regard to the term "ataxia," although etymologically it might be applied to the disordered movements in Conolly's case (which might then be named ataxia agitans), yet if we adhere to the meaning which Duchenne attaches to the word, viz., want of co-ordination of the muscular acts, the term ceases to apply. In Conolly there is no loss of co-ordination, no interruption of the co-ordinating apparatus, but only a spasmodic interference with the regular movements, in consequence of deficient power, with increased excitability of the nerve centres, especially in the spinal cord.

If we required to invent a name, perhaps dystaxia agitans might express sufficiently the characteristic symptom; or, adopting an old classical name, we might call it scelotyrbe agitans; but the chief distinction would be sufficiently brought out by a modification of the more usual phrase, viz., "pseudo-paralysis agitans."

Although, in consequence of the confusion of nomenclature, the question of name required discussion, the more important inquiry in regard to the nature of the affection may be summed up very briefly. Notwithstanding the formidable appearance of Conolly's symptoms, a careful examination appears to me to lead undoubtedly to the conclusion, which in regard to prognosis is of the utmost importance, that his disorder is, like St Vitus's dance, functional merely, and does not depend on an organic lesion of the spinal cord or cerebellum. This seems proved,—1*st*, By the absence of decided motor paralysis; 2*d*, By the absence of any affection of the sensibility; 3*d*, By the fact that he can maintain his equilibrium, and walk as well with his eyes closed as open. The steadiness of the movements when he was under the influence of chloroform, the variations in the symptoms under rest or excitement, and the great improvement, though only temporary, which took place in the beginning of March,—all point to a functional disorder of the spinal cord. Since Conolly came under observation, I have found that Dr Handfield Jones, in his recent work on Functional Nervous Disorders, refers to cases of functional paralysis agitans, which resembled Conolly's, though less severe, and which ultimately recovered.

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[1] See Cruveilhier's case of Gruyère, p. 117; also the case of C. A., p. 104, and others, in Topinard, De l'Ataxie Locomotrice, Paris, 1864.

I do not think it necessary to adduce any arguments against the opinion of the existence of disease of the cerebellum in Conolly's case. He does not present the loss of equilibrium, nor the peculiar staggering, nor the headache, vomiting, etc., which characterize cerebellar disease.

The conclusions, which I submit, are :—

1*st*, The case is one of functional disease of the spinal cord, allied to chorea—(spinal chorea?). It is not paralysis, nor ataxia.

2*d*, The disorder belongs to the class "tremores," and is of the kind sometimes called paralysis agitans (not the senile form,—non-senilis); but from the absence of muscular paralysis it cannot be so named correctly. It may be called dystaxia or scelotyrbe agitans, or, perhaps better, pseudo-paralysis agitans.

3*d*, In the form, and to the extent presented in this case, it is a rare affection, as existing independently of old age, mercurial poisoning, great debility, or organic disease of the cerebro-spinal axis.

4*th*, It probably depends on a weak and excitable condition of the motor centres in the spinal cord, due to anæmia of its grey substance.

5*th*, The predisposing cause of the disease was the accident which the patient met with by the fall of the scaffolding; the exciting cause was the fright occasioned by the second fall. It is not at all likely that the ague or rheumatism, which occurred and had been cured so many years before, had any share in its causation.

Postscript.—Dr Charles Wilson, whose reputation as a linguist and antiquary is well known, informs me that in Roxburghshire, where he practised for many years, "there was always a popular distinction, Scoticè, between the '*perls*' (paralysis) and the '*bevers*' (Danish *bævre*, Lower Saxon *bevern*, to tremble, shake, or be agitated by shaking). The latter was the paralysis agitans or shaking palsy, though sometimes used in a kind of secondary sense for the tremors following a debauch."

ARTICLE III.—*Observations on Contemporary French Surgery.* By JOHN DUNCAN, M.A., M.D., F.R.C.S. Edin.

HAVING lately had the opportunity of studying during six months French Surgical practice, it may not be uninteresting to note the impressions produced by it. I shall, confine myself to points of special importance, wherein it differs from our own, and I shall consider,

I. Certain methods of operation; and,

II. The treatment of some particular diseases.

Three modes of operation are worthy of attention. These are: 1*st*, "écrasement," or division of tissues by constriction; 2*d*, cauterization; and, 3*d*, "arrachement," or the "tearing" process.

1*st*, The ligature is an application of old date in various forms of surgical disease; but it has only in more recent times been applied to the speedy division of tissues. In the early part of this century, indeed, its cutting action is referred to by Roux, Dupuytren, and others. In 1838, M. Mayor of Lausanne published a memoir on the subject, in which he very strongly recommended it. M. Graefe, also, for the extraction of nasal and other polypi, constructed an instrument quite similar to those now in use, and it, or a modification of it, was adopted by various surgeons. The present notoriety, however, of this mode of operation is due entirely to M. Chassaignac, who has invented an écraseur, and applied it in every conceivable way. It is unnecessary to describe an instrument which is everywhere sufficiently known. Suffice it to say, that by some surgeons string and iron wire are substituted for the metallic chain, but that these have the disadvantage of dividing much less easily, and therefore of being more easily broken than the last. The manner of using a constrictor may require some explanation. When the tumour is small and pediculated, the chain is simply thrown round its neck, and the section at once performed; and when sessile, the operation is scarcely more difficult, for it is requisite only to transfix its base by a trochar or needles, which prevent the instrument from slipping. Occasionally, however, manœuvres of a much more tedious and complicated nature become necessary. On Monday the 7th of March 1864, an encephaloid—as was believed, a degenerated fibrous—tumour of the mamma about the size of an orange, was removed by M. Chassaignac. To avoid comprehending much skin, which is not easily divided, a canula was lodged, by means of its trochar, under the base of the tumour in its shortest diameter. The chain was passed through the canula, the latter was withdrawn, and by slow constriction the tumour was split into equal portions. By similar means the halves were taken away in successive pieces, five or six in number; and, as some suspicious-looking nodules still remained in the substance of the pectoral muscle, they were removed in the manner described for small sessile tumours. In all there were required ten or eleven applications of the instrument, each exacting a considerable time.

I may mention another case, one of cancer of the rectum. As a preliminary, the rectum was transfixed from before backwards, the chain passed, and a linear wound thus made in the perinæum, allowing of pretty free access to the disease. Portions of the mass were then seized and pulled down by large vulsella, which were constantly losing their hold of the friable tissue, and it was not therefore without some difficulty that the loop was passed round them, and their sections successively accomplished. In such cases the chain écraseur is evidently the most convenient; but in others, as in fibrous polypus of the uterus, the iron wire has a decided advantage. This depends on its property of passing from a right angle to a straight line in being screwed home. The wire is passed through the dilated os uteri, and over the body of the polypus at right angles to the handle of the instrument. Being then drawn back by the screw or other apparatus, it gradually brings itself into a straight line with the handle, and so acts directly on the neck of the growth.

The wonderful properties and consequently extensive application claimed for these instruments by their inventors, are only equalled in the advertisements of Parr's life pills or Holloway's ointment. They may, however, be summed up as follows:—That hæmorrhage is done away with, and that erysipelas and pyæmia are avoided; and certainly, if such were real, a vast service would have been rendered to surgery. Let us examine a little into the truth of these claims, and in doing so at once admit that hæmorrhage is prevented. I have seen operations on the most vascular parts of the body, such as the rectum and os uteri, in cancer of these organs. In none did the hæmorrhage exceed one or two spoonfuls; and by a careful and slow progression, I believe that even that might be avoided. In less vascular parts the amount of blood lost does not exceed the drop or two caused by the introduction of the needle or trochar. Such a result might indeed have been expected from the analogy of crushed and torn wounds. Every surgeon has seen cases of this kind, whether in the crushing of a finger, as happens every day, or in the wrenching off of a limb, as occurred to the miller, whose history is given by Cheselden,[1] and the boy mentioned by Sir C. Bell,[2] who, after being raised to the roof by machinery, himself fell to the ground, while his arm was carried into the "other apartment." With the constrictor, however, there are certain differences. On the one hand, there is some little danger of a vessel being partially divided and bleeding from its open side; and on the other, if there is less of the twisting and tearing of the artery and surrounding parts, which assist so materially in the temporary hæmostatics of accidents, there is, what does not occur in them, an amount of forcible compression which, applied to almost any solids whatever, would make them continue to adhere even after it was removed. This cannot, of course, be supposed sufficient for the main trunks of the body. The ligature alone can satisfactorily control them. But the testicle I have seen removed without the smallest hæmorrhage, and the hæmorrhoidal, mammary, and arteries of even larger size, do not bleed after being thus divided. So much, then, may be accorded to the écraseur. It does not seem, however, to have the same advantages over the bistoury in reference to erysiplas and pyæmia,—an advantage which, if it did exist, would be of vastly greater importance than the one we have discussed. Its results must be compared, not with operations by the knife in general, but with those of the magnitude and nature to which it is confined. The écraseur, for instance, cannot be generally used for the major operations of surgery, such as amputations or excisions. It has indeed, been used by its inventor and others in the former, and even in lithotomy.[1] But these one or two exceptional cases can have no value in a general estimate, which should relate solely to its common uses. It is much vaunted as being free from danger in the operations for fistula in ano and for hæmorrhoids. It ought to be so; for our present effectual operations are thoroughly safe. In a vast number of fistula cases I have met with only one accident, a gangrenous erysipelas of the perinæum and scrotum. This will now and then arise from the merest scratch; and, curiously enough, among eighteen or nineteen operations on the rectum, which I saw practised by the inventor of the "écraseur lineaire," I witnessed a forcible dilatation by the finger for fissure of the anus, which was followed by the same result. Again, in excision of the mamma, another supposed triumph of the écraseur, while house-surgeon with Mr Syme, I had under my charge eighteen cases, all cured without a bad symptom; and in Paris I saw only six or seven performed by the constrictor,—also cures, but one after a slight erythema. As a whole, in fact, and taking into account operations of equal magnitude on both sides, I should, from my own observation, be inclined to put wounds by the bistoury and the écraseur as nearly as possible on the same level, so far as regards the danger of the patient, whether in reference to erysipelas or pyæmia.

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[1] Anatomy, p. 321.

[2] Sir C. Bell's Institutes, p. 49.

I have been still further confirmed in this opinion by the examination of certain statistics[2] of the écraseur; for I find that it is

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[1] I have the history of nine cases of amputation. The results do not seem particularly favourable—certainly not more so than with the bistoury:—

Amputation.	Ages.	Disease.	Result.	
1. Thigh.	35.	Caries.	Death.	
7. Leg.	35.	Fracture.	Cured.	The amputation of the leg is in most cases the French substitute for that at the ankle-joint, immediately above the malleoli.
	30.	Joint-disease.		
	20.	"		
	18.	Wound.		
	17.	Joint-disease.		
	16.	"		
	14.	Caries.		
1. Fore-arm.	17.	Joint-disease.	Cured.	

It will be observed that the patients were all young, and that consequently with the bistoury similar results might have been expected. In fact, as it was a trial, favourable cases were doubtless selected. It seems now given up.

[2] M. Maisonneuve's Clinique Chirurgicale, p. 211.

precisely in those cases in which we would expect a cure with certainty that the table presents no deaths, while otherwise the mortality is now greater, now less, than is usual. Statistics, however, without details, are but poor guides, and in stating my belief that the dangers are nearly equal, I rest much more on what I have seen than read. If the opinion be a correct one, the fact is one which could hardly have been anticipated from our knowledge of the effects of bruising and laceration. Contused and lacerated wounds, it has long been a dogma in surgery, are more apt than any others to be followed by dangerous sequelæ. But these sequelæ depend upon the bruising of surrounding parts, and the extravasation of blood therein, which make them more liable to take on any inflammatory action that may arise. Now, after the écraseur none of this bruising exists, the wound resembles a clean cut, and the neighbouring parts are as sound as before the operation. In the opinion given above, I am doubtless at variance with those who maintain the necessary superiority of the bistoury, but my reasons I have already stated. On the other hand, the supporters of the écraseur as the better weapon, depend, it would seem, much more upon theoretical argument than practical induction. In relation to pyæmia, moreover, these arguments are founded on what, since the investigations of Virchow, must be looked on as a false pathology. Purulent infection, they say, can rarely occur, because the veins by which the morbid matter enters are occluded in the division. But, admitting this occlusion, pyæmia, it may be answered, is not a purulent infection, as was formerly supposed. It must now be regarded as a poison of nature unknown, which, entering the economy by absorption from a wound, produces inflammation of serous membranes, and, among others, of the veins at the injured part, which thereby causes softening and disintegration of the clots already formed in these veins, and, in consequence, metastatic abscesses in the lungs, liver, and body generally. All this the occlusion of the veins cannot prevent, as an open mouth is not necessary to absorption. But further, pyæmia, as a rule, does not commence till a wound, however caused, is in a suppurating state, and, whether produced by the bistoury or écraseur, is essentially in the same condition as to veins and all other tissues. Theoretically, therefore, as well as practically, there is no reason to suppose that the former of these instruments is more liable to this danger than the latter.

While I admit that the écraseur is not more dangerous than the knife, and that it is efficient in its control of hæmorrhage, there are certain disadvantages which must be regarded in an estimate of its value. In the first place, it often destroys a quite unnecessary amount of skin, that texture of the body which nature worst repairs. The consequence is a most lengthened cure during the slow formation of a large cicatrix. In tumours of the mamma, for example, a most happy result would be cicatrization in six weeks or two months; while after an operation by the bistoury about half that time is required, and occasionally the whole may be healed in a week. Union by the first intention is, of course, out of the question; and, even supposing that the skin were divided by the knife, and only the deeper parts by the écraseur, this result could not be hoped for. The nature of the wound is such that it can only heal by granulation. A slight exfoliation is certain to take place from the surface; for although the neighbourhood be not bruised or torn, the cells through which the chain has passed have been contused and deprived of their vitality. This is a most serious objection; for, other things being equal, that which will delay a cure should certainly be avoided. Other disadvantages also exist, though not perhaps of such importance. The tedious character of the operation itself is one of these. The exhaustion of a patient is now much mitigated by the use of anæsthetics; but even yet, the longer a patient remains on the table, the more will he be worn out when the influence of the chloroform has passed off.

The cases in which the écraseur should be used may be inferred from the foregoing observations. They are, 1*st*, those in which hæmorrhage after cutting would be difficult of control; and, 2*d*, those in which no loss of skin is involved; union by the first intention is impossible or undesirable, and its application is easier than that of another method. Among such I would include the fibrous polypus of the uterus, the cauliflower excrescence of the os,[1] and cancer of the rectum in the extremely rare cases fitted for any operation. The safety and ease of the operation in the first two, I have had several opportunities of witnessing, and I have not the slightest hesitation in recommending its use. But few cases of cancer of the rectum come under the notice of the surgeon in a position or condition justifying removal. I have, however, seen one in which the cure had lasted several years; and in that already related in this paper, though the utmost that could be hoped was a doubtful palliation, the immediate result was successful enough. It has been used not without success in excision of the tongue; but the fact that cases of hæmorrhage have occurred, one even requiring ligature of the carotid, does not speak in its favour. There may be other uses which I have not mentioned, and which would occur in the course of practice. It might very probably be applied with success to the long pelvic adhesions which sometimes complicate ovariotomy, and make necessary the application of several clamps; nay, it might possibly even be employed for the division of the ovarian pedicle itself. However that may be, I have referred to its principal uses; and it will be observed that the instrument, far from being a substitute for the bistoury, is one of very limited application.

2*d*, The removal of growths by caustic has been practised always and in all countries; and it would be absurd to mention it as peculiar to France, had it not been adopted as the preferable operation in all practicable cases by a distinguished Parisian surgeon, M. Maisonneuve, whose mode of using it is worthy of consideration. For the caustic he maintains the same immunity from the common causes of traumatic death as M. Chassaignac and others claim for the écraseur, and with it he bi-weekly removes tumours of the mamma, epithelioma of the lip, cancer of the tongue, and, generally speaking, growths in every region of the body. He applies the caustic in a manner peculiar to himself. A paste, composed of chloride of zinc, flour, and water,[1] is mixed, thoroughly dried, and then divided into flat pieces of various lengths, and having the shape of a long isosceles triangle. When about to remove a tumour by these his "flêches," M. Maisonneuve thrusts a bistoury from the circumference, under the base of the tumour, and follows it immediately by a stick of the paste. As this has been ascertained to destroy about half an inch all round, the arrows are inserted at this distance from each other, till at length the tumour finds itself encircled and undermined. Covered by charpie they are allowed to remain till next morning, a period of about eighteen hours, and a poultice is applied on their removal. The slough separates in from ten to fourteen days.

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[1] M. Mayor published, in 1832, a "Memoire sur la Ligature appliquée à l'Amputation du col de l'Utérus."

Here, as with the écraseur, we may freely admit that there is no bleeding at the time of their introduction. It seems doubtful, however, whether or not there is some danger from secondary hæmorrhage. I have seen one case at least, in which the separation of a cauterized slough was followed by bleeding not a little alarming; and M. Maisonneuve himself records one in which hæmorrhage, requiring ligature of the carotid, followed the removal by "flêches" of a tumour in the upper part of the neck. As a preventive of pyæmia it has been much praised, on the theory that the pyæmic poison, consisting of decomposed or decomposable animal matter or being a product of it, is prevented from entering the economy, by the interposition between the mass to be removed and the sound parts of the non-putrefiable substance killed by the chloride of zinc. It is forgotten that the poison, if this be its origin, may be easily introduced from the pus during and after the inflammatory separation of the slough. Caustic, no doubt, is in this respect a very safe mode of inflicting a wound; but there seems little ground to believe it better than the bistoury, if proper cleanliness be observed, and a free drain given to the pus. That it is not entirely devoid of danger I indeed know, for I have seen a man die of pyæmia after the removal of a cancerous tumour over the lower jaw by means of the "cauterization en flêches." It is said also to secure an immunity from erythema and erysipelas; but it is to be feared that these diseases, so incomprehensible in their etiology and capricious in their attacks, will not be warded off by this more than by other surgical appliances; and in reality they do occur. But while the zinc arrows do not merit the high encomiums passed on them, as on caustics in general, by M. Maisonneuve, they are at least a good method of cauterization. They admirably fulfil their end,—that of removing a diseased portion. Several times I have seen most extensive epithelial growths of the lip and tumours of the mamma, and twice about three-fourths of the tongue excised, so to speak, in this way. In one of these last a curious incident occurred, which I cannot avoid mentioning, although unconnected with my present subject. On the thirteenth day the tongue was found attached by a mere thread, and on slight traction came away. The astonishment of the surgeon was great on discovering that about three inches of the gustatory nerve were attached to its posterior extremity. Not the slightest bad consequence, however, followed this curious extraction.—But to return: While this paste acts with certainty, it has also the advantage of acting with greater precision than other caustics. The surgeon applies the flêches with care in the position he wills. They do their work without having to destroy a thick mass on the surface, and the extent to which they act is ascertained with considerable exactitude. They are therefore at once without the danger of going too far, and the inconvenience of requiring a second application. But while thus more manageable than other caustics, they do not merit a very much wider encomium. For epithelioma of the lip the knife is infinitely superior, as capable of remedying at once the deformity it occasions. Thus, should it be necessary to remove a considerable portion of the lip; with the one, two operations are necessary; with the other, by the admirable plastic operations at our command, the part taken away can be at the same time restored. For tumours yet covered by the skin it has the same disadvantages as the écraseur, and both therefore are inferior to the bistoury. For small open sores, in which the thickness of tissue to be destroyed is not great, the superficial application is as efficient and more convenient. The cases in fact are not many in which it could be required. A few cancers of the tongue may come under this category, when the patient objects to other operations. It might find a legitimate use in extensive cancer of the mamma, when the skin is largely ulcerated, where a large gash would be left with the knife, and without certainty of removing the whole; for it is probable that of large open wounds that produced by the caustic is the safest. Again, possibly in recurrent fibroid tumours it might be employed, for in the opinion of some of our greatest surgeons this growth is less liable to return after removal by the caustic than the knife. In little else could it be advantageous. It is, as might be supposed, extremly painful, not only at the introduction and during the action of the "arrows," but even for some hours after that has ceased, and should therefore be employed only when obviously more convenient than other methods.

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[1] Composition—Chloride of zinc, one part; flour, two parts; water, as little as possible.

3d, In the third mode of operating, to which I referred in the commencement of the paper, the procedure "par arrachement," there is nothing particularly novel, so far as concerns the principle. Under the designation of enucleation it has been long practised and taught. Fatty tumours are taken away, often without a scratch from the bistoury, except in the first incision, and every surgeon is in the habit of aiding the knife by a judicious application of force. But by some, very much greater prominence is given to this than has hitherto been the case, either in the lecture-room or operating theatre; and by them it is held to be, in its ultimate results, equally good with, and, during the operation, often safer than the knife. A man presented himself at the Hôtel Dieu towards the end of April 1864, suffering from fibrous tumour of the lower jaw, and it was determined to excise from the symphisis to the articulation on the right side. For this purpose an incision was made in the middle line, from the mouth to the chin, and the bone was then sawn through. The mucous membrane was divided near the teeth, and in front of the coronoid process. The finger was now introduced between the periosteum and the bone, and the tissues separated from the latter as high as the insertion of the temporal muscle. This, being rather strong for the blunt instrument, was divided by the scissors, and the jaw was torn from its socket by a twist. Again, on the 5th of March, M. Maisonneuve operated on a patient for encephaloid tumour of the neck. The tumour was situated under the sterno-mastoid, on the left side, was very doubtfully movable, and, in short, the propriety of any operation was more than questionable. An incision was made down to the disease, and with the fingers a way was torn and bored, about half round the tumour. By this time, however, the hæmorrhage had become so considerable that further progress was arrested, and only half the tumour rapidly torn away. The blood now flowed furiously, caustic was applied, hot irons were ordered to be got ready, perchloride of iron was thrust into the wound, and at length, by means of the last styptic, aided by compression, the bleeding was stopped, and the patient was sent to bed, not certainly in a more favourable condition than when she had risen in the morning. This case does not speak in favour of the operation; but at the same time it must be said that the misfortune arose rather from the mistake of operating at all than from the nature of the plan adopted. The operation on the jaw, however, is worthy of remark. The arguments in its favour are, that it permits a smaller incision, that it divides fewer arteries, and that it avoids cutting the branches of the facial nerve, as they issue from the parotid gland. The difference in the size of the incision, however, is but slight, and it heals in this region by the first intention, while the line along the base of the jaw is more concealed than the other in a woman, and equally so by a beard. The few lower branches of the facial nerve which are divided have been found of little moment, and the facial artery on the one hand is of no more importance than the labial and coronary on the other, for it is ligatured as soon as cut. The rest of the operation is simply the sub-periosteal one, done by an inferior instrument, and, where circumstances permit, should be performed in the more perfect manner. One point to be considered is, whether the twist, with which the jaw is taken out at the end, merits adoption; and I am inclined to think that it does. It requires but little force, as I have found by experiment on the dead subject. Every thing is already divided, with the exception of the external lateral and thin capsular ligaments, and the external pterygoid muscle, of which a small portion comes away with the bone. The process, by experience, is found unproductive of harm, and by it the internal maxillary artery is avoided, which, when sliced in a longitudinal direction, is sometimes difficult to tie. Moreover, the truth is, that surgeons at this period of the operation do greatly assist themselves by the use of torsion.

The general question as to the substitution of tearing for cutting may be dismissed in few words. A torn wound is more liable to inflammation than a cut, and the proper course is for the surgeon to consider how much force he is justified in using in each particular case. A good general rule would be, that, where there is simply a connexion by loose cellular tissue or such like, the finger should be used,—where textures have to be divided in their continuity, the knife. In dissecting cellular tissue by the finger the wound is left smooth and regular; while in others, and especially in the muscular, the fasciculi are torn of different lengths, leaving little pouches to collect the pus, and protruding nodules very likely to become gangrenous. This rule indeed is very much what has always been practised, though not perhaps distinctly enunciated.

II. Having thus considered these points of a general character, I would now refer to the treatment of one or two particular diseases, for it is, of course, impossible to treat of everything new or strange in a capital where hobbies are ridden with peculiar vivacity.

Since the time when M. Velpeau introduced into France the injection of hydroceles by tincture of iodine, this treatment has become the one almost universally adopted. Not that objectors are wanting; for while one surgeon makes use of pure alcohol, another cauterizes through the canula with a nodule of nitrate of silver, and a third still injects the old cuisine of white wine and Provence roses. Still iodine is the injection of the great mass of practitioners, and its use is now extended to many other forms of disease. As with us, in fluid bronchocele and in hydrocele or hæmatocele of the neck it is constantly employed; but in addition, what is not common here, it is often practised, on the recommendation of M. Velpeau, in chronic synovitis of the knee. It is said to be only used after the failure of other measures, or the repeated return of fluid in the joint; but sometimes no great care is exercised to ascertain the previous treatment, for it was only discovered after the injection had been made, that one of the four cases which I have seen thus treated had never been previously blistered. However dangerous this proceeding may seem, it is not much so in reality. These four cases were all successful; and one of them, a child of ten or twelve years, could walk with ease two months afterwards, with no great stiffness of the joint, and, so far as could be judged, was perfectly cured. Such bold deeds must not, of course, be rashly done. The inflammation might, on very slight provocation, pass the adhesive limit, and cost the patient life or limb. But instances do occur in which ordinary treatment has failed, or is but temporarily successful, and in which the patient, precluded from work and enjoyment, is anxious to try what gives the feeblest chance of success; for such, we have here a tolerably certain, and not unsafe cure. After the injection the limb should be placed at complete rest in a splint, and on the slightest appearance of excessive action refrigerants should be resorted to. When the inflammatory stage has passed, gentle motion must be employed to preserve, if possible, a movable joint.

There is yet another form of injection more extensively applied in Paris than with us,—the injection of perchloride of iron into veins and arteries. Varix is a disease which, by careful attention to support and rest, may in the rich be kept sufficiently under control; but with many a poor man it produces such inconvenience as seriously to interfere as well with his work as his comfort. Many remedies have therefore been tried, but most of them have proved either so useless or so dangerous as to be little practised. Since the styptic and coagulating properties of the perchloride of iron have become generally known, it has been applied to this form of disease among others, and its success, to say the least of it, is encouraging. In a series of about twenty cases in the practice of M. Maisonneuve most have been afforded relief, and none have had the slightest bad symptom. One patient, whose left leg had been treated eighteen months before (he had thought it prudent not to risk too much at once), was so pleased with the result that he came back to have the same cure performed on the right. Like the injection of which I have already spoken, this should only be performed when simpler means have failed,—when the elastic stocking is unsuccessful, or the patient cannot procure one. The palliative is so good that it would be, to say the least of it, imprudent to substitute for it an operation which certainly cannot be without its danger; nay, I would even go so far as to say, that when it fails there is most often little chance of success by an operation on the superficial veins, which probably are not alone diseased. In the performance a bandage should be rolled tightly round the limb, above the seat of disease, for the purpose at once of enlarging the veins and stopping the upward progress of the blood and perchloride. The most absolute quiet should afterwards be enjoined till all signs of irritation have passed away.

In certain cases of aneurism also the perchloride is used with benefit; but care should be taken to select suitable cases. Its danger is a too extensive coagulation of the blood. The injection therefore of an aneurism on the main artery of a limb is liable to be followed by death of the whole or part of the member. For these we have the opening of the sac, the ligature of the artery, and compression, to be adopted as circumstances may require. But there is another set of cases, in which the aneurism is situated on a trunk, whose ramifications are confined within certain limits and devoted principally to anastomosis. To these the treatment may be applied. On the 9th of March 1864, a patient was brought into the operating theatre by M. Nélaton. His history was a curious one:—In 1849, the sciatic artery was ligatured at its exit from the pelvis by M. Sappey for aneurism of that artery, situated several inches below this point. From the description of the case by that surgeon it was learned that the pulsations ceased at the time, but returned in fifteen days and continned; and also that the patient had refused to submit to the laying open of the sac, which was afterwards proposed. The aneurism had since remained stationary till a few weeks before he consulted M. Nélaton. It had during this last period enlarged slightly, and on three occasions had bled with some violence. Once, indeed, the surgeon called in had very great difficulty in arresting the hæmorrhage.[1] M. Nélaton performed the injection of perchloride of iron. A very small trochar was thrust into the tumour, and several drops of blood were allowed to issue by the canula to make sure that it was in the liquid part. An aortic tourniquet was then screwed down, and, when it was ascertained that all pulsation had ceased in it and in the femoral arteries, twenty-five drops of perchloride were injected. The result was admirably successful. The compression of the aorta was maintained for fifteen minutes, and when it was taken off no pulsation returned in the aneurism. On the third day there was a little pain in the tumour, but this passed off, and in a month it was reduced to a small hard lump, which would doubtless long remain.

The choice here lay between injection, opening the sac, and ligature of the internal iliac, for a repetition of that already performed by M. Sappey held out but slight chance of success. Of these the proper course was, I think, chosen. Ligature of the iliac is an operation at once difficult and dangerous in the highest degree.

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[1] The case in this respect is interesting, as the openings were quite healed at the time of operation. Mr Syme has related a curious case of prolongation of life after external rupture of an aneurismal sac, and many authors have recorded internal ruptures that did not prove fatal. I once carefully dissected an aortic aneurism which had burst into the pericardium, but was unable to discover the opening. The patient had died slowly from impediment to the heart's action by about eight fluid ounces of blood.

Nearly half the cases have been fatal in their results. Laying open the sac would no doubt have been safer; but those who have seen Mr Syme perform his wonderful operations, must acknowledge that such require a man of great skill and resolution, and are not devoid of great danger. I am of opinion, in short, that the injection was the safest of all, and it was certainly very successful. Had it failed, it could have been repeated; and this M. Nélaton had done in a former case of sciatic aneurism. Had it excited too great inflammation, as has never yet happened, the sac could have been laid open, and if the arteries were not occluded, they could then have been ligatured. I witnessed also the successful cure by this method of an aneurism of the facial; but it is needless to relate it in detail. It might, I believe, have been cured either by compression or ligature of the artery; and the preference would be given according to the surgeon's belief in their respective innocuity.

The treatment of stricture of the urethra differs in France and in Great Britain. In both countries the majority of surgeons use dilatation for the mass of strictures, and resort to other operations only when the stricture is resilient or resists the ordinary means. At the present day, in Great Britain, such operations are reduced to two:—the external division, as practised by Mr Syme; and the more recent method of splitting strictures, invented by Mr Holt. But while with us the internal division is almost entirely abandoned, it is by no means so in France, and a great variety of instruments have there been invented for the purpose. Of these the two most generally employed are those of M. Civiale and of M. Maisonneuve, —the former cutting from behind forwards, the latter from before backwards. Both of these surgeons use their instruments in every case,—M. Civiale as an adjunct to the process of dilatation, M. Maisonneuve as a substitute for it. Thus, when the urethra has been dilated almost to its normal calibre, M. Civiale says to his patient, "In order to make your cure more lasting and effectual, there is a little operation, absolutely without danger, which ought to be performed." M. Maisonneuve, who does not attempt dilatation unless the stricture is so very little of a stricture that it admits No. 8 or 9 bougie, addresses his patient with the same assurances of safety, and proposes his operation as immediately effectual. Of the two, the instrument of the latter is preferable, as it can be introduced into strictures of a very narrow bore, while the other can only be used where already a No. 6 could be passed. As to the danger, I really believe it is but slight, at least with the urethrotome of Maisonneuve. In several cases operated on by M. Civiale, I witnessed no deaths; but two patients were brought to the very edge of the grave by what was called urethral fever, a disease which I was unable to distinguish from pyæmia. Of about twenty cases of stricture divided by M. Maisonneuve, or by others with his instrument, all, with one exception, were free from accident. This one, which occurred in the practice of M. Desormais, was complicated by enlargement of the prostate, and had, I believe, been performed on the occasion of retention of urine. Some days afterwards the catheter was taken out, and its removal was followed by the issue of a considerable quantity of pus. It was at first believed that this was caused by the division of the stricture. The patient, however, complained of no pain at that part even on pressure, and it was discovered by rectal examination that the prostate had regained its normal size. The pus accordingly was attributed to a ruptured abscess of that gland. It is difficult without accurate data to compare this method with those used in Great Britain. The external division is so effectual that one would be disinclined to substitute for it any other not evidently more safe; and if dilatation should in some instances be replaced by something more speedy, that of Mr Holt is certainly the most convenient, and in all respects the best. By the urethrotome of M. Maisonneuve, I feel sure that the urethra is scratched or cut in other points than those of stricture, as is proved by the exclamations of the patient before the cutting edge reaches the contracted part. This is certainly an enormous disadvantage, and one which is sufficient to condemn the operation as unfit for ordinary use. I can conceive it might be employed when, after failing to reach the bladder by the ordinary means in a case of retention of urine, the surgeon had by chance succeeded in passing the little elastic bougie by which the cutting part is preceded.[1]

I had intended to make remarks on the subperiosteal excision in cases of acute necrosis, and on the treatment of lachrymal tumours; to relate some interesting individual cases which occurred in the practice of various surgeons during my stay in Paris, such as the reduction of a dislocation of the cervical vertebræ resulting from disease, the internal division of stricture of the œsophagus; some curious cases of adenoid tumour of the mamma under the care of M. Velpeau, and one of artificial anus under M. Nélaton; and to touch at the same time on the treatment of fractures, and the erroneous ideas that obtain in reference to some particular amputations. Let me merely remark, in passing, that the ankle-joint operation of Mr Syme is virtually done away with in disease, by the belief that in caries of the tibio-tarsal articulation, amputation should be performed in the lower part of the leg, while in diseases of the other tarsal bones partial removals ought to be effected. I witnessed several cases admirably suited for the ankle-joint, in which stumps, every way inferior, were produced by the methods of Chopart or Lenoir. I attribute this greatly to the fact that when the operation has been practised, it has been modified for the worse according to mistaken notions. This paper, however, has already extended so far that I must pass at once from these points to notice very shortly, before concluding, the after-treatment of patients in Paris.

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[1] The caoutchouc catheter, now a good deal used in Paris, after such operations, and in cases of enlarged prostate, ought to be known and used in this country. So much mischief is often done in retention from prostatic enlargement, by attempts to pass metal instruments, or gum-elastic ones armed with stilets, that every surgeon ought to be provided with the caoutchouc. They are much easier to introduce than the gum-elastic, which indeed is a somewhat useless instrument.

Not long ago a discussion arose in the French Academy on the cause of the very high mortality of the major operations in Paris. As to the fact all were agreed, and as to the cause many were of opinion that the superior success in England was due to the better stamina of the patients. It might perhaps be accounted for in other ways; and among these might be reckoned the overcrowding of hospitals, the tendency to avoid fresh air or anything like an open window, and generally the hygiene and after-treatment of the patients. However this may be, the mortality has brought about two results very worthy of notice: on the one hand, what is very creditable to the surgeons, the avoidance of such operations as are found to be fatal; and on the other, the invention of the various substitutes for the bistoury, of which I have spoken. Out of evil comes good; for the cure of many a joint disease has resulted from the former; and from the latter, methods which, if not more safe than the knife, are occasionally useful adjuncts to it. It would, however, have been productive of benefit had the Parisian surgeons bestowed more of their care and great ingenuity on the after-treatment rather than on operative modifications; for it is in this respect, it seems to me, that British surgery may very favourably be contrasted with Continental. Although some of the Parisian hospitals are constructed in all respects on the most perfect principles, in them as well as in the older ones but little use is made of their advantages. Especially in winter, all fresh air is as a rule most carefully excluded, and on entering the wards one is at once struck by the excessive heat and close smell which prevails. The Hôpital Lariboisiere is an exception; and to this, not to his use of the écraseur, I attribute the generally healthy character of the wounds in the wards of M. Chassaignac.

Again, in diet, much too great reliance is placed on the antiphlogistic system. After an operation the patient is by most surgeons almost starved; and in one case, when we would have poured in nourishment and stimulants to tide the man over a dangerous crisis, I heard the surgeon so far relax as to order a little "bouillon." In proper circumstances low diet is most valuable, but after a severe operation there is usually in hospitals less danger in erring on the other side.

While thus in constitutional treatment French surgeons appear to me greatly to err, they do not less so in their local applications.

The most remarkable variety exists in the dressing of wounds, but almost always the methods are cumbersome and complicated. Take the most common form for a stump. A piece of linen, drilled with numerous holes, is smeared with oil or ointment and laid over the wound. Over this is placed a thicker cushion of charpie, wet or dry, with "compresses," that is, folded rags, above it; and to keep the whole in position, a coarse bandage is wound round and round almost *ad infinitum.* Except by some unorthodox men, the dressing is not changed oftener than once a-day, even when the discharge is very great; and it may be, a much longer interval is allowed to elapse. Modifications are made in the ointment according to the state of the wound; and in other respects, as with us, the sulphate of copper, or such like stimulant, is used if required. It must be at once evident how very inferior this is to a more simple dressing. The idea, in fact, of letting alone, and merely protecting from dust, etc., by a light layer of lint, and from motion by attention to position, is almost unthought of in Paris. Ointments and greasy applications, which might be applied to keep a sore open, are in daily use, and often contain concoctions worthy of a Middle-age quack; and the wound is kept in a bath of its own discharge from the closeness of the dressing. But while this is generally true, it must be said that several surgeons, seeing the error of this treatment, have adopted others free from its disadvantages; and doubtless the next generation will abolish it. Thus M. Chassaignac (we shall leave out of account his "pansement par occlusion") simply paints an open wound with weak nitrate-of-silver solution, and covers it lightly with charpie. The drainage tubes which this surgeon has introduced, and which he uses extensively in the after-treatment of stumps, and indeed in all cases when a free discharge is required, seem to me extremely useful. In stumps, for instance, the want, which Mr Syme occasionally supplies in the ankle-joint operation by making a hole in the flap, might be supplied by a tube, and the same is the case in the linear method of excising the elbow or shoulder. M. Nélaton, for stumps, employs alcohol largely diluted. M. Maisonneuve, although he still uses the cerates, commonly dresses with a little charpie steeped in glycerine, an admirable application; or for foul sores in carbolic acid, a disinfectant of considerable power. Most, however, adhere to the dressing first mentioned. The consequence is, that what union by the first intention does take place is speedily destroyed by accumulated pus, and accordingly, except in wounds of the face, is rarely looked for, if not expressly avoided. Often, indeed, in amputations or excisions of tumours, so far from the edges being brought together with a view to secure a speedy union, they are left purposely wide apart, or even with charpie interposed, to make sure of granulation only. The stumps are therefore slow of healing; when healed have a broad cicatrix: and we sometimes see men with the long stump of an amputation in the lower part of the leg, who are obliged to walk on the knee with an inconvenient appendage protruding behind them. What, then, we borrow from French surgery we must owe to the ingenuity and address in devising and executing operations for the cure of disease, and of these I have mentioned several from which something may be learned. The observation of their treatment when the operation has been completed is of use, as serving to demonstrate the admirable powers of nature in producing good results even under the most disadvantageous circumstances.

ARTICLE IV.—*On the Statistics of Puerperal Insanity as observed in the Royal Edinburgh Asylum, Morningside.* By Dr J. B. TUKE, Medical Superintendent of Fife and Kinross District Lunatic Asylum.

(*Read before the Medico-Chirurgical Society, 5th April* 1865.)

THE following paper was drawn up a short time ago, more for the purpose of satisfying my own curiosity than for publication. I was anxious to discover how far the records of our own asylum corresponded with the published results of other hospitals for the insane, and therefore gathered together from the case-books of the Royal Edinburgh Asylum, 155 cases of so-called puerperal mania. All these cases have been admitted within the last eighteen years, and, with very few exceptions, the history of each patient has been most carefully reported in the case-books of the institution by the various resident physicians. As an earnest for the accuracy of the details, I may mention the names of Drs Skae, Howden, Wingate, Sherlock, Yellowlees, Clouston, etc., as those attending and reporting the several cases. Having collated these 155 cases of puerperal mania, it appeared to me that it might not be unimportant to place the results on the records of this Society; and I therefore submit to you an unambitious paper, as it lays claim to no originality, but one, I trust, not entirely uninteresting, as illustrating the practice of our metropolitan lunatic asylum in one of the most interesting forms of insanity.

Before proceeding further, I must state that a very large proportion of the puerperal cases here spoken of were of the most severe character, such as could not be treated at home, whose violence or long continuance necessitated their removal to an asylum. The milder forms of the disease we seldom, if ever, see, such being treated at home under the care of the family medical attendant. The results of treatment, therefore, of puerperal insanity *as a whole*, are not set forth in this paper; it merely demonstrates the lesson to be learnt from the experience of a hospital, whose patients have suffered from the malady in its full force.

Although these 155 cases have been spoken of generally as suffering from puerperal insanity, I do not propose to regard them all as such, but rather to divide and arrange them into three groups,—a step, I cannot help thinking, warranted by a variety in symptoms and pathology.

In works on Midwifery and Mental Diseases we find the several forms of insanity which occur during pregnancy, follow parturition, and supervene on lactation, all arranged under the common head of Puerperal Mania. This, with regard to the first and third divisions, is of course a misnomer, a contradiction in terms; and it seems rather curious that it should have been so long adhered to, more particularly as it tends to confuse and almost stultify deductions from the few statistics of puerperal mania of which we are possessed. For instance, any comparison drawn between any given number of labours and any given number of so-called puerperal cases, must lead to erroneous conclusions, if the insanity of pregnancy is confounded with pure puerperal mania, or if, as is the case, the anæmic insanity of lactation is confounded with either. To obviate this mistake, I have drawn up a series of tables, in which the particulars of the three forms of insanity are separately tabulated under the heads of—

Insanity of Pregnancy.
Puerperal Insanity.
Insanity of Lactation.

That almost all authors on this subject, from Gooch downwards, and more especially Pritchard, have admitted this distinction, I do not deny; but I think I may venture to say, that no series of cases has as yet been recorded in such a way as to bring out prominently the characteristics of each group distinctively. All the cases in which well-marked insanity occurred during pregnancy are placed under the first head. In many of the cases of the second class, or true puerperal insanity, a slight depression of spirits previous to confinement is recorded, but not more than is so frequently observable amongst expectant mothers, and not sufficient to prove the actual existence of insanity. There are, on the other hand, cases in which insanity was fully developed prior to, but became aggravated after confinement. These are all classed under the head of Insanity of Pregnancy, as the cause was, we are entitled to hold, the systematic changes of that period, although the further disturbance set up by childbed may have changed the type of symptoms, and, as occurred in four cases, violent mania supervened on the melancholia of pregnancy.

Between the 1st of January 1846 and the 31st of December 1864, there were 2181 female cases of insanity treated in the Royal Edinburgh Asylum. As above stated, 155 were so-called puerperal cases, making a per-centage of 7·1; but, after careful revision of each case, and acting on the principle of classification just alluded to, the following appears to be the correct subdivision:—

Insanity of Pregnancy,	28
Puerperal Insanity,	73
Insanity of Lactation,	54

The first group, Insanity of Pregnancy, thus bearing a per-centage of 18·06 on the total of 155: the second, Puerperal Insanity proper, 47·09; and the third, Insanity of Lactation, 34·8.

INSANITY OF PREGNANCY.

Column 1 of Table I. (p. 1025), shows the ages at which the attack took place. The proportion of cases occurring between 30 and 40, as compared with those between 15 and 30, is somewhat remarkable, as showing that the tendency to insanity does not decrease with the age of the fertile woman. In the last two cases, occurring at 43 and 44, the patients had suffered from previous attacks of like nature, the latter having recurred after a lapse of thirteen years.

Of more importance perhaps is column 2, which shows during which pregnancy insanity developed itself. It will here be seen that primiparæ are by far the most liable, a circumstance which might have been expected, when we take into consideration the moral exciting causes, anxiety and dread of the coming event, which exist to a greater degree in the inexperienced woman. The case occurring at the eighth confinement was a recurrent one, the patient having suffered from two previous attacks of puerperal insanity.

In only 19 cases was it possible to ascertain with certainty the exact month of pregnancy at which the mental symptons first appeared. Of those not tabulated, 4 are reported as having begun to be insane during the last months of pregnancy. Of the remaining 19, 3 showed symptoms of mental derangement during the third month, 5 during the fifth, 1 during the sixth, 9 during the seventh, and 1 during the eighth. The fact is not without interest, that the great majority of the attacks occurred at those periods of utero-gestation which are generally considered critical.

In the insanity of pregnancy the symptoms are as a rule of a melancholic type, 15 out of the 28 cases being recorded as melancholia, and 5 as dementia with melancholia. In no form of insanity is the suicidal tendency so well marked: 13 patients, or nearly half, had either attempted or meditated suicide. In some the attempts were most determined, a loathing of life and intense desire to get rid of it being the actuating motives. In the melancholic cases we can frequently trace back the delusions to the morbid fears, restlessness, capriciousness, and irritability of the pregnant woman, which, becoming exacerbated, amount to actual insanity, and prompt the unhappy victim to self-destruction. One patient, who was admitted after attempting suicide, and who made a rapid recovery, became insane during her next pregnancy; her friends hoped to be able to manage her at home; the result was she succeeded in poisoning herself. In another case the patient came to the asylum praying for protection against herself, and another felt the strongest desire to kill her child. Mania with exaltation is reported twice, and both these cases are unpromising. One is now under treatment, and although a slight improvement manifested itself immediately after labour, the mania soon returned, and, it is feared, is now degenerating into dementia.

Moral insanity is by no means unfrequent, dipsomania being the most common symptom. This generally occurs during the earlier months of gestation, and is probably also only an aggravated form of the well-known morbid craving or longing for particular articles of food which characterizes the earlier months of pregnancy. As it advances it increases in intensity, and gives rise to actual delusion and attempts at suicide. In two cases the moral perversion was evinced by a homicidal impulse. I might enumerate the particulars of many other cases, but content myself with reiterating the statement, that melancholy and moral perversion are the leading symptoms in the insanity of pregnancy.

Another interesting fact connected with the mental derangement of pregnancy is, that hereditary tendency has been recorded in 12 out of 28 cases. The predisposition in 9 out of these 12 was ascertained to be on the female side of the family in the following relationships: in 3 cases the patient's mother either was or had been insane; in 2, maternal aunts had been similarly affected; in 3, sisters had been patients in lunatic asylums; and in 1, several members of both sides of the house were liable to attacks of mental derangement. It is not improbable that the predisposition existed in some of the other cases, but was either not admitted by the relations or could not be ascertained.

As to the liability of recurrence. In 18 cases the existing attack was the first, in 5 it was the second, in 2 the third. Of these 7 recurring cases, 3 were known to have suffered from melancholia during previous pregnancies, 3 from pure puerperal mania, and 1 from insanity during her last nursing.

This form of insanity is very amenable to moral treatment in its earlier stages; on referring to column 5, you will see that 19 cases recovered under six months. The assurance of protection, the regularity, amusements, and employment alone to be found in an asylum,—above all, the freedom from domestic anxiety and the misapplied sympathy of relatives,—in a large majority of cases are productive of the best results. It is impossible for the class from which the large proportion of our patients is derived to obtain these advantages elsewhere, and I am by no means certain that change of scene, procurable by those of the higher class, is not counterbalanced by the home influence, and the craving for sympathy only increased by the efforts the patient knows are being made in her behalf.

Of itself it is not fatal. One patient died three years after admission of phthisis. What is most to be dreaded is the attack becoming chronic, or degenerating into dementia. Of the five cases in which this last was the result, acute mania supervened on labour, and terminated in hopeless imbecility. None of these cases, however, were admitted into the asylum till some months after their confinements, when the disease was firmly seated and incurable.

II. Puerperal Insanity.

The number of cases of pure puerperal insanity amounts to 73. On glancing at the first column of Table II. (on page 1026), you will see that the same remark as to the increase of mental alienation in proportion to advancing age holds good in puerperal insanity as well as in that of pregnancy. Of the 28 cases whose age was 30 and upwards, 8 were primiparæ,—a fact suggesting the increased liability to mental derangement of women who become mothers for the first time at that somewhat advanced period of life. I endeavoured to elaborate column 1 so as to show the relation of age to confinement, but found that it would cover pages of print. The general result, however, was, that if we take into consideration the rarity of first confinements after 30 years of age as compared with those occurring previous to that period, an increase of liability is shown to exist. Again, if we compare the amount of cases between 20 and 30 (forty-one) and those between 30 and 43 (twenty-eight), we come to the same conclusion as to this form of insanity as a whole.

Column 2 confirms the observation so often repeated that the first confinement is the most dangerous, and also that the peril diminishes with each subsequent labour. In 9 cases the exact confinement was not recorded, but the fact that they were multiparæ was ascertained.

In 15 cases the attack was recurrent. In 1 case the patient had suffered in a like manner after 3 previous confinements, in 4 cases from 2 previous attacks, and in 10 from 1. There is a female at present resident in the asylum who was the subject of an attack of puerperal insanity many years ago. She has subsequently suffered from derangement in the shape of acute mania on 6 occasions, but none of these could be traced to any form of so-called puerperal mania, although she has born several children in the interim. The attack for which she is now under treatment appeared five years after the birth of her last child. There are other instances of a like nature in the case-books of the asylum, which prove that an attack of puerperal insanity tends to produce a constitutional susceptibility to other forms of mental alienation.

In column 4 (on page 1026), you will find tabulated the period after birth at which insanity developed itself. You will see that in only 2 instances were the mental symptoms evinced at a later period than one month; thus confirming the observations of most writers on the subject. In one case, 6 and in another 10 weeks elapsed between labour and the commencement of the maniacal symptoms; both these patients had suffered from profuse hæmorrhage following labour, and were admitted in a very weak state, a considerable discharge from the vagina helping to exhaust their systems. Of course it is open to question how far these patients should be regarded as the subjects of puerperal insanity, as it is possible that asthenic insanity would more properly designate their ailment, but still, from the strong presumptive evidence, I have classed them under the former head.

This table, taken into consideration along with the following one, shows some curious results. All the cases where melancholia was the prevailing symptom developed their insanity beyond sixteen days after labour, all occurring before that period being evidenced by acute mania; dementia and melancholia characterized the derangement appearing towards the end of the month after labour, and proved the most incurable form of the disease. Mania, as a rule, was violent, the patient being noisy, restless, sleepless, and occasionally afflicted with hallucinations of the various senses. The suicidal tendency was very strongly marked, 25 patients having either attempted or expressed a desire to commit self-destruction. As a rule, however, this propensity did not last long, but abated with the violence of the symptoms. The suicidal tendency of the puerperal differed also from that of the pregnant patient in assuming the character more of impulse than the result of a perverted train of thought. In three violent cases of puerperal mania I have noticed an extraordinary amount of salacity a very few days after labour; masturbation was excessive in all these patients, but whether this was the result of the irritation consequent on the labour causing perverted sensation or actual salacity, I cannot say.

In three cases also I have found albumen in the urine. These cases were all admitted within a week of confinement, and were all of the most violent class of maniacs. In one, profuse hæmorrhage had followed labour. She was perhaps the most violent of any; in the other two, the labours had been quite natural. The urine was only slightly clouded with albumen, and disappeared within eight days in each case. They all had rapid pulse, moist skin, and violent delirium, and were in imminent danger of life on admission. The symptoms abated in from ten days to a fortnight, and although in each instance the mind remained affected for some months, they all recovered. Unluckily, I could never obtain any very accurate history of the symptoms in these cases, and the mania was so extreme when I first saw them as to mask any head symptoms, and prevent the patient from giving any account of herself. Notwithstanding this they agree in many respects with the cases reported by Dr Donkin of Newcastle (reported in the Edinburgh Medical Journal for May 1863), illustrative of Professor

Simpson's theory, that a certain type of puerperal mania is produced by uræmic poisoning. They are by no means conclusive, from the deficiency of the history previous to admission, but afford strong presumptive evidence as to the truth of the doctrine.

Hereditary tendency was proved in 22 cases, and as in the insanity of pregnancy, was traceable to the female side of the family much more frequently than to the male.

In 9 cases the labour had been instrumental, in 4 tedious, in 2 twins had been born, in 6 profuse hæmorrhage had succeeded labour, and in 2 the child was stillborn.

This large proportion of 23 complicated labours out of a total of 73 indicates the tendency of unnatural parturition to produce insanity. The various irregularities doubtless operate in different ways, those where the suffering has been long continued depressing the nervous system directly, those in which large quantities of blood have been lost, producing anæmia of the brain, and in the case of the child being stillborn, a moral shock acting on a mind naturally predisposed to the affection. In the patients where this latter was the result, insanity appeared very shortly after the termination of the confinement.

You will see in the table of symptoms one case set down as epileptic mania. This patient took a fit immediately after the birth of the child, a maniacal paroxysm followed, and at every menstrual period since she has been similarly affected.

Two cases are reported as having had chloroform administered during childbed, a number so small as to give the strongest denial to any absurd theory regarding the danger of its exhibition.

The relative proportion of unmarried to married women afflicted with this disease is less than that reported by most asylums. They stand as 13 to 73, nearly a sixth. This may be accidental, but the moral causes, shame and disgrace, must have considerable influence over the development of this affection. Curiously enough no single patient is reported as having an hereditary predisposition.

The Table of Results shows a per-centage of 76·7 of recoveries, 10·9 of deaths from all causes, and 9·5 of patients who became demented. Two were discharged relieved, both of whom recovered, thus raising the per-centage of cures to 79·4. The period of residence in the recovered you will find in column 7; the very large proportion were discharged before six months; some of the 10 whose stay in the asylum amounted to nine months might have been dismissed sooner, but remained voluntarily as a matter of precaution. The chances of recovery of reason at a later period than six months rapidly diminish. Generally speaking, if the mania is prolonged more than a month, or at the outside six weeks after confinement, the probabilities of ultimate cure are very faint. Within my own experience, the following is the most usual train of symptoms in cases who recover their reason. Within three weeks, or more frequently earlier, the mania gradually subsides, and is replaced by a state of dementia, generally accompanied by delusions, which almost invariably assume the form of mistaken identity. These gradually disappear, leaving a haziness of apprehension and a state suggesting the idea of waking from a dream. The patient can now generally be induced to work, and otherwise employ herself. From that moment you may look with almost certainty to ultimate recovery.

I have inquired of several convalescent patients how much they recollect of the incidents of the last few months or weeks. The account usually given was, that memory was lost for a time, —the period of the maniacal paroxysm,—but they have always given a wonderfully accurate account of proceeding subsequent to its disappearance. The delusions are very vividly impressed on the recollection, and I have been told by convalescents that it was a long time before they could argue themselves out of them. It is intensely interesting to watch the gradual re-assertion of reason; to me it is difficult to conceive a greater amount of satisfaction to be derived from the successful treatment of any other disease. In the four patients who recovered after a period longer than nine months the delusions were peculiarly persistent, but ultimately disappeared. Three of them were cases of melancholia. I may here mention that this symptom is much less amenable to treatment than acute mania, and more frequently degenerates into dementia. Puerperal melancholia amongst the cases now under consideration appeared far on in the month succeeding labour, and was not, except in two instances, accompanied by a suicidal tendency, the majority of the patients being weak anæmic women with a tendency to phthisis. The train of symptoms was eminently asthenic, and the proportion of recoveries much lower than where mania was the leading symptom. Within the last two months, and since this report was commenced, 2 cases of puerperal insanity have been admitted into the asylum, both of which give me the impression of being subjects of a degree of congenital imbecility. One is maniacal, the other demented, and both are, I fear, incurable. They have small badly-shaped heads, and bear the stamp of a low type of organization. Puerperal insanity in cases like these is most hopeless, and renders the subject of it the inmate of an asylum for life.

Moral causes are frequently assigned for the attack: fright and sudden shocks being the most common. In two instances where this is reported the lochial discharge was said to have been arrested, and mania to have set in shortly afterwards.

Cold, chills, the taking of spirits shortly after confinment, are also reasons given for the appearance of insanity.

Of itself, puerperal insanity has been by no means fatal in the practice of the Royal Edinburgh Asylum, only 2 deaths being clearly traceable to it, both of which took place within a week of admission. They both presented the unpromising symptoms of quick pulse, jactitation, intolerance of sound, and that rapid catching at trifles said or done which is frequently seen in delirium tremens. They died of exhaustion, one in three, the other in seven days after admission. No post-mortem examination was permitted. These cases were in all probability instances of the puerperal paraphrenitis first mentioned by William Hunter, but as no post-mortem examination was permitted of either patient, the diagnosis could not be verified. Another patient died in two days after admission from pelvic cellulitis, and eleven days after labour. In her case the latency of symptoms so common in disease amongst the insane was very remarkable; no evidence of inflammation was afforded after her admission, and the eminent physicians who had attended her previously, did not suspect the existence of the cellulitis, but to such an extent had it advanced, that, on post-mortem examination, a large quantity of pus was found in the pelvic cavity. The brain presented no abnormal features. A thin deposit of osteophyte was present. Two more patients died shortly after admission, both cases complicated with inflammatory affections: in one bronchitis, in the other peritonitis, was the immediate cause of death. This must induce one to look upon cases of puerperal insanity complicated with inflammatory diseases with great suspicion, and to be most guarded in making a prognosis.

The remaining fatal cases succumbed to phthisis after a prolonged residence in the asylum, and cannot be fairly taken as resulting from the primary disorder.

I have, lastly, placed in your hands a table, which, I think, illustrates the necessity for early hospital treatment in this disease. With the exception of those who were actually moribund on admission, and 2 who became demented, all the patients who were placed under treatment within one month after the development of insanity recovered. The statistics of this form of mental alienation would present much more favourable results, if the patients were removed early to the shelter of an asylum, if we could induce the public to give up old-fashioned prejudices, and look upon such institutions as hospitals for the cure of a disease by no means unamenable to treatment in its earlier stages.

The system of treatment in the Royal Edinburgh Asylum has been very uniform. The support of nature to withstand the wear and tear of the disease is, of course, the most important; for this purpose the constant administration, artificially if necessary, of custard and beef-tea, little and often, has been found the most effective and convenient. Of late stimulants have been to a great degree discontinued in excited cases, as they appear to aggravate the mania, without affording any permanent support to the system. In melancholic cases, on the other hand, a limited amount of wine or whisky is most beneficial. Sedatives in whatever shape are looked upon with distrust, for, however much they may subdue the intensity of the symptoms, it is believed that they prolong the duration of the paroxysm. In one case, in which cannabis indica was exhibited in large doses, I have to confess that it is my belief that the patient was far from benefited; on the contrary the period of dementia with delusions was unusually long, although she ultimately recovered her reason. But again, on the contrary, in some of the women whose cases are characterized by restless melancholy or dementia, the administration of large doses of morphia has been accompanied by the very best results; under this treatment a few cases of this nature have recovered rapidly. Nature seems to indicate the remedy; in those where there is great intolerance, as evidenced by sickness and vomiting, I have rarely seen happy effects produced by pressing the drug. Sedatives in large doses have been strongly recommended by some authors at the commencement of the attack, with the view of arresting it; of this I cannot speak, but feel certain that such a course, pursued after the mania has established itself, will not be found successful in the great majority of cases.

INSANITY OF LACTATION.

Having gone perhaps rather too much into detail in the cases of pure puerperal insanity, I will run over more quickly the various columns of the table illustrating the insanity of lactation.

In this form also I must direct your attention to the large proportion of cases occurring after thirty years of age, and likewise to the rarity of the disease supervening on the first nursing, as seen in columns 1 and 2 of Table III. (p. 1028). Unluckily no record has been kept in 14 cases as to the nursing during which the attack appeared, but all these patients are spoken of as multiparæ. In 2 of the cases where the insanity appeared during the first nursing, profuse hæmorrhage had succeeded labour, and in 4 others the patient was described as a weak, phthisical, or anæmic woman.

Column 3 demonstrates the danger of too prolonged lactation; the very large proportion of cases having occurred after the sixth month of nursing. In only 41 cases do the books show the period of lactation at which insanity developed itself, the expression "during the last nursing" being merely used; but in every case the mental derangement is traceable to over-lactation. In not a few patients one labour had succeeded another so rapidly as to have weakened the constitution, and in others, the immunity from pregnancy sought for by keeping the child long at the breast, had been dearly paid for by an attack of insanity.

Acute mania evidenced the insanity in 10 cases, melancholia in 39, and dementia in 5. The acute mania, as a rule, in this form of insanity is severe but evanescent; it rarely lasts more than 10 days or a fortnight, and is generally attended with hallucinations of the different senses, and delusions, as in puerperal mania, of mistaken identity. A patient lately discharged from the asylum was excessively noisy, violent, and sleepless. On admission she mistook me for a doctor who had been attending her, and whom she much disliked, and accordingly, whenever I visited her, I was the object of her most unlimited abuse. This continued until the tenth day. On the evening of that day, on paying my last visit, I saw that a change had taken place; on speaking to her I found she was perfectly calm and composed, and, as far as I could judge, sane; she apologized for all the bad language and abuse she had given me, and said that she recollected perfectly well the incidents of the last few days, and explained her reasons for her apparent dislike of me. She never showed another bad symptom, and was discharged in three weeks perfectly recovered. Of course, this is an exceptional case, a degree of dementia almost invariably being left for a few weeks after the acute mania passes off.

The melancholia which characterizes the insanity of lactation is of various degrees of intensity; but where a suicidal tendency is evinced the attempts to carry out the purpose are most determined. Occasionally the duration of the attack is not greater than three weeks or a month; in such cases a degree of hysteria is generally present.

In almost all cases of insanity of lactation which have come under my notice during the last two years, exophthalmia and bruit de diable have been marked symptoms, increasing in intensity with the period of nursing. As these bodily symptoms disappeared so did the mental, such cases almost always recovering.

Of itself, this form of insanity is not fatal, only one patient having died after several years' residence. The degeneration of the mental symptoms into dementia is more to be feared, as that was the result in 12 out of the 54 instances; and the two who are returned as still under treatment are not promising cases. Here, for the third time, I must reiterate the statement, that unnecessary delay in sending the patient to an asylum was the chief cause of this unhappy result. In a disease so essentially anæmic as the insanity of lactation, hospital treatment is absolutely necessary amongst patients derived from the lower or lower middle classes of society. It is somewhat surprising to see the marks of treatment on many patients on admission. One of the cases mentioned in column 4 as now under treatment came to the asylum about six months ago. She had the marks of blisters behind her ears, on the nape of her neck, on her temples and forehead, and she was profusely salivated. We had long great hopes of her recovery, but I very much fear her wits have been blistered and calomeled away. This of course is an extreme case, but we not unfrequently meet with gross ignorance of the nature of the disease; for instance, tartar emetic is frequently reported to have been given to reduce the violence of the paroxysm in this and other anæmic forms of insanity. The period of residence of those recovered is seen in column 6; they all recovered within a year. The length of time under treatment was generally proportioned to the degree of anæmia under which the patient laboured on admission.

In conclusion, I will merely recapitulate the leading points which appear to me to be brought out by the tables under consideration.

1st, That an increase of liability to insanity exists between the ages of 30 and 40 in child-bearing women, and that first confinements occurring at that period are peculiarly frequently followed by true puerperal insanity.

2d, That primiparæ are more commonly the subjects of the insanity of pregnancy and puerperal insanity than multiparæ.

3d, That the insanity of pregnancy in the majority of cases is developed during the third, fifth, or seventh months.

4th, That the insanity of pregnancy is generally evidenced by melancholia or moral perversion, and that it is very curable.

5th, That the hereditary tendency is peculiarly traceable in these three forms of insanity, and that in a large proportion of cases it exists on the female side of the family.

6th, That puerperal insanity leaves a tendency to other forms of insanity.

7th, That the puerperal insanity characterized by melancholia rarely commences until nearly a month after labour.

8th, That a tendency to suicide is a very frequent symptom.

9th, That complicated labours are more frequently followed by puerperal insanity than natural ones.

10th, That cases of puerperal insanity, in which acute mania is the leading symptom, are more amenable to treatment than those in which melancholia exists.

11th, That the insanity of lactation does not ensue on the first nursing so frequently as on subsequent ones, and the longer the child is kept to the breast the liability necessarily increases.

12th, That the insanity of lactation is more transient than either of the other forms, and that where evidenced by acute mania is less persistent than where melancholia exists.

13th, That delusions as to personal identity are very common symptoms in the three forms of insanity.

14th, That none of these forms of insanity are of themselves very fatal, except when complicated with other and especially inflammatory diseases. That they are all very amenable to treatment when such treatment is adopted early, and that the longer the patient is deprived of the benefits of an asylum the chances of recovery decrease.

15th, That this type of disease was anæmic in these three forms of insanity, which indicated the administration of a highly nourishing diet, but a very cautious use of stimulants.

16th, That the exhibition of narcotics is not beneficial where the leading symptom is acute mania.

TABLES ILLUSTRATIVE OF PUERPERAL INSANITY IN THE ROYAL EDINBURGH ASYLUM.

Total of so-called Puerperal Cases, . 155

SUBDIVISION.

Insanity of Pregnancy, ...	28
True Puerperal Insanity, ...	73
Insanity of Lactation, ...	54
	155

TABLE I.—*Insanity of Pregnancy.*

1. Ages at which Attack occurred.		2. Pregnancy during which Insanity supervened.	3. Month of Pregnancy during which Insanity supervened.
15 years,	1	In 9 cases during 1st pregnancy	In 3 cases during 3d month
19 "	2	4 " " 2d "	5 " " 5th "
21 "	2	3 " " 3d "	1 " " 6th "
22 "	3	2 " " 4th "	9 " " 7th "
23 "	1	2 " " 5th "	1 " " 8th "
25 "	1	1 " " 6th "	9 not recorded
26 "	3	1 " " 8th "	28
29 "	4	6 multiparæ the number of pregnancy not recorded	
31 "	2	28	
32 "	2		
34 "	1		
35 "	1		
36 "	1		
39 "	1		
42 "	1		
43 "	1		
44 "	1		
	28		

4. Mental Symptoms.		5. Result.		6. Length of Time under Treatment.	
Mania, with Exaltation, .	2	Recovered, .	21	Under 3 weeks,	1
Melancholia, . . .	15	Died (3 years after admission), .	1	" 4 "	1
Dementia, with Melancholia,	5	Under treatment,	1	" 5 "	2
Dipsomania, . . .	4	Became demented,	5	" 2 months,	5
Moral Perversion, . .	2		28	" 10 weeks,	1
	28			" 3 months,	2
				" 5 "	2
				" 6 "	3
				" 2 years,	2
				" 3 "	2
				" 4 "	1
				Remain demented,	5
				Died, . . .	1
					28

In 13 cases suicidal tendencies were exhibited.
" 12 cases hereditary predisposition was ascertained to exist.
" 4 cases the patients were unmarried.

TABLE II.—*Puerperal Insanity.*

1. Age at which Attack occurred.		Cases.
At 20	in	5
21	〃	2
22	〃	2
24	〃	6
25	〃	11
26	〃	5
27	〃	3
28	〃	5
29	〃	2
30	〃	3
31	〃	1
32	〃	3
33	〃	3
35	〃	5
36	〃	3
37	〃	2
38	〃	3
40	〃	2
41	〃	1
43	〃	2
Not known		4
		73

2. Number of Confinement on which Insanity supervened.		Cases.
On 1st	in	34
2d	〃	16
3d	〃	5
4th	〃	3
5th	〃	2
6th	〃	2
8th	〃	1
9th	〃	1
Multiparæ, but exact number of confinement not recorded,	〃	9
		73

3. Showing tendency to Recurrence.

In 58	cases	the present was	1st	attack
10	〃	〃	2d	〃
4	〃	〃	3d	〃
1	〃	〃	4th	〃
73				

4. Period after Confinement at which Insanity developed itself.				Cases.
Under	1	day	in	9
〃	2	〃		6
〃	3	〃		1
〃	4	〃		4
〃	5	〃		4
〃	6	〃		3
〃	7	〃		10
〃	8	〃		3
〃	9	〃		2
〃	10	〃		4
〃	11	〃		1
〃	12	〃		2
〃	13	〃		2
〃	14	〃		4
〃	15	〃		1
〃	16	〃		1
〃	18	〃		2
〃	21	〃		2
〃	24	〃		2
〃	26	〃		1
〃	6	weeks	in	1
〃	10	〃		1
			In	7
the exact period is not recorded more particularly than "a few days after the birth," &c.				
				73

5. Symptoms (Mental).	
Acute Mania, .	53
Melancholia, .	15
Acute Dementia, .	4
Epileptic Mania, .	1
	73

6. Results.	
Died, . . .	6
Became Demented	7
Discharged Relieved,	2
Recovered, . .	56
	73

7. Period of Residence of those Discharged Recovered.			
Under	1	month,	2
〃	2	〃	7
〃	3	〃	10
〃	4	〃	5
〃	5	〃	6
〃	6	〃	12
〃	9	〃	5
〃	12	〃	3
〃	18	〃	1
〃	1	year,	2
〃	2	〃	1
			54

8. Causes of Death, and length of Residence of Deceased.		
1	Pelvic Cellulitis, .	2 days
2	Exhaustion,	7 〃
3	Peritonitis,	2 〃
4	Phthisis,	9 months
5	Exhaustion,	3 days
6	Phthisis,	2 years
7	Bronchitis,	8 days
8	Phthisis,	2 years

Puerperal Insanity—Continued.

Showing the Duration of Insanity previous to Admission.

9.

No. of Cases.	Insane previous to Admission.	Recovered.	Relieved.	Became Demented.	Died.
2	For 2 days	0	0	0	2
2	〃 3 〃	1	0	0	1
3	〃 4 〃	3	0	0	0
1	〃 5 〃	1	0	0	0
5	〃 6 〃	4	0	1	0
4	〃 7 〃	3	0	0	1
2	〃 8 〃	1	0	0	1
2	〃 9 〃	2	0	0	0
1	〃 10 〃	1	0	0	0
2	〃 14 〃	2	0	0	0
6	〃 21 〃	5	0	1	0
4	〃 28 〃	4	0	0	0
5	〃 6 weeks	5	0	0	0
4	〃 2 months	3	0	1	0
3	〃 3 〃	3	0	0	0
4	〃 4 〃	3	1	0	0
5	〃 6 〃	3	1	1	0
6	〃 9 〃	4	0	2	0
2	〃 1 year	0	0	1	1
2	〃 2 〃	0	0	0	2
8	not ascertained, of whom	8	0	0	0
73		56	2	7	8
					73

In 13 cases the patients were unmarried.
〃 22 cases hereditary predisposition was ascertained.
〃 25 cases suicidal tendencies were evinced.
〃 9 cases the labour had been instrumental.
〃 4 〃 〃 〃 tedious.
〃 2 cases twins had been born.
〃 2 cases chloroform had been administered.
〃 6 cases profuse hæmorrhage had succeeded labour.
〃 2 cases the child was still-born.

TABLE III.—*Insanity of Lactation.*

1. Age at Time of Attack.	2. Nursing during which Insanity appeared.	3. Month of Nursing during which Insanity appeared.
Age. No. of Patients. 19 . 1 20 . 1 22 . 2 25 . 4 26 . 4 27 . 5 28 . 2 29 . 5 30 . 6 31 . 3 34 . 4 35 . 3 36 . 3 37 . 2 38 . 2 39 . 2 40 . 2 42 . 1 Not known, 2 — 54	In 8 during 1st nursing. 4 〃 2d 〃 9 〃 3d 〃 5 〃 4th 〃 6 〃 5th 〃 2 〃 6th 〃 1 〃 7th 〃 1 〃 8th 〃 2 〃 9th 〃 1 〃 10th 〃 1 〃 12th 〃 14 cases the women were multiparæ, but exact confinement not recorded. — 54	In 2 during 3d month. 6 〃 6th 〃 4 〃 7th 〃 2 〃 8th 〃 6 〃 9th 〃 6 〃 10th 〃 5 〃 11th 〃 6 〃 12th 〃 2 〃 13th 〃 2 〃 16th 〃 13 cases month not recorded. — 54

4. Results.	5. Symptoms.	6. Length of Time under Treatment of those Recovered.
Died, . . 1 Became Demented, 12 Under Treatment, 2 Recovered, . . 39 — 54	Acute Mania, . . 10 Melancholia, . . 39 Dementia, . . . 5 — 54	3 weeks in 2 cases. 1 month 〃 3 〃 2 〃 〃 4 〃 3 〃 〃 6 〃 5 〃 〃 4 〃 6 〃 〃 5 〃 7 〃 〃 4 〃 8 〃 〃 4 〃 Over 9 〃 〃 7 〃 — 39

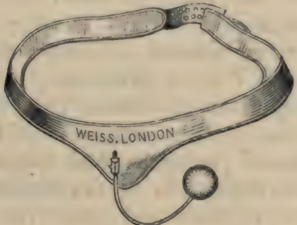
In 17 cases suicidal tendencies were evinced.
〃 14 cases hereditary predisposition was ascertained.
〃 2 cases profuse hæmorrhage had occurred after labour.

ARTICLE V.—*A Description of a New Form of Uterine Support in Prolapsus.* By CHARLES BELL, M.D., F.R.C.P.E., Etc.

(*Read before the Obstetrical Society, 22d February* 1865.)

WHEN we consider the peculiar means by which the womb is retained in its situation, the remarkable functions it performs, which have the effect of vastly increasing its weight and size, and also that it assists in supporting the superincumbent viscera of the abdomen, it is not surprising that its displacement should form one of the most common ailments the female is liable to. But although uterine displacement is of frequent occurrence, it is not of fatal tendency. It demands our attention, however, not only from the distressing local symptoms which attend it, but also from the influence it has on other organs, which in consequence are often so severely affected as to lead to the supposition that they are the seat of organic disease. Prolapsus is met with at all periods of life after puberty, but it occurs most frequently in women who have had large families, and especially in those whose necessities compel them to exert themselves soon after their confinement. It is also a frequent if not an invariable result of rupture of the perineum, in which case it generally assumes the more aggravated form of procidentia, in which the pessaries hitherto in use afford little benefit, and the operation proposed by Dr Baker Brown cannot always be depended on, even were it eligible in every case.

It is not my intention, however, to enter on a lengthened discussion on an ailment so well known as prolapsus, and one which has been described by almost every author who has treated of the diseases of females, as my object at present is merely to bring under the notice of the Society and the profession a new form of uterine support, which I have found of great benefit in prolapsus and more especially in procidentia.



This support is composed of two distinct parts, which are easily separated, although when in use they are firmly united. The one part consists of a steel band an inch broad, covered with leather, which encircles the pelvis, and buckles behind, and having a heart-shaped plate of the same materials in front corresponding to the mons veneris. To this plate the second part, consisting of a globe-shaped wooden pessary an inch in diameter, is appended by means of a narrow piece of German silver, nine inches in length, which is rounded at one end and screwed into the pessary, while the other end is flattened and secured in a socket by a screw-nail. By this arrangement it can be lengthened or shortened according to circumstances, and being pliable, it is easily bent, so as to place the pessary in the superior axis of the pelvis; at the same time it is of sufficient strength to give a firm and uniform support to the womb. When this pessary is once properly adapted to the pelvis it gives neither pain nor uneasiness, and it does not interfere with the due performance of the functions of the pelvic organs, while it has the inestimable advantage of being easily introduced and removed by the patient herself. She should therefore remove it every night, and have it properly cleaned before introducing it in the morning; thus obviating some of the more serious objections which have been brought against the use of pessaries, particularly by Dr Baker Brown.[1] With due attention on the part of the patient, it does not produce "irritation or excoriation, and with these leucorrhœa"; it is quite compatible with cleanliness, and being of the same diameter as the cervix uteri, it does not "stretch or keep up the relaxation of the vagina." On the contrary, while the circular band gives a feeling of comfortable support, and relieves the weary pain and weakness in the back, the pessary removes the internal down-bearing and dragging sensation which are so distressing to the patient. I have particularized the effects produced by the different parts composing this support, because it is necessary to remove the pessary during the monthly period, as it would interfere with those arrangements which females generally make for their own comfort at that time; but in removing it, they are not deprived of all the advantages afforded by this support. That it does not produce the irritation which has been attributed to other pessaries appears from the fact, that I have known a patient to wear it with perfect comfort and impunity until the fifth month of pregnancy,—a circumstance which I think is owing to its being firmly fixed, and not producing friction either on the womb or vagina. Besides the advantages I have enumerated, it has the additional one of allowing the application of other remedies, which are useful in restoring the tone and healthy condition of the vagina, the want of which are the cause of prolapsus, and not, as has been asserted by some authors, the relaxation of the uterine ligaments, whose proper functions are to regulate and guide the ascent of the womb during pregnancy, and to prevent it pressing injuriously on the intestines and the important abdominal bloodvessels. This opinion seems to be supported by the fact that the ligaments are increased in strength during pregnancy. This is more particularly apparent in the right round[1] ligament. Hence we find that the womb is generally drawn or leans to the right side, by which the rectum is less incommoded than it otherwise would be.

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[1] Surgical Diseases of Women, 2d Edition, p. 98.

I was first induced to employ this support some years ago in the case of a lady who had suffered from procidentia for thirteen years, in consequence of the perineum being ruptured. During the first nine years she was able to replace the womb when in the recumbent posture; but for the four years previous to my seeing her, she had been unable to do so. She was in consequence in a very miserable condition, being unable to go up stairs or move about the house without the greatest distress and inconvenience. On examination, I found the womb protruding beyond the vulva, forming a tumour as large as a child's head, and covered with a hard scaly matter like the skin of a fish. I was enabled with some difficulty to replace it, but the slightest exertion on the part of the patient immediately expelled it. I made various attempts to retain it, but in vain. I tried Professor Simpson's ingenious wire pessary, and I was in great hopes that it would be successful in retaining it in its situation, but on calling the following day, I was disappointed to find the womb again prolapsed. It then occurred to me to try this support, and I had the satisfaction to find it was perfectly successful. Immediately on adapting it to the pelvis, the patient expressed a feeling of comfort; and when I called the next day, I was suprised to find that she had been able to go out a-shopping, and had walked a great deal, with perfect ease and comfort, which she has enjoyed ever since.

Having found this support so beneficial in this severe case, I have been induced to employ it in many other cases, and with the same happy results. Although this pessary was originally intended to be worn with the circular band, I have had it also adapted to the truss for crural hernia, when that disease existed along with prolapsus; and it afforded the same beneficial effects as in the cases referred to above. It is easily adapted to the truss, as it only requires the flat end of the German silver to be pierced so as to correspond with the screw-nails which fasten the pad, and the same nails, a little lengthened, secure it along with the pad. In this case a little more care is required in adapting the pessary to the pelvis, but once it is so, the patient can quite easily manage it herself.

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[1] Dubois' Traité Complet de l'Art des Accouchments, p. 182.

Part Second.

REVIEWS.

1. *On Long, Short, and Weak Sight, and their Treatment by the Scientific Use of Spectacles.* By J. Soelberg Wells, Ophthalmic Surgeon to, and Lecturer on Ophthalmic Surgery at the Middlesex Hospital. Second Edition; pp. 214. London: John Churchill and Sons: 1864.

2. *Entoptics, with its Uses in Physiology and Medicine.* By James Jago, M.D. Oxon, etc. London: John Churchill and Sons: 1864.

3. *The Ophthalmoscope: its Varieties and its Use.* Translated from the German of Dr Adolf Zandar, by Robert B. Carter, M.R.C.S., with Notes and Additions by the Translator. Pp. 225. London: Robert Hardwicke: 1865.

1. As it is only very lately that this work has been fully and favourably noticed in these pages, a very few words will suffice in reviewing this second edition. The first thing we observe is an improvement in the outward appearance of the work, it being reduced to a more convenient size; at the same time the number of the pages is more than doubled.

The chapters on the Lesions of Accommodation and Refraction, including myopia, presbyopia, and hypermetropia, are essentially similar to the corresponding ones in the first edition, although here and there altered, improved, and brought down to the level of the very latest continental researches. An entirely new chapter is added, which gives a brief but intelligible description of the curious anomalies of refraction, which depend on a varying condition of refraction in the different principal meridia of the *same* eye, to which the name of "Astigmatism" is given.

It has been proved by Donders that a slight degree of this condition is nearly always present even in perfectly healthy eyes, so that vertical lines can be clearly seen up to a further distance than horizontal, while again the near point for the horizontal lines is nearer than for the vertical. Careful ophthalmometric measurements have shown that the curvature of the cornea is generally greater in the vertical meridian than in the horizontal. This is normal or regular astigmatism depending on the curvature of the cornea, and which does not practically interfere with acuteness of vision; but a very slight increase, even of this normal form, will cause considerable and progressive impairment of vision. Astigmatism (pathological, impairing vision) may be, as Donders has shown, of three kinds:—

1*st*, Simple, in which the refraction of one principal meridian is normal or emmetropic—and of the other, either myopic or hypermetropic.

2*d*, Compound, in which both meridia are abnormal, in the direction either of hypermetropia or myopia; but in both the abnormality is the same in character, differing only in degree.

3*d*, Mixed, a very rare form, in which the one principal meridian is myopic—the other, hypermetropic.

The diagnosis of astigmatism depending a good deal on subjective symptoms, and thus being greatly complicated if the patient is unintelligent, an observation of Schweigger's recorded here is interesting and valuable, *i. e.*, that a direct ophthalmoscopic examination will show that the optic disc is elongated, or at least appears so, in such cases, and that this elongation is in a direction corresponding to the meridian of greatest curvature.

A description is given of the method of choosing suitable lenses for the remedying of the various forms of astigmatism; but for this we must refer the reader to the work itself.

From one who has studied so deeply, and can so well expound the opinions of others, as Mr Wells has shown he can, the profession will expect and doubtless welcome original contributions to ophthalmic literature.

2. "The light that enters our eye causes us to see, under certain conditions, a series of objects that exist in the organ itself. An investigation of these conditions is called Entoptics." In this manner does the author of this very curious and interesting work define his subject. As we read that a sect of religious devotees in Central Asia find their summum bonum, and their highest ideal of philosophic meditation, in the placid contemplation of their own navels, so the enthusiastic student of entoptics must, in like solitary musing, gaze *at* or *into* certainly not *out of* his own eyes. That to a certain extent such an examination is possible is unquestionable, but as to how far it can ever be made available in ophthalmic diagnosis we are not yet in a position fairly to determine.

For precise entoptical observations, Dr Jago tells us we must use fine pencils of light, and these of two kinds—divergent and convergent. The source of light should be *small*, *bright*, and *round*, as one derived through a pin-hole in a card, or by reflecting sunlight into the eye from the head of a pin, or a small convex lens. With a very simple apparatus necessary for obtaining these pencils of light, the observer is prepared, so far as apparatus goes, for entoptical researches.

The author tells us, in the preface, that during his student-life he discovered that his own eyes contained muscæ; he was then ordered to abstain from books, had to take refuge in thinking, and finally took to examining his own muscæ by means of a punctured card. Since then he has continued the subject, inviting his friends and acquaintances to search their eyes in the same way, and thus accumulating evidence. After the publication of two smaller papers in 1845 and 1859, he has at last published a book on the subject, containing a large amount of curious and original research, giving a brief account of the labours of others in the same field, and illustrated by more than fifty diagrams of entoptical appearances.

A chapter is devoted to the description and explanation of the apparitions to which eyelashes, eyelids, and conjunctival fluids give rise. Another to the iris and crystalline lens viewed entoptically. In the latter, the author endeavours by a rough sketch "to convey some idea of the features of my right lens," and though it may not be a correct, we must at least believe that the likeness is not a flattering one;—it contains "specks," "curvilinear refracting bodies,"—which, if not "oil-drops, look like cysts."

The chapter headed, "The structure of the vitreous body determined entoptically," is a very curious one,—the gist of it is in the following "definition" of his views given by the author:—

> "From the walls of the cavity behind the crystalline lens, as far as it is lined by hyaloid membrane, there springs into view a lax network of beaded fibre, which is the frame of an invisible membrane that divides the peripheric portion of the vitreous into a certain number of little chambers, separating them from each other, and from a larger middle one. These compartments are filled with fluids of graduated density, in such order that the densest lies next the capsule of the lens, and the rarest next the retina, so that the vitreous body is, in all probability, a compound optical instrument, whose anterior constituents excel the posterior in refractive power."

The author believes that the "web of beaded fibre" can be seen and demonstrated, has seen it in his own eyes times without number, and gives some most appalling-looking pictures, one resembling nothing so much as a nightmare idea of a large tapeworm which had succeeded in tying itself into knots.

The practical value which the author believes that these researches have is a medical rather than a mere physiological one, for the author holds that many muscæ volitantes, which would otherwise be supposed to be pathological, are really only "the images thus discussed, an accident of the essential structure of all human eyes."

The retina viewed entoptically, and a chapter on "visual sentients," conclude the work.

In the latter an account is given of those curious quasi-luminous areolæ, with central grey or dark spots, which are produced by local pressure on the sclerotic, even through the closed lids. It is this natural phenomenon which has lately been proposed as a new diagnostic of the state of the retina, in cases where, from opacity of the lens, we are debarred from the use of the ophthalmoscope. The proposer, M. Serres d'Uzès, calls it phosphenic retinoscopy. Cases can easily be conceived in which the patient, being observant and intelligent, such simple and painless experiments might give useful information.

Before concluding this brief notice of an otherwise interesting, curious, and excessively original book, one defect, almost a deformity, must be noticed,—that is, the amount of self-assertion, almost jealousy, which characterizes all the author's references to other labourers in the same field. He has reaped, and brought sheaves home; other men have loaded their wains for the same barn; there is no need for one to point at the sheaves in another's load and say, This is mine; that was grown on my field. They are all going the same way.

3. This treatise on the varieties and use of the ophthalmoscope is in many respects very German in its character. Inexhaustible patience and industry, great power of compilation, with a tendency to tediousness and to an undue and unnecessary display of learning, with, at the same time, a love of minute diagnosis, which gives treatment only the second place, is apt to characterize the products of the German mind. The premature and lamented death of the author has deprived Mr Carter his translator of his advice and assistance. Still we are sure that for English readers Mr Carter has done his work admirably well. His own additions, which are numerous, and which are kept distinct from the text by the use of smaller type, are many of them extremely valuable; most of them being quite necessary, in consequence of the advance science has made in the three years which elapsed between the publication of the German work and its English translation.

To whom do we owe the invention of the ophthalmoscope? That is the first topic in the introduction. Dr Zander merely mentions the name of Cumming, whose paper, in 1846, on the luminous appearance of the human eye, gave the first really practical suggestion as to the present method of examining its interior. There seems now to be little doubt that it was Cumming's paper which, falling into the hands of Brücke at Vienna, first directed his attention to the subject, and enabled him, by the aid of the practical sagacity and mechanical genius of Helmholtz, to give the ophthalmoscope to the world.

The first chapter treats of the varieties of the ophthalmoscope, and the principles on which they depend; and of course the catalogue is elaborate, accurate, and detailed. The chief divisions are the following:—

1*st*, Ophthalmoscopes with reflectors formed by parallel surfaces of glass—those of Helmholtz and Follin. Intermediate between these and the next, Dr Carter has inserted Mr J. Z. Laurence's reflecting ophthalmoscope, which, by means of a piece of plate-glass interposed between the eye of the patient and the source of light (on the principle of Professor Pepper's ghost), gives a reflection of the eye which is visible to more than one observer at once.

2d, Homocentric ophthalmoscopes—these are the ones in most frequent use in this country—imply the use of a perforated concave mirror for illumination, and lenses of various forms between the mirror and the patients; either concave, if for the examination of a *virtual* erect image, or convex, as is most common for the *actual* inverted image. In this list are found the instruments of Ruete, Anagnostakis, Ulrich, Stellwag von Carion, Hasner, Liebreich, and many others. Dr Zander gives full descriptions of eighteen of this class.

3d, Heterocentric ophthalmoscopes,—of which thirteen varieties on different principles are already in the market to perplex the purchaser. Among these are the instruments of Coccius, Tehender, and Frobelius.

4th, Binocular ophthalmoscopes.—These in their ideal perfection enable us to see the objects as they really are, vessels as vessels, rounded and tortuous; the papilla will be no longer a deception, but really seen in its normal condition. The credit of their invention is due we are told to Dr Giraud-Teulon.

5th, Orthoscopes, for the examination of the eye under water.

6th, Autophthalmoscopes, for the investigation of one's own eyes. These last are specially to be recommended to those young practitioners who have a profession better than their practice, and the use of them may serve (along with auto-catheterism as lately recommended by a distinguished London surgeon) to relieve the tedium of a barren consulting hour.

In the second section, the examination with the ophthalmoscope is described, its difficulties owned and accounted for. Elaborate tables are given for calculating the focal length of the lenses required, under varying conditions of sight in the eyes of the observer and observed respectively.

Section third describes the ophthalmoscopic appearances of healthy eyes. A quotation from this chapter, describing the entrance of the optic nerve gives a fair idea of the style of the work, its extreme minuteness and accuracy of description, and the frequency and fulness of its quotations from other authorities:—

"The entrance of the optic nerve presents within normal limits considerable diversities of shape and colour. With regard to shape, it is in most cases round, but seldom perfectly circular, often oval,—with the long diameter perpendicular,—angular in almost all cases of convergent squint, never oval with the long diameter horizontal. In many cases, the ophthalmoscope exhibits round the entrance a dark ring or crescent, which depends upon a collection of pigment in the choroid in the immediate neighbourhood of the nerve, and has no pathological signification. At the point where the lamina cribrosa ceases, the nerve is contracted, and the opening in the choroid is narrow, and in a certain measure compresses the trunk. For this reason a sort of double border is often seen around the nerve entrance, and between the limits of this border it is still surrounded by the choroid, in which at this part Donders has sometimes seen a choroidal vessel.

"With regard to the limits of the nerve, the following description is quoted from Liebreich:—'Under the choroidal margin is the line, more or less dark, that indicates the border of the opening in the choroid; under the sclerotic margin is a bright crescent or circle, formed by the curving round of the sclerotic fibres, and appearing between the choroidal margin and the fine greyish line that indicates the narrowest part of the nerve itself, and is therefore called the proper nerve boundary.'

"The colour is either white, yellowish, reddish, or bluish, with grey or greenish admixture. The glistening whiteness described by some observers (tendinous glistening of Coccius) is not found in healthy eyes, but is present, as we shall see hereafter, in many conditions of disease.

"Among the causes of variation of colour, within normal limits, may be mentioned the age of the individual, the manner of distribution of the vessels upon the optic disc, the illumination, and the colour of the surrounding field. In eyes rich in pigment, the illuminative disc is compared by Dismanes to the moon in a fine summer night, and constrasts strongly with the neighbouring parts. It is sometimes surrounded by a margin of pigment-granules, often perceptible as a dark ring. In eyes with less pigment, the disc cannot be so conpicuously illuminated, or so sharply defined, and presents usually a more reddish tint.

"In the bright disc of the papilla there may often be observed an admixture of a delicate greyish or bluish stippling, in elongated dots, lines, or clouds, by which the intensity of the light is so increased, sometimes in the periphery, more frequently in the centre, and especially between the vessels, that the part displaying it appears to shine."—Pp. 98, 99.

An elaborate enough attempt at word painting this is, and is a fair specimen of similar descriptive passages in the book. In concluding this section, Mr Carter gives a few notes on the subject of researches by the ophthalmoscope, and their results,—researches in fact in entoptics,—a single paragraph we would recommend to the serious consideration of Dr Jago:—

"It is evident, I think, that the possessor of an autophthalmoscope will be much tempted to subject his own eyes to prolonged examinations, such as no patient would undergo at the hands of a surgeon. On this account the apparatus is probably a dangerous one. Dr Heymann speaks of secondary images, colour phantoms on the dazzled portions of his retina, which I myself would prefer not to experience; and I hear from Dr Giraud-Teulon, that he has found it necessary to lay aside his instrument. The fundus of one's own eye is beyond all doubt a highly interesting object, but the evidence now in existence tends to show that natural curiosity concerning it may be gratified at too high a cost."

Section fourth is devoted to the ophthalmoscopic appearances of diseased eyes. While such observations are still in their infancy there is a danger that every little variation in colour and elevation be considered a new disease and be described as such; hence, probably at present we have too many varieties of disease, as we *certainly* have too long stories about them. Mr Carter finds it necessary to cut down sixty paragraphs "of extraordinary prolixity and tediousness," which Dr Zander has quoted from Professor E. Jäger, into three very readable pages, and in so doing he has exercised a wise discretion. We have no doubt, that added experience will enable us very materially to shorten many descriptions and amalgamate many supposed varieties of eye diseases.

Section fifth (and last in the original) is a very brief notice of the ophthalmoscope in its relations to forensic medicine, showing its value in cases of malingering and simulated blindness, and its usefulness as a means of determining the date of death by the state of turbidity of the media, as at one time was anticipated might be the case.

Mr Carter has added a sixth section, "in deference to the practical character of the English medical mind," on the influence which the ophthalmoscope has had upon treatment. It is a brief notice of ordinary treatment of eye cases, and a special pleading, most unnecessarily enthusiastic, in favour of iridectomy. The operation in cases which require it does not now need external aid. What its supporters have to fear is, its too indiscriminate application in cases which do not require it or are utterly hopeless.

The work of the translator has been well done; for the sense has been given in readable English, without the slavish adhesion to the German idiom too often seen in translations of professional works, which, while it renders their perusal a task, does not in the least imply any greater accuracy in the conveyance of the author's meaning.

Six lithographs are prefixed, furnishing "ideal representations of some of the more important appearances of the fundus oculi;" and in the body of the work are numerous woodcuts illustrating the different forms of the ophthalmoscope, and diagrams of the optical principles involved in each form.

The publisher's part is admirably done, the type being clear and pleasant, and the lithographs extremely well executed.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLIV.—MEETING VI.

5th April 1865.—Professor DOUGLAS MACLAGAN, *President* of the Society, in the Chair.

I. MALIGNANT TUMOUR OF THE UPPER JAW.

Professor Spence showed a tumour of the upper jaw, involving the whole of the right superior maxilla, and in the palatine aspect extending somewhat towards the left. The tumour had existed only three months, its growth having been very rapid. Unfortunately an incision had been made into it by a surgeon. When Mr Spence saw the patient the tumour was of large size, was growing very rapidly, and the skin of nearly the whole of the right cheek was adherent to it. Still, as the growth did not extend back towards the spine, as the base of the cranium was not involved, and as the eye was not displaced, Mr Spence had thought it right to operate, so as to give the woman a chance of at least prolonging her life. Accordingly, the operation was performed, and the whole of the tumour removed, together with a further section of the palate, which seemed somewhat affected. As the integuments of the right cheek and side of the nose were involved, a large gap was left, which Mr Spence closed by a flap of skin dissected from over the lower jaw and upper part of the neck. The case had gone on favourably, the appearance of the patient was considerably improved, and she might be looked upon as safe at least for a time. Sketches of the patient before and after the operation were exhibited.

II. EPITHELIAL CANCER OF THE HAND.

Professor Spence showed a forearm which he had removed to-day on account of cancroid or epithelial cancer of the hand. The patient was a man about sixty-four years of age. Mr Spence had seen him three or four years ago, and at that time the disease was in much the same state as at present. The patient was a mason, and ascribed the disease to working much with lime. When seen on the first occasion, a single enlarged gland could be felt in the axilla. Mr Spence saw him from time to time, but lost sight of him until the present occasion, when the patient returned for the removal of the disease. There was still an enlarged gland in the axilla; but as the man's health was tolerably good, and as the disease was extending, Mr Spence considered the enlargement of the gland might be the result of irritation, and the disease itself of a local character. He accordingly had thought it right to give the man a chance, and had to-day removed the forearm. Mr Spence had shown to this Society some years ago a specimen of a similar disease, but which at the time looked still worse than the present case. The bones of the leg were involved, and there was an enlarged gland in the groin. Still Mr Spence had amputated the thigh; the patient did well, the enlarged gland disappeared, and the man was now quite well.

III. CYSTIC TUMOUR OF THE MAMMA.

Professor Spence showed a simple cystic tumour of the mamma which he had removed to-day. It was large, prominent, and was easily enough removed. The greater part of the breast was compressed by it. The cyst contained a dark-coloured fluid.

IV. HÆMATOCELE OF THE PATELLAR BURSA.

Professor Spence showed a specimen which he had removed by operation yesterday, and which illustrated a rather rare form of disease—hæmatocele of the bursa patellæ. The growth constituted a prominent tumour, dark coloured externally; and as it felt pretty solid, Mr Spence had removed it, instead of laying it open and touching it with iodine as he generally did. The preparation exactly resembled a hæmatocele of the tunica vaginalis.

V. INTESTINAL CONCRETION CAUSING FATAL OBSTRUCTION.

The President, on the part of Dr T. J. Maclagan of the Dundee Infirmary, exhibited an intestinal concretion, and read the following notes of the case:—

"S. M'K., æt. 20, a millworker, admitted 5th March 1865, under the care of Dr Pirie, stated, that though never of robust constitution, he always enjoyed pretty good health till five years ago, when he became troubled with diarrhœa, accompanied by griping pains in abdomen, which have continued, with occasional intermissions, up to present time. Three months ago, he began to suffer from sickness and vomiting, which have continued to increase in urgency and severity up to date of admission; for last five days had retained nothing on stomach; vomited matters said to consist of what is taken into stomach very little altered. Patient had a haggard expression, a look of much suffering, and was greatly emaciated. Abdomen was slightly tumid, and so tender on pressure as to exclude the possibility of deriving any information from palpation. Pulse was small, feeble, and rapid; tongue moist, and thickly coated with a white fur. Hot fomentations were applied to abdomen. Diet was limited to milk and lime water, and small quantities of brandy in cold water frequently repeated; the stomach, however, retained nothing. A warm water enema came away very slightly tinged. The patient was much annoyed with constant hiccup, and during last two days with painful tenesmus and constant calls to go to stool, nothing, however, being passed. Urine was scanty, of a dark amber colour, sp. gr. 1030; contained no albumen, but was very deficient in chlorides. No remedy afforded the slightest relief to the symptoms, and the patient died on the eighth day after admission.

"On opening the abdominal cavity forty hours after death, the stomach and intestines were found much distended with gas. A small quantity of fluid existed in peritoneal cavity. The mucous coat of the stomach was slightly congested, especially near cardiac orifice. The small intestine was of a dark red hue, much congested, dilated, and of a considerably increased circumference. On laying it open, it was found to contain gas and a greenish chyme, the mucous membrane being much congested, and the valvulæ conniventes unusually prominent. The large intestine was distended with air, devoid of other contents, and healthy in appearance. At the seat of the ileo-cœcal valve was felt a hard mass about the size of a hen's egg. On removing a portion of the small and large intestines immediately above and below this, it was found that the obstruction to the canal was complete; and on laying open the gut, the cause of the obstruction was found to be an intestinal concretion, formerly located in the caput cœcum coli. The coats of that part of the gut which embraced the concretion were much thickened. The concretion itself was of a brownish grey colour, having very much the appearance of felt, of an irregular form, with several indentations on its surface, and having numerous fine hairs scattered over it; weight, ʒvi. On section, it was found to consist of concentric layers arranged round a hard nucleus, the inner part having layers darker in colour and less distinct than the outer. The microscope showed the nucleus to consist of bone, and the felty outer part to consist of a confused network of minute hairs, having the interstices filled up by brownish amorphous matter."

The President remarked, that this concretion was evidently mainly composed of the husk of the oat, and exactly resembled specimens which had been laid before the Society by Mr Turner of Keith. Such specimens had become much rarer since the introduction of better winnowing machines, by which the husk was more completely removed from the grain.

VI. CONGENITAL DEFICIENCIES IN THE CRANIUM.

The President, on the part of Dr T. J. Maclagan, exhibited a skull-cap, and read the following notes of the case:—

"A well-developed, florid-complexioned female, with rather intelligent expression, came to hospital on 18th October, for the purpose of having a growth on scalp removed. The tumour was situated over the occipital protuberance, devoid of hair, and about the size of a hen's egg; evidently cystic in character; has existed since childhood, and is inconvenient from its increasing bulk. Excised in the ordinary way; hæmorrhage controlled by pressure—no ligature used; went on very well for two days. On the third day symptoms of inflammation of membranes appeared, and gradually increased in severity, till the patient died comatose on the fifth day after the operation.

"*Sectio.*—On putting finger into wound, it passed into the bone by an aperture, the bottom of which could not be reached. On removing the calvarium, there were found to be three apertures in the skull, occupying the positions seen in the preparation, and directly over the lowest of which (that near the occiput, on the detached portion of skull) the tumour was situated. Besides the dura mater, each of the two upper apertures were bridged over by a cribriform plate of fibrous tissue. The remains of a similar structure existed at the edges of the lower opening. The veins of the cerebrum were loaded, and at the base of the organ was found a quantity of fluid, which existed in unusual quantity in the ventricles as well. The base of the brain, from the optic commissure to the posterior extremity of the cerebellum, was covered with a layer of lymph and pus, quite obscuring the structures beneath."

VII. ON THE STATISTICS OF PUERPERAL INSANITY.

Dr Batty Tuke read a paper on this subject, which will be found at page 1013 of this number of the Journal.

Dr Skae entirely concurred in the conclusions come to by Dr Tuke. The views proposed by the author of the paper were not new, but Dr Skae considered that Dr Tuke had done good service by dividing and classifying the cases included under the general term of puerperal insanity.

VIII. CASE OF AN UNUSUAL FORM OF NERVOUS DISEASE.

Dr Sanders read a paper with this title, which will be found at page 987 of this number of the Journal.

At the conclusion of his paper, Dr Sanders introduced the patient for inspection by the members of the Society.

Dr Skae remarked, that the term paralysis is often applied to cases where there is no real palsy, and instanced the general paralysis of the insane, in which there is a defective co-ordination without true muscular palsy. The erroneous nature of the term general paralysis of the insane has long been felt and various other names offered, and it is now frequently called general paresis. Several papers have lately appeared on it under this name in the Annales Medico-Psychologiques, and the Asylum Journal. The same disease, without insanity, has been called " progressive general paralysis," and is now described by Duchenne and Topinard under the name " Ataxie locomotrice progressive."

Dr Sanders was of opinion that the inaccuracy and vagueness of nomenclature to which he had alluded in his paper, in regard to the various forms of paralysis agitans, and to which Dr Skae now referred in connexion with the general paralysis of the insane, was a very serious evil, and ought as soon as possible to be remedied. To attain correctness, however, it appeared to him necessary to keep in view, that the term paralysis, which in its original limited sense meant loss of voluntary motion, is now commonly used in a wider sense, and possibly, in consequence of recent accessions to our knowledge, its meaning may require still further extension. Hitherto two kinds of paralysis have been admitted: 1*st*, paralysis of motion (muscular motion, vasomotion, etc.); and, 2*d*, paralysis of sensation (of common sensation, and of the special sense of touch, temperature, etc.). To these it might now be necessary to add a third kind, viz., paralysis of co-ordination, or ataxia, which affects the nerve centres, and not, as in the preceding kinds, the nerve trunks or their roots. Any one of these kinds of paralysis may exist independently of the others; in clinical investigations they must be sought for separately, and the presence or absence of each specified, before we can know precisely whether paralysis can be said to be present or not. Some persons would deny the term paralysis to the paralysis of sensation; and many more would refuse this term to the ataxiæ; hence we have in the latter the vague terms of " apparent paralysis without loss of muscular power," " paresis," etc.,—all implying the difficulty of stating the true state of the case, which is, that there is some kind of defect in the motor apparatus, but not absence of voluntary motor power. If we recognised a paralysis of co-ordination as the essential condition in ataxia, all this difficulty and confusion would be avoided. Dr Skae considered the progressive locomotor ataxy (" Duchenne's disease," as Trousseau calls it) to be the same disease as the general paralysis of the insane, only with the absence of the insanity. Dr Sanders could not agree with that opinion; several diagnostic marks, and especially the affection of the lips and tongue, had been pointed out distinguishing the two diseases. The resemblance which those diseases presented in certain features rendered their distinction from each other one of the most important points of diagnosis which can be conceived. Dr Sanders knew of at least one case of progressive locomotor ataxia, which had been set down by high authorities as general paralysis of the insane, many years ago, giving rise to the most serious and gloomy anticipations on the part of the patient's friends, —anticipations which fortunately have not been realized.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXIV.—MEETING V.

8*th February* 1865.—Dr PATTISON, Vice-President, in the Chair.

I. ON MEDICATED PESSARIES AND SUPPOSITORIES.

Professor Simpson wished to bring under the notice of the Society some changes that had been made in the form and composition of medicated pessaries, and to throw out some suggestions as to the more extended employment in practice of various kinds of suppositories. He had been in the habit for many years past of applying different kinds of medicated pessaries to the cervix uteri in the treatment of uterine disorders; having been led to their use by finding that bromide and iodide of potassium, and many other substances, could thus be administered locally, which were of no avail in the form of a lotion. As a medium for the administration of drugs the ointment-pessary was not new, for Daremberg, in his late translation and commentary of Oribasius, has pointed out that a form of it was made use of long ago in Rome. Pessaries had not yet found an acknowledged place in the Pharmacopœia; but that did not interfere with their usefulness. Up to a recent date, he (Professor Simpson) had been in the habit of using pessaries composed of simple ointment, with the special drug rubbed up in it, made into a globular shape, and stiffened on the surface by being dipped in some warm melted ointment in which the proportion of wax largely predominated. But, latterly, Messrs Duncan, Flockhart, & Co., of this city, had made them for him of cocceine, which presented this great advantage over the old ointment, that it was much more consistent and firm when cold, whilst it melted at a lower temperature, and with greater rapidity. In consequence of this greater solidity of the cocceine it was found that pessaries made of that substance were more easily employed by the patients themselves than the softer and more yielding ointment balls; and the facility of introduction was still further promoted by having them made not spherical, but conical, —almost precisely of the shape of a Minié bullet.

Professor Simpson had brought with him specimens of some of these pessaries; but their various constitution and uses would be most easily seen by reference to the following list of them, which had been recently published by the chemical firm to which he had alluded:—

Atropine (Belladonna)	Sedative		1-20	grain.
Opium	do.		2	grains.
Morphia	do.		½	grain.
Bismuth, Oxide	Cicatrizing and Emollient		15	grains.
Borax	do.	do.	15	do.
Zinc, Oxide	do.	do.	15	do.
Tannin	Astringent		10	do.
Alum	do.		15	do.
Alum and Catechu	do.		15	do. of each.
Do. Iron	do.		10	do.
Acetate of Lead	do.		7½	do.
Do. do. and Opium	do.		5	do. 2 gr. Opium.
Matico	do.		10	do.
Sulphate of Iron (dried)	do.		10	do.
Gallic Acid	do.		10	do.
Perchloride of Iron	Hæmostatic		5	do.
Persulphate of Iron	do.		5	do.
Sulphate of Zinc (dried)	Caustic		10	do.
Carbonate of Soda	Antacid		15	do.
Carbolate of Lime	Deodorant		5	do.
Iodide of Lead	Alterative and Resolvent		5	do.
Do. do. and Atropine	do.	do.	5	do. 1-20 Atropine.
Do. Potassium	do.	do.	10	do.
Bromide of Potassium	do.	do.	10	do.
Mercurial	do.	do.	30	do. (*Ung. Hydrarg*).

Many of the pessaries are made with morphia (½, 1, or 2 grains) in them, to suit cases where a sedative is also required. They are also sometimes made hollow in the centre, to allow of a few drops of chloroform liniment being introduced along with them. Various other kinds of pessaries are made.

Besides being useful in the manufacture of pessaries, cocceine would be found invaluable as a medium for the administration of medicinal substances by the rectum. The ordinary suppository mass was apt to be either so soft as to be difficult of introduction through the anus, or so firm in its consistence as to be very slow of solution; and the result in some cases was, that either the medicine was imperfectly applied, or the suppository was so long of dissolving, that the drug had not the opportunity of exerting its specific action at a sufficiently early period. These drawbacks to the use of suppositories were quite done away with when they were made of cocceine, which is at once firm and fusible; and he (Prof. S.) believed that the advantages attached on its use would now probably lead to the more frequent application of medicines through this channel. We had long been in the habit of administering opiates in this way, especially the muriate of morphia in doses varying with each particular case. For many years the suppositories he had most frequently employed contained half a grain of morphia in each; but latterly, for ordinary uses, he had had them made with only a third of a grain, because in a few exceptional cases the half grain exhibited by the rectum seemed to act too powerfully. Dupuytren had pointed out that sometimes opium seemed to affect patients more powerfully when administered per anum, than in the ordinary way; and this case was one of those where the individual appeared to be unusually susceptible of the influence of the dose. But, besides morphia and the opiates, there was a great variety of other remedies that might be usefully applied in this way, as would be seen from this list of suppositories :—

Aloine (with Soap)	1 grain, 5 grains Dry Soap.
Belladonna	½, 1 and 2 grains (*Ext. Bellad.*).
Bismuth, Oxide	10 grains.
Borax	5 do.
Copper, Acetate of	2 do.
Elaterium	½ grain.
Ergot of Rye	2 drops Oil.
Gall and Opium	5 grains and 1 grain.
Gamboge	5 do.
Henbane	5 do. (*Ext. Hyoscy.*).
Iron, Alum	3 do.
Do. Persulphate	2 do.
Mercurial	6 do. (*Ung. Hydrarg.*).
Morphia	¼, ⅓, ½, 1, 1½, and 2 grains.
Opium	2 grains.
Podophyllin	1 grain.
Santonine	5 grains.
Soda, Hyposulphite of	5 do.
Tannin	5 do.
Zinc, Oxide	10 do.
Do. Sulphate (dried)	3 do.

Thus, there were, first, a number of cathartics which might be introduced into the rectum, and thus often afford relief to patients, without their being subjected to the disagreeable ordeal of swallowing some nauseous medicine. We had all known that nurses were occasionally in the habit of introducing a bit of soap into the bowels of children, with the view of inducing a motion, and the result was thus probably due to the physical irritation that ensued. But the question was, whether we could not introduce some substance into the rectum in the form of a suppository, which would exert such an action on the mucous or muscular coats of the bowel as to lead to a full evacuation. He (Prof. S.) had employed the gamboge suppository in some instances, with a satisfactory result. When introduced into a rectum which was distended with fæcal matter, it almost always acted comfortably within an hour; but if introduced into an empty bowel it gave rise to severe griping. He had under his care a lady from Liverpool, who told him the other day, with an air of much surprise, that her bowels had acted the night before. She had been habitually constipated, and for many years had had no relief except from the use of an enema, which she used to employ in the morning; and the evening evacuation which had so astonished her resulted from the action of a gamboge suppository which had been introduced. He was not, of course, prepared to say what the precise doses of the different drugs, when thus employed, ought to be; certainly the half-grain of elaterium noted in the list was too large a dose, and in one case had produced some dysenteric symptoms. He sometimes saw, along with Dr Maclagan, a young lady in whom half-a-grain of podophyllin in a suppository acted very easily and well; but, in some other patients, the use of a podophyllin suppository had been followed by a very prolonged diarrhœa.

Again, mercury could be administered in the suppository form in cases where it was deemed necessary to salivate, for its specific action was sometimes very rapidly developed when it was thus employed. In this form, moreover, it was one of the most effectual means at our command for the destruction of ascarides; an object which in other instances might probably be attended by the use of suppositories containing santonin or hyposulphite of soda as their active ingredients.

Besides the drugs of the aperient class, we might employ others of a more sedative or tonic character. Thus, the acetate-of-copper suppository had been found useful in case of bleeding piles, and he had seen a patient who was suffering from fissure of the anus, and who rebelled against the use of morphia, greatly relieved by employing suppositories containing the dried sulphate of zinc. Ergot might be administered in this manner in appropriate cases; and quinine and iron might be similarly administered,—especially with patients whose stomach resented the use of chalybeates. Astringents, too, of every variety, could be employed with the greatest benefit; and, indeed, one could hardly predicate how many kinds of medicines we may yet learn usefully to administer in the form of a simple suppository.

Dr Gordon remarked that it was generally considered that medicine was not so active when administered by the rectum. It was a point for further investigation, as, from Dr Simpson's account, it acted in some instances with great effect. He thought the dose contained in some of the suppositories too strong.

Dr Stephenson stated that Professor Christison did not consider opium so active when given by the rectum. He stated in his lectures that some French authors considered it quite as active; but if this were the case, many persons ought to have been poisoned, for, in Scotland, the dose was generally doubled when given by the bowel; but we never saw any poisonous effects when so administered.

Dr Wilson said he often ordered two teaspoonfuls of laudanum in a starch injection.

II. CASES OF ABNORMAL LABOUR.

Dr Andrew Inglis read the following notes of cases:—

CASE I.—I saw this patient after about eighteen hours of pretty severe labour, the pupil in attendance having got anxious. The pains were strong, but irregular. The abdomen was about the size generally met with at the seventh or eighth month, and very hard, as the uterus remained slightly in action between the pains. The vagina was moist, but without any mucilaginous discharge, and the cervix was flattened out as at the full time. The os was high, and only opened to about the size of a shilling, and the membranes were entire, but not forming any bag. The head presented; but at this stage no suture could be found. About twelve hours later I saw her again, and was told that during the interval the pains had become a little stronger and more frequent. On examination, the os was found a little more open, but otherwise there was no change. The presentation was now plain. The head was at the brim, and directly in the *transverse* diameter, with the face to the left. All attempts to adjust it failed. We therefore directed our attention to complete the dilatation of the os, and after five or six hours it was open. The head then came down into the pelvis, still keeping the transverse position, in spite of all efforts to push it round, and the left parietal bone, which was next the pubes, came more down than the right. The pains then increased in strength, but became less frequent, and the head, which seemed small, got moulded to the shape of the pelvis. The membranes then ruptured, and a large caput succedaneum began to form on the left side of the head, and at last protruded from the labia. As the state of the patient began to grow serious, I should have liked to use the forceps at this stage, but the position of the head made it seem doubtful whether they could be used with safety to mother and child. I therefore waited, with the determination not to interfere unless a complete stoppage should take place. After she had been about forty-five hours in labour the face turned suddenly round into the hollow of the sacrum, and the occiput came down under the arches of the pubes. Two or three pains in close succession then came on, and terminated the labour. Before the placenta was thrown off, bleeding came on. I therefore put in my hand to take it away, and in doing so discovered the cause of the course the head had taken. On the sacro-iliac synchondrosis of the left side was a long flat node of bone, covering the articulation. It projected inwards, so as to prevent the head taking the oblique diameter at the brim; and even a little way into the pelvis it must have caused considerable obstruction. The child was alive. It was very small, and the head was very much distorted. The vertex was pushed up, and the side which had been next the tumour was pushed in, while the left parietal region, which had protruded so long, was very much swelled.

Her previous history throws no light on the subject of the tumour. She is 24 years of age; was a domestic servant; and has now been four years married. She has always been subject to rheumatism, and has more than once had to give up her place for a time owing to it. Her hands have suffered so much that she cannot wash; but though she has had pains in the lumbar region, she does not think that they have been worse there or more fixed than in any other part of the body. She had one child, two years ago; was nearly as long ill, but the child was smaller, and did not thrive. Her recovery was slow; the abdomen was tender for some time; and she was very much exhausted.

Case II.—On the 1st of January last, I was called to a case of labour. The patient was 19 years old, and it was her first confinement. The pains had been coming on once every five minutes. The abdomen was small, as if she had only been in the sixth month; but the cervix was as thin as it could possibly be, and quite fully developed. The vagina was moist; a little thick mucus was coming away now and then; and the os was slightly opened. The fœtal heart was loud and strong, and the placental sound normal. As labour seemed inevitable, and the pelvis was deformed, I separated the membranes from the cervical portion of the uterus, as far as I could reach with the finger, and gave her a full dose of opium. This took place about 7 o'clock in the evening, and she had no more pains till 10 the next morning, when they returned suddenly and strongly. About 11, she complained of severe pain in front, which seemed to proceed from the bladder, and was relieved at once by the catheter; and about 12, when the os was fully dilated, it began to return, but was relieved in the same manner. About half-past 1, or nearly 2, the head was down in the pelvis, but she seemed quite exhausted; and the pains ceasing to move the head any farther, about 3, I applied the forceps. A good deal of force was required to extract the child, which was very small. He was less than 3 lb. when weighed with his clothes on. There was no reason to suppose, from the appearance of the skin or nails, etc., that the child was premature; and I was told that the mother herself was no bigger at birth. It took the breast greedily at once, and is now thriving well. The mother menstruated last in the end of March, and was often sick in the morning during April and May; but, from her size, did not expect to be confined for some time. She recovered well; the catheter was required only once; and on the ninth day she was going about perfectly well.

Although the labour had not been much more than five hours in length, it seemed to me right to use the forceps at once; for I had no doubt that the pains had ceased, because they had met with effectual resistance (as was proved by the force required to extract), and, though they might have returned with renewed vigour some hours after, still they would not be likely to be so strong as formerly. Besides, she was suffering very much from the pressure of the head in the pelvis.

I examined the pelvis carefully afterwards, and found the ischia much too near, and a bulging forwards of the upper edge of the pubic bones, while the lower edge was bent inwards. This would account for the urinary symptoms. Should the patient become pregnant again, I think it would be advisable to induce premature labour, whenever her abdomen attains the same size that it did in this last pregnancy.

Case III.—About the end of August last a patient called and asked me to attend her at her confinement, which she said would be in the second week of September. She told me that it was her fourth pregnancy, and she had a strong healthy child about three years old with her, which she said was her last. I asked if she had got on well at her previous confinements, and she at once, without hesitation, said that she "always mended well." I saw her about the middle of September, when she said she was very uncomfortable, and was sure that she was past her time, and was bigger than she had ever been.

In spite of all this, she was not taken ill till the 16th of October, a full month at least after the time she had counted on. The pains came on suddenly about 1 P.M., and the messenger said that the child was almost born. When I arrived I found the membranes protruding outside the vulva, forming a bag nearly as large as a child's head. The pains were strong, and a continuous straining, like tenesmus lasted during the intervals. The bag was so tense that I was unable to make out the presentation. I gave her chloroform, and at once ruptured the membranes, expecting a transverse presentation, but found the head at the brim. The presentation was quite normal, but the pelvis was badly deformed. The sacrum projected inwards, and could be easily reached; the outlet was not much contracted, but pointed obliquely to the right side; the coccyx was very short, and a long way to the right of the mesial line.

The os was fully dilated, but a small portion of it, like a tongue, was jammed against the pubes. Meconium was coming away with the discharges, and no fœtal sound could be distinguished. Turning was out of the question. I therefore applied the forceps, and began to extract. The hold obtained was good; the tip of one blade rested on the mastoid process of one side, while the other was in front of the opposite ear. As soon as the head began to descend the prolapsed piece of the os went up. After pulling as hard as possible, from a quarter-past 2 till a quarter-past 4, the head protruded a little from the labia, but the bones of the head were giving way, so that the forceps would not hold any longer. I therefore placed the patient on her back, and with the forceps twisted the occiput round to the right, so as to bring the forehead opposite the left sacro-sciatic notch, which was much larger than normal, on account of the distortion of the coccyx. The forceps were then taken off, and my right hand was passed over the forehead, to the orbits, while the left was placed at the occiput. By this means I managed with some difficulty to bring out the head so far that the natural pains terminated the labour. The uterus contracted firmly and continuously during the whole of this process until the placenta was thrown off, and the tenesmus I mentioned before continued for nearly an hour after delivery. The soft parts swelled up a good deal before I left the house, but in the evening had resumed their normal condition. There was no injury of any sort, and when I called the next morning she had made water freely three times, without any pain. I saw her again, up and well, that day week, and she has continued well since.

The child was large, and weighed over 12 lb. It never breathed or moved, but the heart was beating slightly for about five or ten minutes after it was born. The mastoid process, and part of the occipital bone were considerably indented by the forceps.

The patient's account of herself afterwards quite overturned her previous statement. She was delivered by craniotomy of a boy the first time she was confined; was eight weeks in bed after it, but does not think that her illness had any connexion with the delivery. Her abdomen was leeched at that time, and she had jaundice. Her second child was a boy also; but though the head was broken up, it may have been with forceps. She was seven weeks in bed after this confinement; but is as certain that the illness was unconnected with the confinement, and can give no account of it. Her third child was a girl, which arrived before the time, and the labour lasted four days. On the third day there was a consultation about her, and the child was born on the fourth day, after it was determined to use instruments. This girl is the only live child she has had. She has now gone home to the country, quite offended at the idea that she cannot have a live child at the full time like other women.

I have brought forward this case to-night, partly on account of the difficulty, and partly to show how well a patient may recover after a very severe confinement, *if the labour is short.* She was between three and four hours in labour; and during rather more than half of the time the forceps were being used energetically. I also wish to point out the care we should take in inquiring as to previous history; for, though her statement was evidently not true, I think she firmly believes it, and is convinced that her bad deliveries and long illnesses were merely accidents of the past, and not permanent and unavoidable evils.

CASE IV.—This patient began labour at 9 in the evening of January 9. The pains were strong and good, and at 1 the os was dilated and the membranes (which had come down well), ruptured. The frontal bone was presenting opposite the right groin. The pupil in attendance then sent for Dr F. White, who noted the particulars of the case. I saw her at 6: the face had gradually come down and was opposite the right obturator, but though the pains were strong there was no progress. The pelvis was small and the head was quite above the brim. I put on the forceps, but could make no impression on the head. Turning was then attempted. As this was equally unsuccessful, I then sent for the kephalotribe. Dr A. Simpson kindly came up with it, but at my request tried again to turn. The leg was got down, but this did not help in the least. Neither external nor internal manipulation made any difference, and at last we were obliged to break up the head. This seemed rather a formidable undertaking, as the face was presenting, and both arms and a leg were blocking up the vagina. At 8.50 the head was perforated near the eye, and at 9 the kephalotribe was applied. At first, when traction was made, the head did not come down, but, after readjusting the blades and screw, the head was extracted at 9.15; the whole process lasting exactly twenty minutes. The placenta was then squeezed out of the uterus ten minutes after. She has recovered without any bad symptom; and I have no doubt the good recovery is due to the fact that she was only twelve hours in labour, and also that the head passed through the pelvis in about five or six minutes at the longest.

I examined the pelvis last week, and found the ends of the pubic bones projecting forwards, so as to form a very acute angle close to the symphysis, the circumference and diameters of the brim being thereby greatly diminished.

This is the second time I have had occasion to use the kephalotribe. The first time the instrument was imperfect; and though it might have been of great use in some cases, its shape was unsuited to the individual instance, and it did not give any material assistance; but the present form of blades seems quite equal to almost any emergency of the sort, and certainly is far before the old craniotomy instruments.

The patient has had eight good confinements, but her ninth and tenth were both fatal to the child, and she was told that she would never survive another delivery, as the bones were falling in so rapidly.

Dr Gordon considered that craniotomy would have been better in the third case than submitting the patient to so long an ordeal with the forceps.

Dr Alex. R. Simpson thought that Dr Gordon's suggestion had been in a measure acted upon, as the head had been partly broken up with the forceps in that case. There was one point of interest in the method of conducting the fourth case. From continental obstetricians we had learnt the use of the kephalotribe; and the last case related by Dr Inglis demonstrated the advantage to be obtained from following their practice in regard to the position of the patient. For after Dr Inglis had exerted a due amount of tractile force with the instrument, whilst the patient was lying, as usual, on the left side, without succeeding in effecting any very decided advance of the head, he asked him to "take a pull;" and he (Dr S.), by having the patient turned round upon her back, brought down the head very easily, and without putting forth anything like the same degree of extracting power. It would often be found thus that the supine position was much more suitable for obstetric operations than the ordinary position on the left side. And now that the patient was asleep with chloroform, considerations that would at one time have weighed with us in making choice between the two, might now be left out of the question.

III. SPECIMEN OF AN OVUM.

Dr Alex. R. Simpson showed a specimen of an ovum with the remains of the umbilical vesicle.

SESSION XXIV.—MEETING VI.

February 22, 1865.—Dr Moir in the Chair.

I. ON A NEW FORM OF UTERINE SUPPORT.

Dr Charles Bell read a paper on the above subject, which will be found at p. 1029 of this number of the Journal.

Dr Moir remarked that something firm and fixed like Dr Bell's instrument must be very effectual, especially in long-standing cases. He had no doubt it would be found extremely useful.

Dr Keiller said that this looked a very likely instrument. The principal point with such is to fix it on the pelvis. That was the great advantage of the form proposed by Professor Simpson, but Dr Bell's was superior to it in that respect. The disadvantage of all such means for giving support to the uterus was the pressure on the front of the pelvis, but this instrument was well adapted for comfort in this respect. The advantage of Mr Baker Brown's operation for deficiencies of the perineum was, that you obtained the necessary support required for the use of the usual forms of pessaries. Some object to the use of such pessaries, saying they cause pressure, uneasiness, etc.; his usual answer was, that the same objection might be brought forward to false teeth, where large artificial gums if properly fitted were found to cause no uneasiness. He would not use Dr Bell's support in a recent case, but would prefer some form of gutta percha pessary, for which you could generally get sufficient support although the perineum was ruptured. A very great advantage which Dr Bell's instrument possessed was the readiness and facility of its introduction.

Dr Alex. R. Simpson remarked that the multiplicity of instruments which had been proposed for the support of the uterus showed that one form could not be adapted to all cases. He was much pleased to see this one of Dr Bell's, for there were many cases where you required to give more support than you could gain through the muscularity of the vaginal wall. It appeared to be a very good form.

Dr James Sidey said that he had used this form of support in eight or nine cases, and had found it very useful and a great comfort to the patients.

II. AN IMPROVED FORM OF ABDOMINAL BANDAGE.

Dr James Sidey, in showing this bandage, said the only object which he had in view in bringing it before the Society was, to make known its great cheapness as compared with others supplied by bandage-makers. It was made by Mr Mackinlay, of Bank Street, and cost only 13s. 6d., and was the same in form as those charged 30s. elsewhere.

III. OVARIAN DROPSY, WITH PREGNANCY.

Dr Keiller communicated the particulars of a case of ovarian dropsy, complicated by the occurrence of pregnancy. The patient, a lady about twenty-five years of age, had been the subject of ovarian dropsy for the last six years. The abdominal enlargement very slowly progressed, and ultimately presented the usual characters of a greatly distended unilocular cyst.

In consequence of the degree of discomfort, and the great and increasing bulk, Dr Keiller tapped the patient about three years ago, and drew off twenty-five pints of unusually clear and watery fluid; the swelling entirely disappeared during the operation, so much so that the presence of the cyst could not be detected. For many months after this first tapping, the patient felt so well and free from swelling that a cure was considered to have been accomplished, and about twenty months ago she married, although at that time a renewal of the enlargement was detected on examination. The progress slowly and steadily increased after marriage, fluctuation became more and more distinct, until the abdomen almost reached its former size. Menstruation had continued to occur regularly as before marriage until July last, when it for the first time ceased. Symptoms of unusual discomfort followed, and by-and-by considerable bearing-down feelings and other signs of local pressure and general irritation were experienced. Dr K., on examination, detected the existence of pregnancy, which accounted for the disappearance of the menses and the other symptoms present. The mammary signs were observed and the state of the cervix uteri, and the vaginal application of the stethoscope left no doubt on the subject. In this case the stethoscope was of no use when applied to the abdomen, from the presence of the ovarian fluid; but as applied by Dr Keiller *directly to the uterus*, *along the vagina*, the uterine blowing murmur of pregnancy became at once apparent. The symptoms of local discomfort and downward pressure becoming daily worse In order to allow room for the uterus to ascend out of the pelvis, into which it was confined, Dr Keiller, when pregnancy was advanced to about the third month, thought it prudent to again tap the cyst, with the view of preventing the occurrence of abortion, which seemed to be threatened. The operation was done on the 24th January, when twenty pints of fluid, somewhat more albuminous than on the former occasion, were evacuated. The patient recovered soon, and experienced much relief, the bearing down and other symptoms referred to almost disappearing.

Dr K. stated that he had visited his patient to-day, that she was now about seven months gone, and that he would watch the progress of the interesting case, and not fail to report its result at some future meeting of the Society.

Dr Moir remarked that this was a case where premature labour might be induced should the patient suffer from the presence of the tumour.

Dr Alexander Simpson exhibited a diseased placenta. The history of the case will be communicated at a future meeting.

GENERAL COUNCIL OF MEDICAL EDUCATION AND REGISTRATION.

MINUTES OF MEETING, *Tuesday*, *April* 4, 1865.

ROYAL COLLEGE OF PHYSICIANS, LONDON.

Present,—Dr Burrows, *President*, in the Chair; Dr Alderson, Mr Arnott, Mr Cooper, Dr Acland, Dr Paget, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Mr Syme, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, Dr Sharpey, Dr Parkes, Dr Quain, Mr Rumsey, Dr Christison, Dr Stokes. Dr Francis Hawkins, *Registrar*.

The minutes of the last meeting were read and confirmed.

1. The following Committees were appointed:—

Business Committee.—Dr Andrew Wood, *Chairman*; Dr Alderson, Dr Embleton, Dr Corrigan, Mr Rumsey.

Finance Committee.—Dr Sharpey, *Chairman*; Mr Arnott, Dr Fleming, Dr Aquilla Smith, Dr Quain.

2. Moved by Dr Andrew Wood, seconded by Dr Corrigan, and agreed to,—"That the consideration of the question of the amendment of the Medical Acts shall take precedence of all other business."

3. Moved by Dr Andrew Wood, seconded by Mr Hargrave, and agreed to,—"That the communications which have been received relative to the amendment of the Medical Acts be referred to a Committee, with instructions to report upon them to the Council; the Committee to consist of Dr Embleton, *Chairman;* Dr Paget, Dr Fleming, Dr Aquilla Smith."

4. Moved by Dr Andrew Wood, seconded by Dr Embleton, and agreed to,—"That the resolutions of the Branch Councils on the subject of the amendments of the Medical Acts be now read."

RESOLUTIONS RELATIVE TO SECTION XX. OF THE MEDICAL ACT.

Branch Council for England.

Resolved,—"1. That consistently with the existing provisions of the Medical Act, the Medical Council may make known what they consider to be requisite as a sufficient course of education and examination, and may, if needful, represent cases of deficiency to the Privy Council, for correction; and that the Medical Council has, further, the power of supervising examinations, with a view to their efficiency;—that hitherto these provisions of the Act have not been proved by actual trial to have failed of their purpose, and that it would be impolitic to apply to the legislature for other powers until those already conferred shall have been practically shown to be insufficient.

"2. That whilst the Council have already a means of correcting proved deficiency, they would probably meet with serious difficulty in framing a body of positive regulations to be sanctioned by the Privy Council as universally and rigorously obligatory;—that, on the one hand, a scheme of education which should carry such general assent in the Council as to obtain the sanction of higher authority, might be so general in its provisions as to involve no material alteration of existing regulations, and, if so, would be superfluous and nugatory; that if, on the other hand, it were rendered more specific and rigorous, it would be liable unduly to trammel the free action of licensing bodies and schools, and stand in the way of variations of procedure which might be salutary and convenient. Lastly, that a particular scheme having been once fixed by the Privy Council, no modification or re-adjustment could be obtained without recourse to the same authority, and that, although a system of rules were framed by the Medical Council, cases of alleged contravention would still have to be brought before the Privy Council to be construed and decided."

Branch Council for Scotland.

To confer on the Medical Council definite powers to issue to the various licensing bodies regulations on the subjects of preliminary and professional education and examination.

In order to carry out this object, the Branch Council recommend clauses to the following effect:—"That it shall be lawful for the General Medical Council, from time to time, to issue to the various licensing bodies such regulations respecting the preliminary and professional education and examination of persons desirous of obtaining any of the qualifications mentioned in Schedule (A) to the Medical Act, as may appear to the Council fitted to secure, on the part of such persons, the requisite knowledge and skill for the efficient practice of their profession.

"That all such regulations as shall have been passed by a majority of two-thirds of the General Council shall be obligatory on all universities, colleges, and other bodies enumerated in Schedule (A) to the Medical Act of 1858.

"That, in the event of any of the said bodies not conforming to such regulations, it shall be lawful for the General Council, if they see fit, to intimate to the said body that it has not conformed to such regulations; and to direct that, in the event of the said body not conforming within six months after such intimation, the qualification granted by such body, after the lapse of the said period of six months, shall not be registered.

"That it shall be lawful for any body, in regard to which such direction shall have been given, to appeal to the Privy Council, who shall have power, if they see cause, to disallow the direction of the Medical Council.

"That it shall be lawful for the General Council to restore any right to registration which may have been suspended by them, when they shall be satisfied that their regulations have been conformed to."

Branch Council for Ireland.

Resolved,—"That the following be approved of as the Amendment of Sect. XX., the words 'Preliminary and professional' being inserted before the word 'education,' in line 3:—'Sect. XX. It shall be lawful for the General Council to lay down such regulations respecting the preliminary and professional education and examination of Practitioners in Medicine, Surgery, and Pharmacy, as may appear to them fitted to insure adequate knowledge and skill in the several departments of the profession; and the said General Council shall then submit said regulations to Her Majesty's Most Honourable Privy Council. And the said regulations, if sanctioned by the said Privy Council, shall then be obligatory upon all universities, colleges, and other bodies enumerated in Schedule (A) to this Act.'"

RESOLUTIONS RELATIVE TO SECTION XXXI. OF THE MEDICAL ACT.

Branch Council for England.

Resolved,—"That whereas the legal effect of this section is to require the great majority of persons about to enter the medical profession to possess qualifications in Medicine and Surgery from two different bodies, to obtain which such persons are repeatedly re-examined in the same branches of knowledge, and thereby subjected to superfluous and irksome labours and to serious embarrassment in the progress of their studies, as well as to extra expense, it is highly expedient that every means should be taken by the General Council to promote the adoption by qualifying bodies of the provisions of Section XIX. of the Medical Act."

Branch Council for Scotland.

It was resolved,—"That the Scottish Branch Council, having considered the propriety of amending Clause XXXI. of the Medical Act, are of opinion that what is objectionable in it would be obviated were the combination and co-operation of the licensing bodies, as provided for by Clause XIX., encouraged or even made obligatory, so that facilities may be given to medical students for acquiring the complete or double qualification, without having to pass repeated examinations on the same subjects, which is at once irksome and unnecessary; and in order that the public may be best provided with general practitioners who have been educated and tested in every branch of the profession, as well Physic as Surgery."

Branch Council for Ireland.

Resolved,—"That the following be approved of as as the Amendment of Section XXXI., the words, 'notwithstanding anything to the contrary in any other Act or Acts,' being inserted after the word 'Act' in line 1:—'Section XXXI. Every person registered under this Act, notwithstanding anything to the contrary in any other Act or Acts, shall be entitled, according to his qualification or qualifications, to practise Medicine, or Surgery, or Pharmacy; or Medicine, and Surgery, and Pharmacy, as the case may be, in any part of her Majesty's dominions, and to demand and recover in any court of law, with full costs of suit, reasonable charges for professional aid, advice, and visits, and the cost of any medicines or other medical or surgical appliances rendered or supplied by him to his patients: Provided always, that it shall be lawful for any College of Physicians to pass a bylaw to the effect that no one of their fellows or members shall be entitled to sue in manner aforesaid in any court of law; and thereupon such bylaw may be pleaded in bar to any action for the purposes aforesaid commenced by any fellow or member of such college.'"

RESOLUTIONS RELATIVE TO SECTION XL. OF THE MEDICAL ACT.

Branch Council for England.

Resolved,—"That, in the opinion of this Branch Council, Section XL. of the Medical Act should be amended as follows:—'Any person practising Medicine or Surgery, or being engaged in the treatment of diseases or injuries, for gain, not being registered under this Act, nor being able to give evidence of being qualified to be registered under this Act, who shall take or make use of any of the titles or designations enumerated in Schedule (A) to this Act, or that of Physician, Surgeon, Doctor, Professor of Medicine, Professor of Surgery, or any professional title, name, or distinction commonly used by, or used to distinguish, duly educated or qualified practitioners in Medicine or Surgery, shall, upon a summary conviction, be liable to a penalty not exceeding £ for each offence.'"

Branch Council for Scotland.

To amend Clause XL., so as to render it more efficient than it has hitherto been, for enabling the public to distinguish between qualified and unqualified practitioners, and for preventing unqualified practitioners from assuming medical titles to which they have no right.

In order to carry out this object the Branch Council recommend a clause to the following effect:—"It shall not be lawful for any person, unless registered under this Act, to practise any branch of the profession, taking or using the name or title of a Physician, Doctor of Medicine, Licentiate in Medicine or Surgery, Master in Surgery, Bachelor of Medicine, Doctor, Surgeon, Medical Practitioner, or General Practitioner, or Surgeon-apothecary, or Licentiate or Practitioner in Midwifery, or Professor of Medicine, or Professor of Surgery, or any other medical or surgical title; and every unregistered person so offending shall, upon summary conviction for such offence, forfeit or pay a sum not exceeding £20."

Branch Council for Ireland.

Resolved,—"That the following be approved of as the Amendment of Section XL.:—'Sect. XL. On and after the day of 186 , it shall not be lawful for any person, unless registered under this Act, to pretend to be, or take or use the name or title of Physician, Doctor of Medicine, Licentiate in Medicine or Surgery, Master in Surgery, Bachelor of Medicine, Doctor, Surgeon, Medical or General Practitioner, or Surgeon, or Apothecary, or Accoucheur, or Licentiate or Practitioner in Midwifery, or any other medical or surgical name or title; and any unregistered person so offending shall forfeit and pay a sum not exceeding £20, to be recovered in a summary way before the justices of the peace.'"

5. Moved by Dr Andrew Wood, and seconded by Mr Syme,—"That the Council now proceed to consider the expediency of amending Clause XX. of the Medical Act, and that the following resolution be adopted:—

"That the amended Bill should contain clauses conferring on the Medical Council definite powers to issue to the various licensing bodies, regulations on the subject of preliminary and professional education and examination; and that, in order to carry out that object, clauses to the following effect be adopted, viz.:—

"'That it shall be lawful for the General Medical Council from time to time to issue to the various licensing bodies such regulations respecting the preliminary and professional education and examination of persons desirous of obtaining any of the qualifications mentioned in Schedule (A) to the

Medical Act, as may appear to the Council fitted to secure, on the part of such persons, the requisite knowledge and skill for the efficient practice of their profession.

"'That all such regulations as shall have been passed by a majority of two-thirds of the General Council shall be obligatory on all universities, colleges, and other bodies enumerated in Schedule (A) to the Medical Act of 1858.

"'That, in the event of any of the said bodies not conforming to such regulations, it shall be lawful for the General Council, if they see fit, to intimate to the said body that it has not conformed to such regulations; and to direct that, in the event of the said body not conforming within six months after such intimation, the qualification granted by such body, after the lapse of the said period of six months, shall not be registered.

"'That it shall be lawful for any body in regard to which such direction shall have been given, to appeal to the Privy Council, who shall have power, if they see cause, to disallow the direction of the Medical Council.

"'That it shall be lawful for the General Council to restore any right to registration which may have been suspended by them, when they shall be satisfied that their regulations have been conformed to.'"

Amendment, moved by Dr Corrigan, and seconded by Dr Sharpey,—"That the whole Medical Act be taken into consideration, clause by clause, for approval or amendment; as the taking into consideration of only three clauses must lead to an imperfectly considered bill, which would not command attention from the legislature."

The amendment was negatived: the motion was then put to the vote, and was also negatived.

MINUTES OF MEETING, *Wednesday*, *5th April* 1865.

Present,—Dr Burrows, *President*, in the Chair; Dr Alderson, Mr Arnott, Mr Cooper, Dr Acland, Dr Paget, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Mr Syme, Dr Thomson, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, Dr Sharpey, Dr Parkes, Dr Quain, Mr Rumsey, Dr Christison, Dr Stokes. Dr Francis Hawkins, *Registrar*.

The minutes of the last meeting were read and confirmed.

1. Moved by Dr Andrew Wood, and seconded by Dr Leet,—"That the following be adopted as an amendment of Clause XX. of the Medical Act: 'It shall be lawful for the General Council, from time to time, to lay down such regulations respecting the education and examination of practitioners in Medicine and Surgery as may appear to them fitted to ensure adequate knowledge and skill in the practice of their profession. And the said General Council shall submit said regulations to her Majesty's Most Honourable Privy Council. And the said regulations, if sanctioned by the said Privy Council, shall then be obligatory upon all universities, colleges, and other bodies enumerated in the Act. And it shall be lawful for the Privy Council, upon its being represented to them that any university, college, or other body enumerated in the Act does not comply with such regulations, to declare that any qualification granted by such university, college, or body, shall not confer any right to be registered under the Act. Provided always, that it shall be lawful for her Majesty, with the advice of her Privy Council, when it is made to appear to her, upon further representation from the General Council or otherwise, that such college or body has made effectual provision, to the satisfaction of such General Council, for the improvement of its course of study or examinations, or the mode of conducting its examinations, to revoke any such order.'"

Amendment moved by Mr Syme, and seconded by Mr Hargrave,—"That Clause XX. shall stand as at present; and that if any of the bodies mentioned in Schedule (A) should decline or neglect to comply with the recommendations of the Council, in regard to education—whether preliminary or professional—the Council shall, if they see fit, express their disapprobation."

The amendment was negatived.

Amendment moved by Dr Embleton, and seconded by Mr Rumsey,—"That Section XX. of the Medical Act of 1858 stand as at present."

The amendment was carried, and was then put as a substantive motion.

Amendment moved by Dr Parkes, and seconded by Dr Fleming,—"That Section XX. be amended as follows:—

"'That it shall be lawful for the General Medical Council, from time to time, to issue to the various licensing bodies such regulations respecting the preliminary and professional education and examination of persons desirous of obtaining any of the qualifications mentioned in Schedule (A) to the Medical Act, as may appear to the Council fitted to secure, on the part of such persons, the requisite knowledge and skill for the efficient practice of their profession.

"That all such regulations as shall have been passed by a majority of three-fourths of the General Council shall be obligatory on all universities, colleges, and other bodies enumerated in Schedule (A) to the Medical Act of 1858.

"That, in the event of any of the said bodies not conforming to such regulations, it shall be lawful for the General Council, if they see fit, to intimate to the said body that it has not conformed to such regulations; and that, in the event of the said body not conforming within six months after such intimation, a representation be made to the Privy Council, to authorize the suspension of the registration of the degrees or licences of such body.

"That it shall be lawful for any body, to which such an intimation shall have been given, to appeal to the Privy Council, who shall have power, if they see cause, to disallow the direction of the Medical Council.

"That it shall be lawful for the General Council to recommend the restoration of any right to registration which may have been suspended by the Privy Council, when they shall be satisfied that their regulations have been conformed to."

The amendment was negatived. The substantive motion was then put to the vote, and carried.

2. The resolutions of the Branch Councils relative to Section XXXI. of the Medical Act were read.

Moved by Dr Quain, and seconded by Dr Alderson,—"That no amendment of Clause XXXI. be adopted."

Amendment moved by Dr Embleton, and seconded by Dr Paget,—"That the consideration of Clause XXXI. be deferred until the Council is made acquainted with the provisions of the Pharmacy Bills at present before Parliament."

The amendment was carried, and was then put as a substantive motion, and carried.

3. The resolutions of the Branch Councils relative to Section XL. of the Medical Act were read.

Moved by Dr Quain, and seconded by Dr Paget,—"That the resolution of the Branch Council for England be adopted, with the omission of the words 'for gain,' as the amendment of Section XL., viz.:—'Any person practising Medicine or Surgery, or being engaged in the treatment of diseases or injuries, not being registered under this Act, nor being able to give evidence of being qualified to be registered under this Act, who shall take or make use of any of the titles or designations enumerated in Schedule (A) to this Act, or that of Physician, Surgeon, Doctor, Professor of Medicine, Professor of Surgery, or any professional title, name or distinction commonly used by, or used to distinguish, duly educated or qualified practitioners in Medicine or Surgery, shall, upon a summary conviction, be liable to a penalty not exceeding £ for each offence.'"

Amendment moved by Dr Andrew Wood, and seconded by Dr Christison,—"That Section XL. be amended as follows:—'Any person practising Medicine or Surgery, or being engaged in the treatment of diseases or injuries, not being registered under this Act, who shall take or make use of any of the titles or designations enumerated in Schedule (A) to this Act, or that of Physician, Surgeon, Doctor, Professor of Medicine, Professor of Surgery, or any other title, name, or designation used by, or used to distinguish, duly qualified practitioners in Medicine or Surgery, shall, upon a summary conviction, be liable to a penalty not exceeding £20 for each offence.' "

The amendment was carried, and was then put as a substantive motion.

Amendment moved by Dr Corrigan, and seconded by Dr Stokes,—" That the amendment of Section XL., proposed by the Irish Branch Council, be adopted, viz. :—' On and after the day of 186 , it shall not be lawful for any person, unless registererd under this Act, to pretend to be, or take or use the name or title of Physician, Doctor of Medicine, Licentiate in Medicine or Surgery, Master in Surgery, Bachelor of Medicine, Doctor, Surgeon, Medical or General Practitioner, or Surgeon, or Apothecary, or Accoucheur, or Licentiate or Practitioner in Midwifery, or any other medical or surgical name or title; and any unregistered person so offending shall forfeit and pay a sum not exceeding £20, to be recovered in a summary way before the justices of the peace.' "

The amendment was negatived. The substantive motion was then put to the vote, and carried.

4. Moved by Dr Paget, and seconded by Dr Apjohn,—" That the communications from the Directors-General of the Army and Navy Medical Departments be printed, for the use of the members of the Council."

Amendment moved by Dr Storrar, and seconded by Dr Acland,—" That the communications from the Medical Departments of the Army and Navy be read."

The amendment was carried, and then put as a substantive motion, and carried.

MINUTES OF MEETING, *Thursday, April* 6, 1865.

Present,—Dr Burrows, *President*, in the Chair; Dr Alderson, Mr Arnott, Mr Cooper, Dr Acland, Dr Paget, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Mr Syme, Dr Thomson, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, Dr Sharpey, Dr Parkes, Dr Quain, Mr Rumsey, Dr Christison, Dr Stokes. Dr Francis Hawkins, *Registrar*.

The minutes of last meeting were read and confirmed.

1. *Read*—The following communications from the Directors-General of the Army and Navy Medical Departments, relative to the examinations of candidates for Medical Commissions.—(Minutes General Council for May 7, 1864; and Minutes Executive Committee for June 24, 1864.)

F. Hawkins, Esq., Registrar. Army Medical Department, June 20, 1864.

Sir,—In acknowledging the receipt of your letter, dated 27th ultimo, conveying the request of the General Council of Medical Registration to be furnished annually with information on the following points, viz. :—" (*a*) The total number of candidates for Medical Commissions who have presented themselves for examination;—(*b*) The number of those who passed, and of those who did not pass the examinations of the Board, distinguishing the number of successful and unsuccessful candidates under the respective heads of the several Licensing Bodies mentioned in Schedule (A) to the Medical Act, and specifying their qualifications, Medical and Surgical, and whether they had failed in Medicine or Surgery;—(*c*) The general nature and scope of the examination conducted by the Board, together with a list of the questions proposed by the examiners;"—I have the honour to inform you, in reply, that Lord de Grey has been pleased to accede to the wish of the General Council, and I shall be obliged by your signifying from what date the information is required. J. B. GIBSON, *Director-General.*

F. Hawkins, Esq., Registrar. Army Medical Department, February 23, 1865.

Sir,—With reference to your letters dated 27th May and 24th June last, the former communicating a resolution passed at a late session of the General Council of Medical Education, that certain information should be furnished by this Department annually, in regard to the number of candidates who have presented themselves for examination, and specifying certain points on which the Council request to be informed, I have the honour to forward a statement on the subject, with a list of the questions proposed by the Examiners, and to observe, in regard to the general nature and scope of the examination, that the enclosed copy of the regulations for the admission of candidates into the Medical Department of Her Majesty's Army (section 5, page 8),[1] will probably supply the information which the Council are desirous to obtain.

J. B. Gibson, *Director-General.*

Statement of the Degrees, Diplomas, and Licenses of the Candidates for Commissions in the Medical Department of the Army, who during the year 1864 *have presented themselves for Examination, showing the number that passed, and did not pass, also distinguishing the Qualifications, both Medical and Surgical, under the heads of the several Licensing Bodies, and specifying whether the Candidates failed in Medicine or Surgery.*

Names of Licensing Bodies.	Total Qualifications.	No. Passed.	Failed.	Unsuccessful.			
				In Medicine.	In Surgery.	Failed in Med. & Surg.	Failed in Anatomy.
Royal College of Physicians, London	2	2	...	...	...	...	...
Royal College of Physicians, Edinburgh	27	17	10	4	3	1	2
King and Queen's College of Physicians, Ireland	30	21	9	3	3	2	1
Royal College of Surgeons, England	44	33	11	3	3	3	2
Royal College of Surgeons, Edinburgh	27	23	4	2	...	...	2
Faculty of Physicians and Surgeons, Glasgow	2	2	...	...	...	...	...
Royal College of Surgeons, Ireland	61	47	14	6	4	3	1
Society of Apothecaries, London	21	15	6	1	1	2	2
Apothecaries' Hall, Dublin	1	1	...	...	...	...	...
Doctor of Medicine, University of Edinburgh	16	14	2	2	...	...	...
Doctor of Medicine, Queen's University, Ireland	18	17	1	1	...	...	...
Doctor of Medicine, University of Dublin	...	...	...	...	...	...	...
Bachelor of Medicine, ditto	9	8	1	...	...	1	...
Master of Surgery, ditto	2	1	1	..	...	1	...
Licence in Medicine, ditto	1	...	1	...	...	1	...
Doctor of Medicine, St Andrews	8	7	1	1	...	...	...
Doctor of Medicine, University of Aberdeen	5	4	1	...	...	1	...
Bachelor of Medicine, ditto	10	10	...	...	...	...	...
Master of Surgery, ditto	14	13	1	...	...	1	...
Doctor of Medicine, University of Glasgow	6	5	1	...	...	...	1
Master of Surgery, ditto	2	1	1	...	...	...	1
Total	306	241	65	23	14	16	12

Candidates.	
Successful	120
Failed	31
Total	151

Diplomas and Degrees.	
Successful	241
Failed	65
Total	306

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[1] Subjects of Examination.—On producing the foregoing qualifications the candidate will be examined by the Examining Board in the following subjects (*on next page*) :—

Dr F. Hawkins, Registrar. Admiralty, March 13, 1865.

Sir,—With reference to your letter of the 27th of May last, I have the honour to forward, for the information of the General Council of Medical Education and Registration of the United Kingdom, a Report from the Board of Examiners on the examinations of candidates for Medical Commissions in the Royal Navy during the year 1864. A. BRYSON, *Director-General.*

Dr Bryson. Admiralty, March 6, 1865.

Sir,—We have the honour to submit, for the information of the General Council of Medical Education and Registration of the United Kingdom, the following Report on the Examinations of Candidates for Medical Commissions in the Royal Navy during the year 1864.

1. The total number of candidates who presented themselves for examination during the year was forty-nine; of these twenty-one were rejected, having failed to satisfy us as to their professional knowledge, and one was found to be physically unfit for the service.

2. The accompanying table supplies the information required by the Medical Council with reference to the qualifications of the candidates, and the points on which they were chiefly deficient.

3. The examination is *vivâ voce*, but each candidate is furnished with a paper containing a question or questions upon subjects of a professional nature, to which he is expected to reply in writing, and previous to the oral examination, he is required to translate a paragraph from a Latin author.

4. In its scope the examination embraces all the ordinary subjects of professional study, viz., Anatomy, Surgery, Practice of Medicine, Midwifery, Materia Medica, Chemistry, and Botany.

5. Being a *vivâ voce* examination, and the questions being left to the judgment of each individual examiner, it is not possible to furnish a return of those actually proposed, but appended hereto is a list of the subjects on which the candidates have at different times been examined, and which, put in the interrogative form, gives a fair idea of the general character of the questions. The Council is doubtless fully aware that in such examinations the same question requires to be put in a variety of forms, to meet the various comprehensions to which they are addressed.

6. We regret to have to report that in a very large number of cases indeed the candidates displayed a lamentable ignorance of Latin; some were scarcely able to translate the "Pharmacopœia," and but few possessed a useful knowledge of the language. From the nature of the excuse most frequently made

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Anatomy and Physiology. Surgery.

Medicine, including Therapeutics, the Diseases of Women and Children, Chemistry and Pharmacy, and a practical knowledge of drugs.

(The examination in Medicine and Surgery will be in part practical, and will include operations on the dead body, the application of Surgical Apparatus, and the examination of Medical and Surgical patients at the bedside.)

The eligibility of each candidate for the Army Medical Service will be determined by the result of the examinations in these subjects only.

Candidates who desire it will be examined in Comparative Anatomy, Zoology, and Botany, with special reference to Materia Medica; and the number of marks gained in these subjects will be added to the total number of marks obtained in the obligatory part of the examination by candidates who shall have been found qualified for admission, and whose position on the list of successful competitors will thus be improved in proportion to their knowledge of these branches of science.

The subjects for this branch of the examination will be taken from the following books:

(1.) "Animal Kingdom," by W. S. Dallas, F.L.S.

(2.) "Outlines of the Structure and Functions of the Animal Kingdom," by Rymer Jones; or "Cours Elémentaire d'Histoire Naturelle," par Milne-Edwards.

(3.) Lindley's "School Botany;" Lindley's "Medical and Economic Botany;" Henfrey's "Elementary Course of Botany."

Candidates who may desire it, may also be examined in the Elements of Physics and in Physical Geography. The following books are recommended for this purpose:

(1.) "Elements of Natural Philosophy," by Golding Bird and C. Brooks.

(2.) "Physical Geography," by Mrs Sommerville.

by them, it appears to us that very many had acquired only a sufficient acquaintance with the language to enable them to pass the preliminary examination of other Boards, and that they had then thrown it aside as altogether useless.

7. We have also to regret that so important a branch of professional education as Operative Surgery on the dead body should so rarely enter into the curriculum of study of those who come before us; and we desire to express a strong opinion that no surgical diploma should be attainable, without satisfactory evidence being produced that the principal operations in Surgery had been performed on the dead body under a qualified teacher.

E. HILDITCH, M.D., *Inspector-General.*
J. W. SALMON, *Deputy Inspector-General.*
ALEX. E. MACKAY, M.D., *Deputy Inspector-General.*

Qualifications, according to Schedule (A) *of the different Candidates who were examined for Medical Commissions in the Royal Navy in* 1864, *with the Results of the Examinations.*

[For convenience of printing, the facts contained in the table referred to are given in the following summary.]

1. Mem. R. Coll. Surg. Eng., and M.D. Univ. Edin. Rejected. Deficient in anatomy, surgery, and practice of medicine.
2. Lic. R. Coll. Surg. Edin., and M.D. Univ. Edin. Passed. Surgery and practice of medicine only fair; otherwise good.
3. Lic. R. Coll. Surg. Irel. Found to be physically unfit.
4. M.D. Univ. Aberd., and Mast. Surg. Univ. Aberd. Passed a good examination in all branches.
5. Lic. K. and Q. Coll. Phys. Irel., and Lic. R. Coll. Surg. Irel. Rejected. Ignorant of anatomy.
6. Lic. R. Coll. Surg. Irel. Passed. Anatomy, surgery, and practice of medicine, only fair; other branches good.
7. Mem. R. Coll. Surg. Eng., and M.D. Univ. Edin. Passed a good examination in all branches.
8. Mem. R. Coll. Surg. Eng., Lic. Soc. Apoth. Lond., M.D. Univ. Aberd., and Mast. Surg. Univ. Aberd. Passed. Practice of Medicine only fair; other branches good.
9. M.B. Univ. Aberd., and Mast. Surg. Univ. Aberd. Passed a good examination in all branches.
10. Lic. R. Coll. Surg. Irel., and M.D. Univ. St Andrews. Passed a good examination in all branches.
11. Lic. R. Coll. Phys. Lond., and Mem. R. Coll. Surg. Eng. Passed. Anatomy and surgery good; practice of medicine and other branches only fair.
12. Lic. K. and Q. Coll. Phys. Irel., and Lic. R. Coll. Surg. Irel. Passed. Anatomy and practice of medicine, and all other branches, except surgery, good; surgery fair only.
13. Lic. R. Coll. Phys. Lond., and Mem. R. Coll. Surg. Eng. Rejected. Utterly ignorant of the Latin language.
14. Lic. K. and Q. Coll. Phys. Irel., and Lic. R. Coll. Surg. Irel. Passed. Anatomy and practice of medicine good; surgery fair only; other branches fair and indifferent.
15. Lic. R. Coll. Phys. Lond., and Mem. R. Coll. Surg. Eng. Passed a good examination in all branches.
16. Mem. R. Coll. Surg. Eng., and M.B. Univ. Durham. Rejected for the third time. Anatomy and surgery bad.
17. Lic. R. Coll. Surg. Irel. Rejected for the second time. Anatomy and surgery bad.
18. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Rejected. Utterly ignorant of the Latin language.

19. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Passed a good examination in all branches.

20. Lic. R. Coll. Surg. Edin., and M.D. Univ. Edin. Passed. Anatomy and surgery good; practice of medicine and other branches only fair.

21. Lic. K. and Q. Coll. Phys. Irel., and Lic. R. Coll. Surg. Irel. Rejected at the first examination. Anatomy only fair; surgery very bad. Passed the second examination. Surgery and other branches were fair.

22. Lic. R. Coll. Phys. Edin., and Lic. R. Coll. Surg. Edin. Rejected. Anatomy and surgery indifferent.

23. Lic. R. Coll. Surg. Irel. Rejected. Ignorant of Latin and anatomy.

24. Lic. R. Coll. Surg. Irel. Rejected. Utterly ignorant of the Latin language.

25. Lic. R. Coll. Phys. Edin., and Lic. R. Coll. Surg. Edin. Passed. Anatomy good; surgery only fair; practice of medicine indifferent; other branches fair.

26. Lic. R. Coll. Surg. Irel. Passed. Anatomy, surgery, and practice of medicine good; other branches fair.

27. M.D. Univ. Aberd., and Mast. Surg. Univ. Aberd. Passed. Anatomy and surgery only fair; practice of medicine good; other branches fair.

28. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Rejected. Anatomy bad; surgery and practice of medicine indifferent.

29. Lic. R. Coll. Phys. Edin., and Lic. R. Coll. Surg. Irel. Passed. Anatomy good; surgery and practice of medicine fair; other branches fair.

30. Lic. R. Coll. Phys. Edin., and Lic. R. Coll. Surg. Edin. Rejected. Anatomy indifferent; surgery bad.

31. Lic. R. Coll. Phys. Edin., and Lic. Fac. Phys. and Surg. Glasg. Rejected. Utterly ignorant of the Latin language.

32. Lic. R. Coll. Surg. Edin., and M.D. Queen's Univ. Irel. Passed. Anatomy indifferent; surgery, practice of medicine, and other branches, only fair.

33. Lic. R. Coll. Phys. Edin., Lic. R. Coll. Surg. Edin., and Lic. Apoth. Hall, Dublin. Rejected. Anatomy only fair; surgery bad; practice of medicine indifferent.

34. Lic. R. Coll. Phys. Edin., and Lic. R. Coll. Surg. Edin. Passed. Anatomy and practice of medicine good; surgery only fair; other branches fair.

35. Lic. Fac. Phys. and Surg. Glasg. Passed. Anatomy good; surgery fair; practice of medicine indifferent; other branches fair.

36. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Rejected at the first examination. Anatomy and surgery bad. Passed in 1865, second examination. Passed a good examination in all branches.

37. M.B. Univ. Aberd., and Mast. Surg. Univ. Aberd. Passed. Anatomy, surgery, and practice of medicine good; other branches fair.

38. Mem. R. Coll. Surg. Eng. Rejected. Latin and anatomy bad.

39. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Rejected. Anatomy indifferent; surgery only fair; other branches and Latin bad.

40. Lic. R. Coll. Surg. Edin., and M.D. Univ. Edin. Rejected. Anatomy indifferent; surgery bad; and other branches bad.

41. Mem. R. Coll. Surg. Eng., and M.D. Univ. St Andrews. Passed second examination. Anatomy, surgery, materia medica, and botany good; other branches fair.

42. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Rejected. Latin, anatomy, and surgery bad.

43. Mem. R. Coll. Surg. Eng., and M.D. Univ. Edin. Passed a good examination in all branches.

44. M.B. Univ. Aberd., and Mast. Surg. Univ. Aberd. Passed. Anatomy and practice of medicine good; surgery only fair; other branches fair.

45. Lic. R. Coll. Surg. Edin. Rejected. Latin and anatomy and surgery indifferent; materia medica bad.

46. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Passed a good examination in all branches.
47. Mem. R. Coll. Surg. Eng., and Lic. Soc. Apoth. Lond. Passed a good examination in all branches, excepting practice of medicine, which was only fair; Latin indifferent.
48. Lic. R. Coll. Surg. Irel., and Lic. Apoth. Hall, Dublin. Rejected. Utterly ignorant of the Latin language.

NOTE.—Several of the candidates possessed qualifications which are not included in Schedule (A).

Moved by Dr Andrew Wood, seconded by Dr Parkes, and agreed to,—"That the communications from the Directors-General of the Army and Navy Medical Departments, relative to the examinations of candidates for medical commissions, be received and entered on the minutes."

Moved by Dr Apjohn, seconded by Dr Paget, and agreed to,—"That the thanks of the Medical Council be given to the Directors-General of the Army and Navy Medical Boards, for the valuable returns which they have made of the results of the examinations held by them in 1864, of candidates for medical appointments in their respective services; and that a request be made to the Director-General of the Army Medical Department that he will be pleased to sanction for the future similar returns for Her Majesty's Indian Medical Service."

2. Dr Embleton brought up the report of the Committee on Communications relative to the amendment of the Medical Acts.

REPORT.

Your Committee have read and considered the letters, etc., referred to them, and find that some, having respect to Sections XL., XLI., XLII., and XLVII., of the Medical Act, on which the Council has already come to a decision, do not appear to require to be brought before the Council.

Others, however, referring to other matters, require consideration, and are now brought before the Council, viz.:

1. A letter from J. F. Milner, Esq., Hereford, dated December 2, 1864, praying for protection of registered medical practitioners against the practice of medicine by chemists and druggists, and against the holding of club appointments by the same.
2. A letter from E. E. Tucker, Esq., of Abersychan, dated February 3, 1865, complaining of the practice of surgery by a gentleman in his neighbourhood, whose only qualifications is that of the Society of Apothecaries of London.
3. A letter from Dr Styrap, Shrewsbury, dated March 3, 1864, asking that increased powers be given to magistrates with respect to offences against the Medical Act, and that the penalty on second conviction be increased.
4. The following memorial, dated April 3, 1865, from twenty-eight Licentiates in Dental Surgery of the R.C.S. England, asking that, under the proposed Medical Act, they be allowed to register as Licentiates in Dental Surgery:—

"*To the General Council of Medical Education and Registration of the United Kingdom.*

"We, the undersigned, Licentiates in Dental Surgery of the Royal College of Surgeons in England—(a degree founded by the college on Section XLVIII. of the present Medical Act)—although entitled to register under that Act, in virtue of our other professional qualifications, considering it to be desirable, and believing it to be only just, that those persons who have taken this degree should be allowed the privilege of registering as 'Licentiates in Dental Surgery,' under the proposed amended Medical Act, beg respectfully to bring this, our view of the subject, under the consideration of your honourable board.

"We are aware that the following objections have been raised to this privilege being allowed to the possessors of this degree:—

"*First.*—That, if the privilege were granted to persons who have taken this degree only, they would, under Clause XXXIV. of the present Medical Act, be considered in law as 'duly qualified medical practitioners,' and might, if so disposed, practise legally any or all branches of the medical profession, without having received a full medical education, and thereby interfere with the interests of the fully qualified medical man.

"*Second.*—That to grant this privilege to them would be to infringe upon and injure the existing rights of those persons at present practising dental surgery who do not possess this degree, by giving to the new Act a retrospective character; and,

"*Third.*—That it is open to those persons practising dental surgery, who desire to possess the privilege of registering under any Medical Act, to take the degree of 'Member' of the College of Surgeons as well as that of 'Licentiate in Dental Surgery,' and to register under the former title.

"To the first of these objections we respectfully submit, that the very slight additions we have suggested to the wording of Clauses XXXI. and XXXIV. of the present Medical Act would, if adopted, make it clear beyond the possibility of a doubt, that the possessors of this degree only, if registered as 'Licentiates in Dental Surgery,' could not legally practise any other branch of Medicine or Surgery, and therefore that the incorporation of these, or similar words, into any amended Medical Act, would fully meet and do away with this objection.

"To the second objection we also respectfully submit, that the few words proposed to be added to Clause LV. of the present Medical Act would, if adopted and incorporated with such amended Medical Act, completely protect all the existing rights of those persons at present practising dental surgery who do not possess this degree, as also the prospective rights of those at present preparing to practise dental surgery who may not have it in their power to take this degree, and so meet and do away with this objection.

"And to the third objection we respectfully submit:

"*First.*—That the argument on which it rests is founded upon an imperfect knowledge of the amount of 'special study' required to form the duly qualified dental surgeon, as we think will be apparent to your honourable board by a reference to the accompanying curriculum of education required by the College of Surgeons for the degree of Licentiate in Dental Surgery:—

"*Next.*—That to expect the full course of education required for the 'membership' of the college to be added by the dental surgeon to the curriculum required for his special diploma, in order to give him the power of registering under such Act, would be to exact a more extensive and more expensive professional education, both in respect to time and money, from the dental than from the general surgeon—a requirement which we most respectfully submit would be both unnecessary and unjust; and,

"*Lastly.*—That to be registered as a 'Fellow' or 'Member' of the college only, does not *in itself* afford any proof that persons so registered are duly qualified to practise the speciality of dental surgery, as we think will also be apparent to your honourable board after referring to the curriculum above named.

"Entertaining these views, we think it right to bring them under the attention of your honourable board, and we pray that you will take them into your favourable consideration."

Moved by Dr Embleton, seconded by Dr Fleming, and agreed to,—"That the report of the Committee on communications relative to amendments of the Medical Acts be received and printed, together with the four documents particularized, under the direction of the Business Committee."

3. Moved by Dr Andrew Wood, seconded by Dr Fleming, and agreed to,—"That the Council resolve itself into a Committee of Education, and that the President be requested to take the chair.

4. Moved by Dr Parkes, and seconded by Dr Paget,—"That a Committee of three be appointed, one member from each division of the kingdom, to be

called the Committee on Education. That the powers and duties of this Committee be: 1. To prepare a definite statement of the systems of examination of the different licensing bodies, as stated in the documents already sent in to the Council, so that the agreements and differences of the several systems may be brought into one view. For this purpose the Committee shall be enabled to seek further information, if necessary, from the different licensing bodies, through the Registrar of the Council. 2. To arrange a plan by means of which, without actual visitation, the different examinations may be continually supervised, and to submit this to the several Branch Councils, after which the Committee be empowered to act upon it. 3. To arrange a plan for the visitation of examinations, when that is necessary, either by the agency of members of the Council, or of other members of the profession selected especially for this duty, and to submit this plan to the Branch Council, and subsequently, when it has been approved of, to act upon it."

Amendment moved by Mr Syme, and seconded by Dr Andrew Wood,—"That each of the Branch Councils, or such of their members as may be deputed by such Councils, shall, from time to time, visit the examinations, preliminary as well as professional, conducted by the qualifying bodies in their respective divisions of the United Kingdom, and report the results of their observations to the General Council."

The motion of Dr Parkes having, with the permission of the Council, been withdrawn, Mr Syme's amendment became the substantive motion.

Amendment moved by Dr Paget, and seconded by Dr Quain,—"That the Branch Councils be instructed severally to organize a set of *trial visitations*, on a scale which may enable them to report upon the success of such visitations, the conditions for their efficiency, and the requisite means for rendering them adequately extensive."

The amendment was negatived. The substantive motion was then put, and carried.

5. Moved by Dr Andrew Wood, seconded by Dr Stokes, and agreed to,—"That the Council resume."

6. Dr Quain brought up the following report of the Pharmacopœia Committee:—

REPORT.

The Pharmacopœia Committee beg to report that, after much careful consideration, they requested Mr Warrington, of the Apothecaries' Hall, and Dr Redwood, of the Pharmaceutical Society, to undertake the preparation of the next edition of the "Pharmacopœia," under the supervision of the Committee. The gentlemen named accepted the duty, and they are engaged actively in its performance. The Committee, in the first instance, prepared an outline of the subjects which seemed to them to require revision; and these subjects are made the basis of careful inquiries by Messrs Warrington and Redwood, who have submitted, and will continue to submit, their reports thereon, together with such suggestions as they think proper to make, for the consideration and decision of the Committee. The Committee have also received valuable assistance from Dr Farre, appointed by the English Branch Council, from Dr Moore, appointed by the Irish Branch Council, and from Dr Christison, appointed by the Scottish Branch Council, to report on the improvement in the progress of Pharmacy; and they hope that the services of these gentlemen may be continued.

Taking the first edition of the "Pharmacopœia" as a basis, compiled, as it has been with great labour and expense, the Committee hope that, without making any very extensive or fundamental changes, the next edition will be found acceptable to the profession.

Moved by Dr Quain, seconded by Dr Andrew Wood, and agreed to,—"That the report of the Pharmacopœia Committee be received, and entered on the minutes."

MINUTES OF MEETING, *Friday, April* 7, 1865.

Present,—Dr Burrows, *President*, in the Chair; Dr Alderson, Mr Arnott, Mr Cooper, Dr Acland, Dr Paget, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Mr Syme, Dr Thomson, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, Dr Sharpey, Dr Parkes, Dr Quain, Mr Rumsey, Dr Christison, Dr Stokes. Dr Francis Hawkins, *Registrar*.

The minutes of the last meeting were read and confirmed.

1. The report of the Committee on communications relative to the amendment of the Medical Acts was taken into consideration.

Moved by Dr Acland, seconded by Dr Parkes, and agreed to,—"That with reference to the letter of Mr Milner, of Hereford, a Committee to be appointed, to consider and report whether the Medical Council is charged under the Medical Act with any duty in relation to medical and surgical practice by chemists and druggists; and whether any, and if so what, changes in the Medical Act are desirable with regard to it, and to consider and report on the two bills relating to Pharmacy now before Parliament." The committee to consist of: Dr Acland, *Chairman;* Dr Alderson, Dr Paget, Dr Storrar, Dr Thomson, Dr Apjohn, Dr Parkes, Mr Rumsey, Dr Christison.

Moved by Mr Arnott, seconded by Dr Andrew Wood, and agreed to,—"That a letter be written to Mr E. E. Tucker, to the following effect:—The Council has learned, with great regret, the particulars of the case stated in Mr Tucker's letter. The Council has not, under the present Act, the power to interfere for the protection of the public against ignorance of surgery on the part of practitioners who have no surgical qualification; but the subject now engages the serious consideration of the Council.

Moved by Mr Syme, seconded by Dr Andrew Wood, and agreed to,—"That Clauses XIX. and XXXI. be referred to the proposed Committee on the amendment of the Medical Acts."

Moved by Dr Embleton, seconded by Dr Fleming, and agreed to,—"That an answer be returned to Dr Styrap, to the effect that the subjects of his letter, of the 3d March 1864, are at present under the consideration of the Medical Council."

Moved by Dr Embleton, seconded by Dr Fleming, and agreed to,—"That the following four Sections of the Medical Act, viz., XXXVIII., XXXIX., XL., and XLI., be referred to the proposed Committee on the amendment of the Medical Acts."

Moved by Dr Storrar, seconded by Mr Hargrave, and agreed to,—"That the memorial from the Licentiates in Dental Surgery of the Royal College of Surgeons of England be referred to the proposed Committee on amendment of the Medical Acts."

2. Moved by Dr Sharpey, and seconded by Dr Storrar,—"1. That it is expedient to amend Clause XVIII., by adding to it the following words, or words to the same effect, viz.: 'and may also inspect the written answers of the candidates, and report concerning the examinations and answers to the General Council; and to the persons deputed by the General Council as aforesaid, in such number as may be determined by the General Council, with the approval of one of Her Majesty's principal Secretaries of State, there shall be paid such fees for services and such reasonable travelling expenses as shall from time to time be allowed by the General Council and approved by the Commissioners of Her Majesty's Treasury; and the said payments shall be made out of the residue of the monies annually received for carrying this Act into execution, after defraying the expenses of the General Council and the Branch Councils, and, if necessary, out of further monies to be provided for the said purpose by vote of Parliament.' 2. That the proposed amendment be referred to the Committee on the amendment of the Medical Acts."

Amendment moved by Dr Corrigan, and seconded by Dr A. Smith,—"That the professional examinations conducted by the several licensing bodies should be public, so far as admitting the graduates, members or licentiates of the licensing body conducting such examination, and that such publicity will be sufficient to secure the maintenance of efficient examinations without inspection."

The amendment was negatived.

Amendment moved by Dr Andrew Wood, and seconded by Dr Allen Thomson,—"That it be remitted to the Committee on the Amendment of the Medical Acts to consider the expediency of amending Clause XVIII., by adding to it the following words, or words to the same effect, viz: 'and may also inspect the written answers of the candidates, and report concerning the examinations and answers to the General Council; and to the persons deputed by the General Council as aforesaid, in such number as may be determined by the General Council, with the approval of one of Her Majesty's principal Secretaries of State, there shall be paid such fees for services and such reasonable travelling expenses as shall from time to time be allowed by the General Council and approved by the Commissioners of Her Majesty's Treasury; and the said payments shall be made out of the residue of the monies annually received for carrying this Act into execution, after defraying the expenses of the General Council and the Branch Councils, and, if necessary, out of further monies to be provided for the said purpose by vote of Parliament."

The amendment was carried, and was then put as a substantive motion, and carried.

Dr Corrigan required that the names of the majority and minority should be taken down.

Majority: Dr Alderson, Dr Paget, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Mr Syme, Dr Thomson, Dr Sharpey, Dr Quain Dr Christison. Minority: Mr Arnott, Mr Cooper, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, Dr Stokes.

3. Moved by Dr Quain, seconded by Mr Hargrave, and carried,—"That a Committee of the Council be appointed, with the following duties: 1. To report on any further amendments which may seem to be required in the Medical Act. 2. To communicate with the Solicitor on the amendments already adopted, or that may be adopted, by the Council. 3. To prepare a memorial to the Home Secretary on the amendment of the Medical Act."

Dr Andrew Wood required that the numbers of the majority and minority be entered on the minutes; majority, 15; minority, 4.

The following Committee was appointed:—Dr Andrew Wood, *Chairman;* Mr Arnott, Dr Paget, Dr Embleton, Mr Syme, Dr A. Smith, Dr Sharpey, Dr Quain, Dr Stokes.

4 Moved by Dr A. Smith, seconded by Dr Apjohn, and agreed to,—"That the case of John Carter Barrett, of Castlebar, county Mayo, be referred to the Solicitor."

MINUTES OF MEETING, *Saturday, April* 8, 1865.

Present,—Dr Burrows, *President*, in the Chair. Dr Alderson, Mr Arnott, Mr Cooper, Dr Paget, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Mr Syme, Dr Thomson, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, Dr Sharpey, Dr Quain, Dr Christison, Dr Stokes. Dr Francis Hawkins, *Registrar.*

The minutes of the last meeting were read and confirmed.

1. Read—The following case and opinion of counsel, relative to voting in the Medical Council, together with an additional statement, and further opinion of counsel on the same subject.—(See Minutes of General Council for May 7, 1864, sect. 3; and Minutes of Executive Committee for Oct. 21, 1864).

CASE.

"Counsel will please see the Medical Act, 1858, 21 and 22 Vict. cap. 90.

"By the 9th section, which regulates the meetings of the General Medical Council, it is enacted that 'all acts of the General Council shall be decided by the votes of the majority of the members present at any meeting, the whole number not being less than eight; and at all such meetings the President for the time being shall, in addition to his vote as a member of the Council, have a casting vote, in case of an equality of votes.'

"At a recent meeting of the Council, fourteen members, including the Chairman, were present.

"On a motion being put to the meeting, six hands were held up for it and six against it, the President and one other member of the Council not voting.

"The President thereupon declared that the motion was not carried.

"A member of the Council required the President to give his casting vote, which the President refused to do, adhering to his decision that the motion was not carried.

"The Council desires to be advised as to the effect of the Act of Parliament in respect to voting.—Are all the members present bound to vote? If any member declines to vote, must he be counted, in ascertaining whether any question is carried by the *votes* of a majority of the members present, or may he, by declining to vote, be considered and treated as if he were not present? Is the President, if the votes shall be equal (however that equality is to be ascertained), bound to give a casting vote? May he give such casting vote although he has not given his original vote as a member of Council? or, may he simply, in the event of an equality, declare the motion not carried, without giving his casting vote?

"If the Act is to be read strictly, every motion must be carried by the votes of a majority of the persons present, and it is presumed that the term 'equality of votes' used in the clause must be construed with reference to the prior part of the clause. In the case which actually occurred construing the Act strictly, there was no such majority. Six voted for the motion, six against it, and two did not vote. If the latter are to be counted, the motion was clearly negatived, and no case arose which would give the Chairman the right to a casting vote.

"If the Act may be read as meaning 'by the votes of a majority of the members present' (and voting), then there was an 'equality of votes,' and the question arises whether the President was bound to give a casting vote.

"Counsel is requested to advise on the construction of the Act with reference to the above observations."

OPINION.

"The question, whether all the members present are bound to vote, seems to be one of little practical importance. It is clear that any member could retire during the time of voting, if he did not desire to record his vote, and this perhaps would be the more strictly regular method of declining to vote; but if the question were to be decided in a court of law, whether everybody remaining in the room was compelled to vote, I think the decision would be in the negative, and that it would be considered that any member declining to vote might, for that purpose, be considered as having retired from the meeting.

"My answer to the first question involves the answer to the second. If it is true that a member may remain in the room and yet decline to vote, of course he cannot be counted in ascertaining on which side the majority is.

"The question with respect to the President's votes seems to me also to pertain more to form than to substance, for it is clear that a President, content by his inaction to leave a motion to be lost, might, if compelled to act, vote against it; and as it would be impossible ever to hold that a motion was carried on which the numbers were equally divided, and which the President therefore declared to be lost, I hardly see how the question ever could be raised. Still, in point of form, it seems to me to be proper that every one present at the voting and taking an active part in the business, which is necessarily the case with the President, ought to vote; for it is impossible to consider such a one as retiring from the meeting. If this is so, it follows that the President ought not to give his casting vote without first having given his private vote. ARTHUR HOBHOUSE."

ADDITIONAL STATEMENT.

"With reference to the second branch of the opinion, as to the votes of the President in the Chair, the Medical Council would desire to ask for further explanation.

"In many cases questions may arise on which the President might desire to reserve his opinion, either on public or private grounds.

"The Act says that the President, in addition to his vote as a member, is to have a casting vote. The Act does not, however, say that he shall be bound to exercise the privilege; and the Council do not clearly collect from your opinion, whether the President is legally justified (in the case of an equality of votes of the other members voting) in declaring the motion not carried, without giving either an original or a casting vote."

OPINION.

"I can only repeat my former opinion in slightly amplified language. If the President wishes a motion not to be carried, the more regular course is that he should vote against it. This he may do without expressing any opinion except that which is necessarily involved in his vote, viz., an opinion that, for some reason or other, it is not expedient to carry the motion.

"The same opinion seems to me to be as much involved in the alternative course; for if the President is content to declare a motion lost, which by his vote he might carry, he obviously thinks that, for some reason or other, it is not expedient to carry the motion, otherwise he would vote for it.

"At the same time, if the President chooses to take the second course rather than the first, I do not think that he commits any illegality. It is, in my view, a mere irregularity, productive of no practical consequences at all; and I do not see how the question could be raised in any court of justice.

"ARTHUR HOBHOUSE."

2. Moved by Dr Stokes, and seconded by Dr Andrew Wood,—"That with reference to the Resolution of this Council, on the motion by Dr Leet, June 2, 1863, viz.: 'That this Council is of opinion that registered licentiates of the Apothecaries' Company of Dublin are, as apothecaries, entitled to practise Medicine in Great Britain and Ireland,' the Council resolve that it did not desire to convey that the licence of the Apothecaries' Company of Ireland carried any other qualification than that specified in the Act of incorporation of the company."

Amendment moved by Mr Syme, and seconded by Dr Storrar,—"That the Council do not see any reason for entering into an explanation of their resolution (June 2, 1863) respecting the licence of the Apothecaries' Company of Dublin."

The amendment was carried; and having been then put as a substantive motion, was also carried.

Mr Syme required that the numbers and names of the majority and minority, and of those who did not vote, should be taken down.

Majority, 10,—Dr Alderson, Dr Embleton, Dr Storrar, Dr Fleming, Mr Syme, Dr Thomson, Mr Hargrave, Dr Leet, Dr Sharpey, Dr Christison.

Minority, 6,—Mr Arnott, Dr Andrew Wood, Dr A. Smith, Dr Apjohn, Dr Corrigan, Dr Stokes.

Did not vote, 3,—The President, Mr Cooper, Dr Quain.

3. Moved by Dr Stokes, seconded by Mr Arnott, and agreed to,—"That a committee be appointed to consider and report as to what should be the subjects of general education in which all students should be examined prior to the commencement of their professional studies; the Committee to consist of,—Dr Stokes, *Chairman;* Mr Arnott, Dr Acland, Dr Paget, Dr Storrar, Dr Thomson, Dr Apjohn, Dr Quain."

ROYAL INFIRMARY OF EDINBURGH.

DR SCORESBY-JACKSON has been appointed Physician to the Royal Infirmary in the room of Dr Warburton Begbie, whose period of service expires on the 15th of May.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Observations on some of the Changes which take place after Re-section of Bones and Joints.* By THOMAS ANNANDALE, F.R.C.S., Assistant-Surgeon to the Royal Infirmary, Lecturer on Surgery, Edinburgh.

ANY facts which may tend further to establish or improve conservative surgery must always be interesting to the practical surgeon. I am therefore induced to give an account of several dissections and other observations which I have made in connexion with this subject during the last few years.

CASE 1.—*Excision of the entire Radius.*

In May 1864, Dr Fiddes of Kingston, Jamaica, sent to Mr Syme the right arm, forearm, and hand of a negro, which had been removed after death. The patient from whom the preparation had been taken was an adult negro, who, four years before, had the entire radius excised by Dr Fiddes, for extensive disease of that bone. The patient made a good recovery after the operation, and in a very short time he regained sufficiently useful movements of the hand and arm to enable him to resume his active occupation of a master pilot.

The cause of the patient's death was an attack of pneumonia.

Mr Syme having given me the preparation to dissect, I will here describe the changes which had taken place in the various parts concerned in the operation.

The elbow-joint was slightly movable, but could not be fully extended, owing to a contracted state of the biceps and brachialis anticus tendons. The hand was pronated and articulated with the radial aspect of the ulna, so that it formed an angle with the forearm. Fig. 1 is from a drawing of the preparation taken before the skin was reflected. It shows the general position of these parts to one another.

In the sketch the hand forms almost a right angle with the forearm; but Dr Fiddes mentions, in his account of the case, that during the patient's life the hand was much more in a line with the forearm. The wrist-joint admitted of free flexion and extension, and of some lateral movement upwards and downwards in the direction of the axis of the forearm. On removing the skin, areolar tissue, and deep fascia from the whole arm, forearm, and hand, the following were the appearances of the parts beneath:—The tendons of the biceps and brachialis anticus were contracted, the tendons of the former being inserted into a dense mass of fibrous tissue which occupied the situation of the radius. The extensor and supinator groups of muscles in the forearm were all matted together. The extensor muscles could, however, be partially separated by dissection. The tendons of the extensor muscles of the fingers were in their normal condition, and could be readily traced to their usual points of insertion. These tendons passed under a distinct annular ligament; but, owing to the altered position of the hand, they described a sort of curve on the dorsal aspect of the carpus before they reached the fingers. The extensor carpi ulnaris tendon did not pass under the annular ligament, but lay over the dorsal aspect of the ulna. The supinator longus tendon and the tendons of the extensors of the thumb could not be separated from the mass of fibrous tissue representing the radius. This mass of fibrous tissue occupied the usual position of the radius. It was elongated in shape, and composed partly of fibrous tissue and partly of certain muscles blended with it. It extended from the outer side of the coronoid process of the ulna above, and passed down along the radial side of the ulna, occupying not only the situation of the radius, but also that of the interosseous membrane, being firmly connected to the ulna as far as the first row of carpal bones, where it terminated by becoming attached to the palmar aspect of these bones and to the thickened synovial sheath of the flexor tendons. There was no reproduction of the bone; but one small osseous nodule, about the size of a large pin-head, had become developed about the centre of the fibrous substitute of the radius.

Fig. 2 is taken from a drawing of the dorsal surface of the forearm and hand.

The flexor muscles of the fingers and thumb were normal at their origins, and their tendons passed down towards the wrist, taking a curve in order to pass under the annular ligament and reach the fingers. The synovial sheath of these tendons was much thickened over the palmar aspect of the wrist-joint, and being partially twisted round by the altered position of the hand, it passed up the forearm for half an inch and formed a sort of pad or cushion for the palmar surface of the carpus to rest upon. A new wrist-joint had been formed, by means of which the hand articulated with the forearm. This joint was situated between the first row of carpal bones and the radial surface of the lower end of the ulna, which latter bone had become hollowed out for an extent of about two inches, in order to receive the carpus. The synovial membrane of the

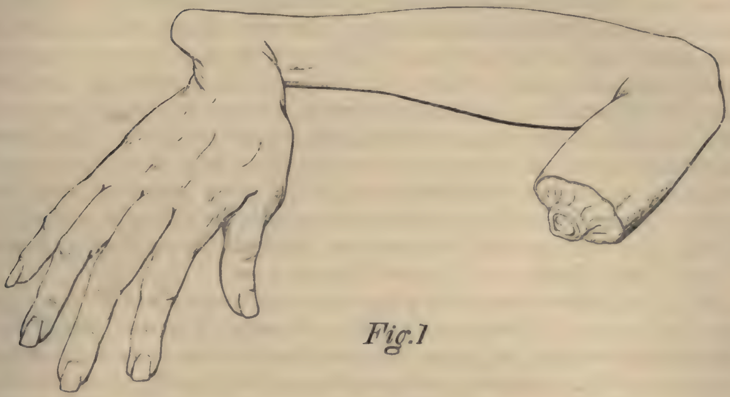


Fig. 1

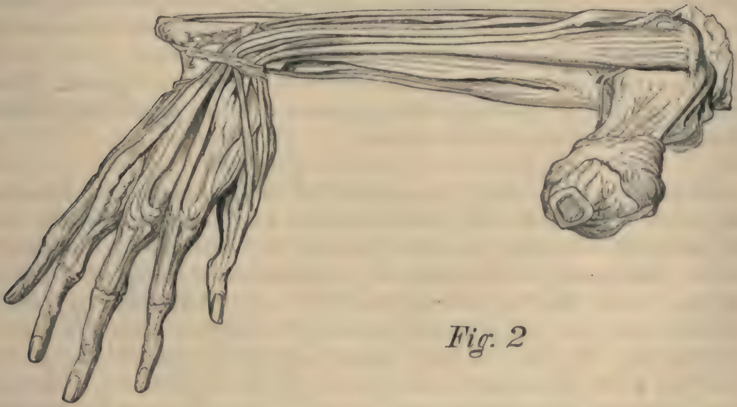
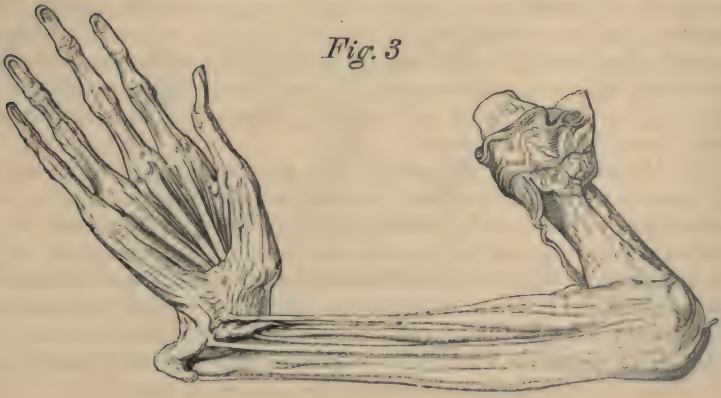


Fig. 2

Fig. 3



wrist-joint was thickened and adherent to the fibrous tissue already mentioned, and to the periosteum of the ulna above, while below it was connected to the carpus. This membrane acted as a sort of strong ligament, and assisted in keeping the surface of the new joint in apposition; but, in addition, there was a distinct external and internal lateral ligament on either side of the joint. The lower extremity of the ulna projected on the ulnar side of this joint, and on its surface was a well-developed bursa lying between it and the skin. The fibro-cartilage which lies between the articular extremity of the ulna and carpus had become displaced, atrophied, and was blended with the internal lateral ligament.

Fig. 3 is taken from a drawing of the palmar aspect of the forearm and hand. On examining the elbow-joint it was found that the ulna articulated with its usual surface on the lower end of the humerus. Both articular surfaces being quite healthy, the articular surface on the lower end of the humerus for the head of the radius retained its natural cartilaginous layer, but the capsular ligament and fibrous tissue representing the radius were firmly adherent to one another and to this articular surface.

The vessels and nerves of the limb were healthy, but were more or less altered in position owing to the change which had taken place in the direction of the hand.

Remarks.—The history and results of this case, and the appearances found on dissection, are, I think, most encouraging to the conservative surgeon. Notwithstanding the excision of the entire radius and consequent removal of the principal support of the carpus, a most efficient new articulation had been formed, by means of which the hand performed its various movements with power and usefulness. In Mr Holmes' Appendix to Wagner's Essay on "Repair after Re-section of Bones" (New Sydenham Society), there are three cases mentioned in which the whole or the greater portions of one or both bones of the forearm were excised,—the result in all these cases being a useful and movable hand. Mr Butcher also reports the case of a lad, æt. 17, whose radius was extensively diseased. Mr Butcher divided the radius immediately below the tubercle, and removed the entire shaft, the epiphysis at the lower extremity being left behind. The young man recovered, with a useful hand and arm.[1]

Mr Holmes, in his Appendix (*loc. cit.*), also refers to a case of Mr Savory's, in which almost the entire radius was removed in a necrosed condition; and I shall presently have to relate a case very similar which came under my own observation. In connexion with the appearances found in the dissection of Case 1, I would make a few remarks,—1*st*, On the formation of the new articulation at the wrist-joint; 2*d*, On the reproduction of the bone in such cases.

1*st*, The changes which have taken place in the wrist-joint resemble very much those which result from an unreduced dislocation of the head of the humerus, femur, or other bone,—the circumstances being much the same in both instances. Owing to the displacement of the carpus and hand, consequent upon the removal of the radius, the first row of carpal bones had formed—with the inner aspect of the ulna—a sort of new joint, the ulna being hollowed out to receive more readily the prominent articular surface of the carpus. The adaptation of bony surfaces to one another, so as to constitute a new articulation, after the excision of joints, bones, disorganization of joints by disease, and in some cases of ununited fracture, is one of nature's most interesting processes, and is met with not only in the form which is present in Case 1, but also in the shape of distinct processes or condyles, which are connected with other surfaces or processes, so that a new joint thus formed may come to have very much the same appearance and usefulness as the original articulation. In another paper I hope to direct attention more particularly to the formation of those new joints in which we have processes or condyles produced; but let us here consider for a moment the manner in which the hollowing out of a bone is brought about, as in the ulna in this new wrist-joint.

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[1] Operative and Conservative Surgery, p. 348.

Some years ago, when making investigations into the surgical pathology of the hip-joint, especially in connexion with those peculiar changes which take place in the head and neck of the femur as a result of chronic rheumatic arthritis, I examined a large number of joints, many of which had suffered more or less from this affection. In these observations I was struck with the appearance of the head of the femur in the earliest stages of the disease. The

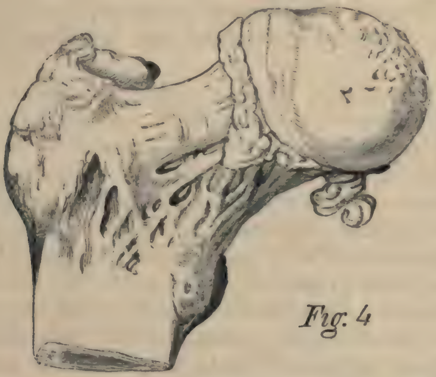

Fig. 4

head of the bone instead of being globular was more circular than usual, as if it had been subjected to a rolling-out process, the amount of the rolling-out being in many cases very marked. (See fig. 4.) On considering the causes which could produce this change, they appeared to me to be the following:—In the first place, the capsular ligament in all these cases was thickened and contracted, and limited somewhat the movements of the joint. Again, there were always nodules of osseous matter deposited round the neck of the bone close to the head. The amount and situation of these nodules differed in different instances, but in all cases their existence limited the movements of the joint,—the situation and amount of these growths determining, as it were, the movements of the articulation. The most common result of these growths or nodules was to limit the movements of the hip-joint to flexion and extension: the other movements being impossible or only slightly permitted. In this way the hip-joint became converted into a sort of hinge-joint, and in consequence the head of the femur was rolled out in the direction of its movements. In more aggravated cases the motions of the joint were still more limited or altered in their direction, the effect being that the head of the femur was rolled into those various curious shapes, pyramidal and otherwise, which are characteristic of the chronic rheumatic arthritis.[1] From our knowledge of these and other changes which take place in the articular extremities of bones, it would appear that when two or more osseous surfaces, articular or non-articular, are rubbed or moved continuously on one another, the result is that one or both of these surfaces becomes more or less rounded off or hollowed out by interstitial absorption, so as to allow such surfaces the better to adapt themselves to, or to move more freely on one another. In the case of new joints we usually find, too, that capsular and other ligaments, or very efficient substitutes for them, are formed, so as to connect the articular surfaces. Another interesting fact in connexion with Case 1 was the free and healthy condition of the tendons of the fingers. This condition must, I think, be in a great measure accounted for by the careful and judicious after-treatment of the case.

There can be no doubt, therefore, that an essential part of the treatment in all these cases where it is desirable that new articulations should be freely movable, will consist in the employment of gentle, early, and constant movements of the part, the movements being made in the direction which will be most useful for the efficiency of the limb implicated, for I think it must be admitted that if such treatment is early and carefully attended to, the surgeon possesses some power in guiding, as it were, the future motions of the new articulation. This treatment will act not only by improving the tone of the muscles and preventing their adhesion, but also by determining, to a certain extent, the direction of the osseous surfaces.

As the best proof of the efficacy of such treatment I would refer to the admirable success which has followed Mr Lister's cases of excision of the wrist-joint, for the success of these cases depends in a great measure upon a careful and persevering employment of the treatment which I have just been advocating.[1]

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[1] Prize Thesis, "On the Injuries and Diseases of the Hip-Joint." Edinburgh University, 1860.

2*d*, Before considering the question of the reproduction of the bone, I will relate an interesting case in connexion with the subject.

CASE 2.—*Removal of almost the entire Radius for Necrosis.*

On the 3d February 1863, I saw Master S., æt. 12, under the care of Mr Syme and Dr Paterson of Leith. Thirteen months before, the patient had been attacked with acute necrosis of left radius, which followed the usual course, giving rise to suppuration and the formation of numerous sinuses, which have continued to discharge pus. On examination of the arm it was found that the elbow-joint was anchylosed. There were several sinuses in the forearm, through which a large portion of dead bone could be felt lying loose. One of the sinuses was therefore enlarged, and a large exfoliation drawn out. A little new bone had been thrown out, principally around the lower end of the radius. None of the periosteum was removed at the operation. The portion of bone removed was 6½ inches in length, and consisted of almost the entire shaft of the radius; the length of the whole radius from one articular extremity to the other was 8 inches. This was ascertained by measuring the extent and position of the bone as it is usually connected with the ulna. After the operation the sinuses and wounds gradually healed, the elbow-joint remaining, however, stiff.

On the 25th of February 1865, two years after the operation, I had, through the kindness of Dr Paterson, an opportunity of examining the arm of this patient, and of getting a sketch of it.

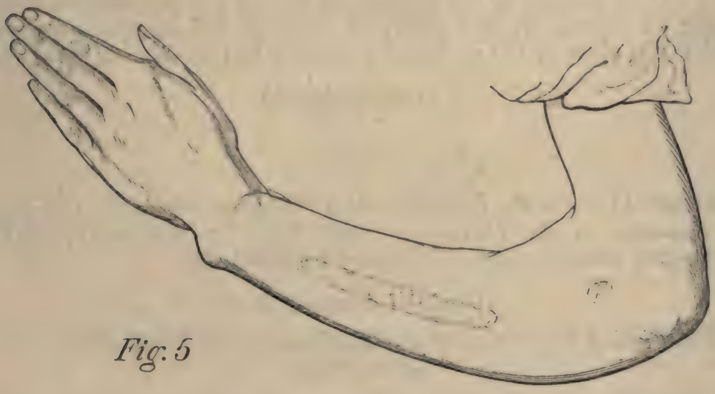

Fig: 5

Fig. 5 is taken from this sketch. The hand was drawn slightly to the radial side, and the extremity of the ulna projected considerably. The elbow was firmly anchylosed. The wrist-joint was quite movable: flexion and extension, pronation and supination, being permitted of. The radius had not been entirely reproduced. About 4 inches of new osseous material could be felt at the upper extremity, and about 2 inches at the lower end; these two portions being connected by a dense and movable fibrous cord, which measured rather more than 2 inches in length.

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[1] *Lancet*, April 1865.

Remarks.—This case is almost identical with the one which has been reported by Mr Savory, and it illustrates well the importance of leaving the periosteum uninterfered with in cases of re-section of bones where we wish new bone to be formed.

From the experiments of Professors Syme, Flourens, Ollier, Demarquay, Langenbeck, and others, and also from a knowledge of various pathological changes which take place in connexion with diseases and injuries of bone, it is now thoroughly established that the periosteum plays a most essential part in the reproduction of new bone; and, therefore, in all operations for the removal of bones or portions of them, it is of the greatest importance that this membrane should be preserved, and its connexion as little as possible interfered with. In Case 1 there was no bone reproduced; and this was probably owing to the fact that, as the disease involved the entire radius, the periosteum could not be left behind, for the dissection showed no trace of the structure. Case 2 was, however, a most favourable one for reproduction of osseous material, owing to the condition of the bone, which lay loose and separated from its periosteum, and was therefore readily removed without interfering with this membrane. The reason that the entire length of the radius was not reproduced was probably due partly to the destruction of a certain amount of the periosteum during the suppurative action going on around it, and partly to more or less contraction which had taken place in the membrane when it lost its osseous support.

(*To be continued.*)

ARTICLE II.—*On the Influence which Consanguinity in the Parentage Exercises on the Offspring.* By ARTHUR MITCHELL, M.D., Deputy Commissioner in Lunacy for Scotland.

(*Concluded from page* 913.)

The Fishing Population of a small Town on the North-East Coast of Scotland.

FOR information regarding this place I am indebted to an educated fisher-lad who resides there, and who collected the facts under the superintendence of a person intimately acquainted with the whole fishing population. I have myself had several opportunities of testing the substantial accuracy of the statements.

The fishing population is estimated at 779, and contains 119 married couples, and about 60 widows and widowers with or without families.

Of the 119 married couples, in 11 cases the union is between full cousins, and in 16 between second cousins; or, in other words, in 27 instances there is a blood-relationship. This is in the proportion of 1 to 4·4 of all marriages. Of these 27 marriages, including 3 which are barren, 105 children have been born. Of these children, 38 are dead (35 having died in childhood), 4 are deaf-mute, 4 are imbecile, 4 are slightly silly (" want a cast"), 1 is paralytic, and 11 are scrofulous and weakly. In other words, 24 out of the 67 living children labour under defects of body or mind, while 1 in 17 is an avowed imbecile, and 1 in 8·4 is weak in mind. These facts are of such a character as to lead us to suspect that more than one of the causes of idiocy must be strong in this community.

The children of those who are full cousins[1] are described as being " *all* of them neither strong in mind nor in body," and the fishers of this place, as a class, are said to be " below par in intellect." In this last opinion I am inclined to concur. It is true, I believe, not of this locality alone, but of nearly all the fishing villages which fringe the north-east coast of Scotland. There is a general lowering of the physical and mental strength in these communities, which is popularly attributed to this system of in-and-in breeding. When compared with the agricultural population, or with the tradespeople of the small towns in the neighbourhood, they are, as a race, inferior both in bodily vigour and intellectual capacity, while their thriftlessness, facility, and want of foresight are notorious. This opinion is founded on personal observation, as well as on the testimony of others. It is popularly thought that, in this respect more than in any other, the evils of consanguine marriages, continued from generation to generation, are evidenced, —the defect at length appearing to become *racical.* I must here state, however, that so far as my own observation goes, and so far as I have learned from others, there seems to be in such communities no exceptional liability to acquired insanity. Indeed, it is believed by many rather to be otherwise.

It has often struck me that the men of these villages had small heads, and so strong has this impression become of late, that I resolved, if possible, to test its accuracy by measurement. I have been fortunate in securing the assistance of a gentleman, who has obtained much curious information for me from the large hat manufacturers of Scotland. I shall not at present communicate this in detail, but shall content myself with stating such facts as may possibly bear on my inquiry.

All unite in saying that the average hat for Scotland is " 7⅛ small,"—representing a head 22¼ inches in circumference, or rather more. The average, however, for the east-coast fishing villages, from Fife to Caithness, is 6⅞ and 7, representing circumferences of 21⅝ and 22 inches,—the extreme north having slightly the advantage. The difference is better seen thus,—A merchant whose trade lay in any small town in the agricultural districts, say of Perthshire, and whose stock was exhausted, wishing to renew it by a purchase of two dozen hats, would select sizes as given below; while a merchant whose stock was in the same position, and who supplied a fishing village on the north-east coast, would make a very different selection, as is also shown below:—

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[1] There is *said to be* an aversion to cousin-marriages, but " cousins' bairns go together readily."

Small Town in Agricultural District of Perthshire.

1	of	6 6/8	size
2	...	6 7/8	...
5	...	7	...
8	...	7 1/8	...
5	...	7 2/8	...
2	...	7 3/8	...
1	...	7 4/8	...
—		—	
24;	mean,	7 1/8	...

Fishing Village on North-East Coast of Scotland.

3	of	6 5/8	size
6	...	6 6/8	...
8	...	6 7/8	...
4	...	7	...
2	...	7 1/8	...
1	...	7 2/8	...
—		—	
24;	mean,	7	...

The small towns in the agricultural districts of Aberdeenshire exhibit a still larger mean size, and the upper central district of the Stewartry of Kirkcudbright perhaps the largest in Scotland.

It appears probable from these figures that in the district referred to the fisherman's head is really a small one, but if any one desires easily to convince himself that it is so, he has only to try to fit a 7⅛ head with a "sou-wester" in the shop of a fishing village on the north-east coast. I have myself repeatedly seen this trial made.

Fishing Villages of Portmaholmack and Balnabruiach, in Easter Ross.

For the information which this locality supplies, I am indebted chiefly to Mr John Ross, inspector of poor of the parish of Tarbat.

The population of the two villages, which adjoin each other, is estimated at 1548, and the number of married couples at 355. Of the marriages there are 62 between full cousins, and 20 between second cousins or between persons more distantly related. There is a blood-relationship, therefore, in 82 cases, or, in other words, in nearly one-fourth of the whole number of marriages.

The number of children born of these 82 couples was 340, of whom 250 are alive.[1] Most of those who are dead, were cut off in infancy, and nearly all of them below the age of 10.

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[1] These numbers are not given as *absolutely accurate.* Except by an organized official inquiry such accuracy could not have been attained. The facts, however, were collected with care, and, I am assured, may be regarded as "*very correct.*"

Of the whole number of the offspring of these consanguine marriages there are, or were, 2 imbeciles, 1 idiot, and 2 cripples. These include all who showed either bodily or mental defect.

The number of children born of the other marriages, where the parents were not related by blood, was not ascertained, but at the same birth-rate, it should amount to 1160, and, the mortality in childhood being the same, there should be 852 now alive. Although the whole number of the offspring was not determined, it was found, however, that it included, 4 imbeciles, 2 idiots, 2 insane, and 2 cripples—embracing all who exhibited any form of bodily or intellectual defect.

This result shows that such calamities fall on the offspring of blood-related parents with greater frequency than on the offspring of parents not related; in the proportion of 2 to 1·2. But this by no means comes up to common expectation. When I began this inquiry my reading on the subject had prepared me to expect worse results, and repeated conversations with Mr Ross led me to suppose that I should find such anticipations fully realized in the statistics of these villages. In transmitting them, however, he remarks: "It is my full opinion still, notwithstanding the above figures, which apparently speak favourably, that such intermarriages tend to lower the physical and intellectual powers of the offspring. Yet in a locality such as Portmaholmack, where out-door exercise and ablutions in salt water are a part of the everyday business of life, I am fully convinced that the evil effects are in a great measure counteracted by these healthy occupations." He makes the foregoing remark in reply to a query asking his own opinion on the subject, "as founded on observation in these villages." That occupation, social habits, etc., can and do modify the evil results of such alliances, both in their character and degree, we have good reasons for believing.

This concludes the evidence which I have collected under the second mode of inquiry recommended.

With reference to none of the localities have I been able to obtain the full information I aimed at. Indeed, I am very conscious that I am far from having succeeded in doing what I proposed to myself. I regret this all the more that I think no method of investigation would so satisfactorily settle this vexed question as that now referred to. The result of my efforts, however, convinces me that it could not be carried out in a thoroughly satisfactory manner by any private individual. If, for several districts of the country not too limited in size, and differing from each other *quoad* the social condition of the people, we could obtain full and accurate information as to the family history of *every* marriage in which a blood-relationship existed between man and wife, and also of *every* marriage in which no such relationship existed, we should assuredly have before us a mass of facts from which we could draw conclusions of a definite character and worthy of trust; but to obtain possession of such data will never prove an easy task.

It appears to me, however, that my research, though it has fallen short of what was intended, has nevertheless resulted in the collection of some useful and interesting matter, through which a certain amount of light is thrown on the influence upon children of consanguinity in parentage. The facts which I have detailed appear to show a great unsteadiness in the character of this influence. Sometimes we find, or seem to find little or no proof that it is an evil influence. At other times this bloodship in the parentage appears to be the origin of much injury to the offspring. More frequently still the facts admit of more interpretations than one, and are not clear or satisfactory in their teaching. It is of importance, however, to know that these differences or seeming differences may occur, and to learn that it is necessary to widen the field of observation, and carefully to inquire into all those circumstances by which it is quite clear the results may be and often are exaggerated, modified, or concealed. It is evident that these results will not be the same under all circumstances,—nay, more, it becomes probable that, to some small extent at least, we have the power of controlling them.

In detailing the facts with reference to each locality, I have done my best to secure accuracy, and to be as full as was consistent with relevancy. As regards the last point I had often the difficulty of doubt, but I have omitted nothing which in my opinion could in any way bear on the question. As regards the first point it must be remembered that a great part of the information was collected for me by others, whom I regarded as competent and trustworthy. I had myself opportunities of visiting many of the localities, when I made such inquiries as I thought would test the value of the statements communicated to me; and where I had not such opportunities, I did what I could by frequent writing to ensure a substantial accuracy, which was all I could reasonably expect, and this I think I have attained.

I have made no effort to arrange or group the facts so as to support one theory or the other, but have given them disjointedly, with such occasional remarks or reflections as they suggested. Each reader can thus form his own opinion as to what they teach.

If taken *as a whole* and fairly interpreted, it appears to me that they lead to the same conclusion as that drawn from the first line of inquiry, viz., that consanguinity in parentage tends to injure the offspring.

They appear to me, however, to point to more than this general conclusion, as, for instance, that in all classes and conditions of society, and under all circumstances, the manifestations of this injury are not alike either in character or degree. It would seem that when these unions of kinship are continued through many generations (in certain classes of the community at least), the evil may show itself rather in a general deterioration of the race than in striking abnormalities and defects. Again,—the results do not appear to be very grave, when the parents live in tolerable comfort, without ambitions, anxieties, or much thought of the morrow; when they follow healthful open-air occupations, living by their muscles, but not overworked, and easily earning enough to procure good food and clothing; when they lead routine but not indolent lives, working but not struggling for existence; when they have a fair education, but are without pretence of refinement; when they steadily adhere to sobriety; when, in short, they have good constitutions and are able to manage these wisely after marriage. On the other hand, when the parents are poor, pinched for food, scrimp of clothing, badly housed, and exposed to misery; when they have to toil and struggle for the bare necessaries of life—never having enough for to-day and always fearful of to-morrow; and especially when, in addition to all this, they are intemperate in their habits, then we find serious evidences of injury, the congenital forms preponderating and bodily malformations being frequent.

It will at once be perceived that, even if consanguinity in parentage had no tendency to injure the offspring, the results I have just described might be expected to follow the conditions I have described. Where the whole surroundings and mode of life of a community are such as keep the health-point high, it is clear that *all* sources of disturbance, whatever be their nature, will be controlled in their operation; and where the reverse holds, these sources of disturbance will be favoured in their work, and intensified. Even diseases which are strongly hereditary will be transmitted to the offspring with a force and frequency which will vary according to circumstances,—according to the circumstances, indeed, of which we are now speaking. And it must be borne in mind that the injuries which result from consanguinity in parentage are very closely akin to injuries from hereditary transmission. It is believed that they are so at least in the great majority of cases,—since they generally result from this that, where there is a blood-relationship between parents, both of them are more likely to have the same disease to transmit to their children than they would be if there was no such relationship, and, as it goes down from both father and mother, it will present itself among the children more frequently and in a more severe form.

It occurs to me here to state that in forming some of the opinions expressed in this paper, I have been insensibly influenced by a host of little things which have come under observation during the inquiry, and which I could not here detail. Such an influence, I think, is legitimate, and can scarcely be resisted. It would be more correct, perhaps, to speak of it as having given strength or weakness to conclusions drawn from stateable evidence, than as having itself led to separate and independent opinions.

Before proceeding to give the general conclusions which I have drawn from the whole research, I shall briefly examine the argument from the practice of in-and-in breeding in the lower animals.

Argument from in-and-in Breeding in the Lower Animals.

One would expect to find this field of inquiry peculiarly rich in experiences tending to clear away doubts as to the effect of unions of consanguinity. In the breeding of his stock, the farmer exercises a complete control over all arrangements, and knows definitely the character of the issue. From such opportunities and advantages we might reasonably look for precise information. This will not, however, be found either in the literature of farming, or from personal inquiry of those who might be presumed to possess it. All farmers, however, are united in expressing the general opinion, that *in-and-in breeding* tends in the long run to the deterioration of the stock. But this statement receives one qualification from one, and another from a second, and so on, according to individual experiences and with special bearings.

The system of in-and-in breeding appears to have been most practised in Short-horn cattle and Leicester sheep. With regard to the latter, it is notorious that Bakewell bred only from his own stock, and, as Professor Low says, "did not scruple to connect together animals the nearest allied in blood to one another."[1] Colling, too, in perfecting his short-horns, did the same, "disregarding affinities of blood."[2] Mason of Chilton is also said to have pursued the same course. And Mr Stephens says, that "there are breeders in England at the present time who maintain that it is the best system, and will follow no other."[3] In rearing game-fowls, Mr Blaine tells us, that the intercourse of a third remove is sought and considered best;[4] and one of my correspondents, the possessor of a flock of fine old Scotch sheep, numbering about seventy, informs me that they are all related by blood, and have been so for eighty years. I am assured that some winners on the racecourse have had the same sire and grandsire, being also nephews of their own sires. The same thing I believe to be true of many of the prize-taking short-horn bulls and Leicester tups.

One is startled by such facts and statements, which are not easily reconciled to common notions, nor to the disapproval of in-and-in breeding which is generally and distinctly expressed. Youatt gives his opinion thus,—"Though some may deny it, it is the fact that strict confinement to one breed, however valuable or perfect, produces gradual deterioration;" and Sir J. Sebright, a great authority, says,—"I have no doubt by this practice being long continued, that animals would in course of time degenerate to such a degree as to become incapable of breeding at all." Professor Low tells us, that even Colling latterly "began to experience that impairment of constitution in his animals which never fails to accompany a continued intermixture of blood in a limited number of animals." In short, nearly all give a general disapproval and condemnation of the system. The precise character of the evil Mr Stephens states more definitely than other writers, —indeed, no one discusses the whole subject more fully. In his work on the Farm, he tells us,[1] that the bones become small and condensed in texture, the skin thin and open, the hair or wool short and thin set, the head and hoofs small, the ears thin and broad, the carcass reduced in size, and the eyes often affected with wateriness; that lameness is frequent; that a liability to catarrhal affections and consumption is established; that a disposition to fatten appears; and, generally, that the whole constitution is much weakened. Professor Low,[2] in speaking of sheep, says, that the system acted on for many generations tends "to render the animals more the creatures of an artificial condition, more delicate in temperament as well as form, less prolific of lambs, and less capable of supplying milk to their offspring."

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[1] Professor Low, Domestic Animals, 191.

[2] Low, op. cit., 382.

[3] Book of the Farm, Stephen, 6273.

[4] Outlines of Veterinary Art, 3d Edition, 325.

I have also seen it remarked that the offspring of such unions are placid and not easily disturbed when feeding, exhibiting a sort of imbecile indifference to what goes on around them,—a quality of great value for quick fattening on little food.

The more, indeed, that the history of Short-horn cattle and Leicester sheep is studied, the stronger does the impression become that these breeds are but the perfection of desirable imperfections—desirable, too, not to the animal itself, but to its owner.

Everything is "secondary to the property of producing in the shortest time the largest quantity of fat with the least consumption of food." The great desideratum is an early arrival at maturity, or at premature age,—an early maturity, too, of particular parts, —of muscle and fat especially.[3]

After all, then, in these cases where in-and-in breeding has been practised with so-called good results, the issue is nothing but the development of a saleable defect, which, from the animal's point of view, must be regarded as wholly unnatural and artificial, and not calculated to promote its wellbeing, enjoyment, or natural usefulness; and in this view, all difficulties disappear. By in-and-in breeding we can certainly establish an artificial type, and fix a peculiarity which is unnatural, and whose only value is its profitable convertibility into gold; but no evidence whatever exists that by such a system of breeding we can improve the natural animal.

Strictly viewed, Colling's "Comet" was neither more nor less than a perfect abnormality—a deviation from a natural animal, perfect in a desired direction. Just as this sort of perfection increases, however, the less useful does the animal become to himself, *if left to himself*, and if deprived of that artificial keeping and management which his artificial constitution demands. If it should become desirable to perpetuate any *peculiarity* in man, then in-and-in breeding may have *good* results,—the results being estimated as good or bad according as they realize, or do not realize, the end in view. I know the case of a man who has supernumerary little fingers, and whose two children and seven grandchildren have the same. Were additional little fingers of great value, the surest way to obtain a race having this peculiarity would certainly be to establish blood-alliances in this family; and when we obtained the desired excess of fingers in the offspring, we should then have the same sort of reason for saying that kinship of parentage had done good, as the farmer has for saying that it has done good when he looks on his Leicester sheep, with little heads and small bones,—ripe, fat, and ready for sale in their very lambhood.

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[1] Stephens, op. cit., 6274.

[2] Low, op. cit., 192.

[3] Low, op. cit., 192 and 388.

Till the excellencies of man are estimated in pounds or inches; till the aim be a perfect artificial and not a perfect natural man; till we want legs at the expense of arms, or arms at the expense of legs, or brain at the expense of muscle, or muscle at the expense of brain; till we want maturity in babyhood, and premature age; till the perfect man be something else than a well-balanced development of all his components, bodily and mental,—we must apply the experience of breeders of stock very cautiously in human physiology.

Conclusions.

The general conclusions to which I have been led by this investigation are briefly as follows:—

I. That consanguinity in parentage tends to injure the offspring. That this injury assumes various forms. That it may show itself in diminished viability at birth; in feeble constitutions exposing them to increased risk from the invasion of strumous disease in after-life; in bodily defects and malformations; in deprivation or impairment of the senses, especially those of hearing and sight; and, more frequently than in any other way, in errors and disturbances of the nervous system, as in epilepsy, chorea, paralysis, imbecility, idiocy, and moral and intellectual insanity. That sterility or impaired reproductiveness is another result of consanguinity in marriage, but not one of such frequent occurrence as has been thought.

II. That when the children seem to escape, the injury may show itself in the grandchildren; so that there may be given to the offspring by the kinship of their parents a potential defect which may become actual in their children, and thenceforward appear as an hereditary disease.

III. That many isolated cases, and even groups of cases, present themselves in which no injurious result can be detected. That this may occur even when all other circumstances are of an unfavourable character.

IV. That, as regards mental disease, unions between blood relations influence idiocy and imbecility more than they do the acquired forms of insanity, or those which show themselves after childhood.

V. That the amount of idiocy in Scotland is to some extent increased by the prevalence of consanguine marriages, but that the frequency of these marriages does not appear to be nearly so great as has been generally supposed.

I have already indicated my views as to the way in which consanguinity in parentage tends to injure the offspring. It matters, however, comparatively little whether these views are correct or incorrect. The chief thing is to show that the injury exists, and that therefore consanguineous marriages should be avoided. This is what may influence the wellbeing of society, and it is the point of importance in the inquiry. In adducing the evidence which seems to me to prove the existence of this injury, I have been careful not to do anything which would lead to an over-estimate of its amount—an error which has been frequently committed. I think, however, that I have been able to show that the injury is not trifling, but that it is of such value as to merit serious consideration and regulate conduct.

But though speculation as to the *modus operandi* may properly be regarded as a secondary matter, it is not altogether profitless; and therefore, before concluding my paper, I shall somewhat amplify the views on this subject which I have already had occasion to indicate.

Let us suppose that man is represented by a series of qualities which either fall upon the straight line of physiological perfection, or which have a greater or less divergence from it on either side, above or below.

Let us also suppose that it may be accepted as true in a general sense that parents live again in their offspring. Though the child is not the exact algebraic sum of his parents, yet both are constantly expressed in him, and the *plus* qualities of the one, or those above the physiological rule, are either increased by the corresponding qualities of the other parent, if their divergence be in the same direction, or they are diminished by them, if the direction of divergence be different. In the first case there is an increased departure from the normal line, *plus* being added to *plus*. In the last case, on the contrary, there is an actual approach to the line, *minus* neutralizing *plus*.

Let us now take some known transmissible peculiarity—a temperament, for instance, than which perhaps nothing more certainly passes from parent to child. This peculiarity involves both *plus* and *minus* qualities.

And let it be admitted that there are few family traits so constant as temperament,—that is, that among persons closely related to each other the same temperament is almost sure to appear with frequency. In a blood-alliance, therefore, the chances are much greater that a husband and wife will have the same temperament than in an alliance without kinship.

Let us suppose such an alliance, and that man and wife do both exhibit the same and a well-marked temperament. The offspring is thus liable to receive it from both sides.

It is of course *possible* that he may not receive this temperament at all but one very much opposed to it, just as he may not inherit a predisposition to insanity though both his parents have been insane, but it is certain that the risk is great that he will do so.

Keeping to temperament as the transmissible peculiarity which we use in illustration of the subject, and supposing the child to receive from both parents the same temperament, then it is highly probable that it will appear in him in a stronger form, and the divergence from the perfect state will be augmented on both sides —the *plus* strengthening the *plus* qualities, and the *minus* the *minus*. The balance of the development of the constituent parts is thus seriously disturbed, and high disproportions arise, which pass the limits of a temperament and become acknowledged or recognised defect.

It is clear that the chances of such a result would have been lessened if one parent had come of a different stock and that the more extensively consanguineous marriages prevail, the greater is the risk of finding in the community all sorts of transmissible peculiarities dangerously intensified. Such unions also tend certainly and in a similar way to increase the power of the hereditary transmission of disease, and they practically augment the risks of evil from that source.

It very frequently happens, in investigating the history of a cousin-marriage, that it is found that the defect which appears in the children had appeared also among the progenitors of the children. Such a case we would naturally call a simple one of hereditary transmission, assigning no part to the consanguinity. A large number of cases of this kind have come under my observation. But when they have been minutely inquired into; when the history of other marriages, not between cousins, in the same family, has been examined and contrasted with those which were between cousins; and when the injury appearing among the children has been compared, both as to extent and severity, with that which had appeared among their ascendants, no doubt has been left on the mind that the consanguinity had given force to the heredity, *both* parents transmitting the same misfortune to their offspring.

What has been held to be true of a mere temperament would of course be true also of any other transmissible peculiarity, defect, or disease. Take deaf-mutism, for instance, and with reference to it we happen to have some interesting facts. It has been ascertained that if a deaf-dumb person is married to one who hears, the chances of their having a deaf-mute child will be 1 to 135; but if deaf-mute persons intermarry, the chances rise to 1 to 20.[1] This is a remarkable fact, and well illustrates my argument. A similar (though perhaps not the same) rise in the rate of production of deaf-mute children would follow the marriage of deaf-mute persons with their cousins, since it is perfectly well known that the hearing members of a family in which deaf-mutism occurs, have often themselves such a possession of the defect as to make it potential in reproduction. A deaf-mute, therefore, in marrying his cousin, would do, more or less nearly, the same thing as if he had married another deaf-mute, so far as concerns his children. He would do this even if his cousin heard; but if he chooses a wife who is both his cousin and also like himself a deaf-mute, then the chances of the defect occurring among his children become nearly as great as he can make them.

ARTICLE III.—*The Actions of the Bromide of Potassium upon the Nervous System.* By J. CRICHTON BROWNE, M.D. Edin., M.R.C.S.E., Medical Superintendent of the Newcastle-on-Tyne Borough Lunatic Asylum, late Assistant Physician, Warwick County Lunatic Asylum.

THE actions of the bromide of potassium upon the nervous system have of late attracted considerable attention. Observations have, from time to time, been made as to its influence over certain nervous functions in health and disease; but notwithstanding that important powers have been attributed to it, it has continued to be rather an object of curiosity than an extensively employed therapeutic agent. During the last twelve months its properties have been subjected to experiment in the Warwick County Lunatic Asylum. Under the direction of Dr Parsey, it has been administered to forty-three patients suffering from various affections of the nervous system or from general diseases complicated by nervous symptoms. While watching its effects upon these patients, and comparing them with the recorded experiences of others, I have not only become convinced of its high value in that department of practice in which I am more immediately engaged, but have constructed a theory explanatory of the several actions which I have found it to possess. I should perhaps rather say that these actions themselves have suggested their own explanation, as no ingenuity or insight was required to perceive their mutual relations and the central point to which they converge. My observations speedily conducted me to a generalization which has been confirmed by my subsequent researches. I have come to the conclusion that the bromide of potassium acts directly as a sedative to the medulla oblongata, soothing and moderating its functions wherever these are in excess of the normal standard. This sedative influence I believe to embrace alike the impressibility and motor excitability of the organ, and it must therefore be obvious that the importance of such a power can scarcely be over-estimated. In an attempt to establish my views, I shall here briefly describe the different actions of the remedy which I have encountered, at least in so far as the nervous system is concerned, illustrating where illustration seems necessary, but avoiding superfluous detail; adopting this method as preferable to a mere tedious enumeration of cases. As whatever I have to say regarding the remedy will be more appropriately introduced under the different heads of the arrangement, I need here only remark, as to the mode of administration, that it has been almost universally given in simple solution in water. The doses have varied from 10 to 40 grains, given twice or three times a-day, after meals or upon an empty stomach. For the attainment of the effects however here discussed, decided doses are generally necessary. The most useful results have accrued under doses of 20 and 25 grains twice a-day. No apprehension need be felt upon ordering quantities apparently so large, when viewed by the light of previously received notions. The lowering effects of the drug upon the constitution are slowly but not insidiously manifested. They are easily recognised, and are at once checked by abandoning the use of the medicine.

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[1] Burton, Causes of Deaf-dumbness, p. 13.

Proceeding, then, to consider the actions of the bromide of potassium, I may first remark that,—

I. It mitigates those convulsive movements or spasmodic twitchings which are the result of the rapid conversion of sensory impressions into motor impulses, or of morbid reflex action through the medulla oblongata, and it exercises a peculiar influence over the phenomena which are characteristic of epilepsy. Recent inquiries as to the structure and functions of the medulla oblongata have proved it to be that common central point where reflex action crosses to either side, and have distinguished it as possessing special capabilities in the excitement of bilateral reflex activity. It is unusually rich, alike in transverse fibres and commissures, and in groups of ganglionic cells endowed with the property of transforming into muscular action any stimulus that may be applied to it. It is thus the great reflex centre which distributes the influences conveyed to it, in the first place to those afferent nerves which are more especially its own—the facial, the hypoglossal, the accessory, and portio-minor trigemini,—and then to those which arise from the lateral columns of the spinal cord, and which, when disordered, manifests its disturbance by an undue proneness to be affected by trifling causes, and by an unusual discharge of outgoing force, productive of involuntary movements; in short, by exaggerated functional activity, resulting from the augmented interstitial metamorphosis of the organ.

The two kinds of action of the bromide of potassium which are distinguished in this section,—its action upon exalted reflex activity and upon epilepsy,—are coupled together, because the first may be not improperly included under the second. Involuntary muscular contractions of a clonic description, the final indications of the increased reflex excitability of the medulla oblongata, when they have sudden loss of consciousness associated with them, amount to epileptic convulsions. The two conditions differ more in degree than in kind. The irritation to which both of them are due may be more intense in the latter than in the former, or may have a wider distribution and more extended dominion; but however this may be, the initial change to which they must both be traced is demonstrated by physiological, clinical, and pathological observations, and by direct experiment, to be resident in the medulla oblongata and upper part of the spinal axis. Purely dynamical in its earliest stages, it eventuates at length in enlargement of the capillaries, disorganization of their walls, and fatty degeneration of the nervous tissues.

Now whether the increased excitability of the medulla oblongata is so great as to be productive of epilepsy, or so slight as to expend itself in minor spasmodic complaints, the bromide of potassium seems to exert an excellent effect upon it. Among the numerous conditions comprehended under the latter class, I have as yet only been able to try it in clonic convulsive movements, connected with hysteria and irritations communicated from the uterine and ovarian nerves; but so satisfactory have been the results attained in these cases, that I shall resort to it with great confidence when opportunity permits, in the twitchings or strabismus of intestinal irritation, of teething, or of anæmia. I have notes of four cases of hysterical spasm in which it was eminently useful,—in two in relieving œsophageal spasm long continued and severe; in a third, in arresting violent oscillatory movements of the eyelids; and in a fourth, in dispelling twitchings of the facial muscles, almost choreic in character, but accompanied by globus. In all these cases irritation of the uterine system was indicated by menstrual irregularity,—in two by leucorrhœa, and in two by menorrhagia; and in all, the propagation of this to the nervous centres was made evident by the preponderating powers of reflex excitability throughout the organism. In all were the effects of the bromide speedy, striking, and uniform, so much so, that after full allowance was made for the incalculable changeability and frequent transitions of this protean malady, and for the influence of that invigoration of the will and the moral sentiments which any new curative effort is so likely to produce, there was still no reasonable ground for doubting that it directly and remarkably palliated the spasmodic symptoms. That it compassed this result by depressing and blunting reflex irritability was rendered probable by the increased tolerance of loud sounds and other external stimuli which accompanied the cessation of the spasms and twitchings. A dose of 30 grains not only gave relief from the morbid muscular movements, but increased the capability of resisting outward irritations and suggestions of the mind. A continued course of the drug kept the tremors, agitations, and spasms altogether in abeyance in two cases, and greatly reduced their severity in two others, and I am therefore inclined to impute to it both an immediate and permanent effect in the treatment of such affections. About the power of the bromide of potassium over epilepsy there can be little dispute. In 1857, Sir Charles Locock expressed his belief that it was of great value in certain cases. He stated that fourteen months previously he had prescribed it in a case of epilepsy connected with sexual excitement after all other medicines had failed, and that the result was an entire cessation of the fits, after they had lasted for nine years. He added, that he had since tried the bromide of potassium in fourteen or fifteen similar cases, and that it had only failed in one.[1] To this remarkable testimony, Dr Sieveking adds, that although he had not enjoyed the same amount of success, he had still found it "decidedly beneficial;"[2] and upon the same subject, Dr Chambers says, that in his experience this remedy "certainly seems to have controlled the disease in an extraordinary way."[3] During last year, the results of some interesting observations upon this action of the remedy, made in the Northampton Lunatic Asylum, were published by Dr Duckworth Williams. This gentleman administered it in thirty-seven cases, and in the history of these he contrasts two periods of five months each, one before and one during the administration. He found that the aggregate number of fits to be distributed amongst the cases was diminished during the latter period by 306 for the males, and 157 for the females. In five months before the administration was begun, 19 males suffered 1012 fits, and 18 females 1127. In five months during the administration the same 19 males suffered 706, and the same 18 females 970. There were only 5 males and 6 females who did not derive palpable advantage from the treatment, whereas many of those who benefited by it did so to a great extent. In E. H. the fits were reduced from 29 to 9, in M. L. from 20 to 5, in J. J. from 246 to 91, and in M. L. from 57 to 8. It is only necessary to add to Dr Williams' statistics, that the period of administration included the spring months, when, according to Dr Moreau and popular belief, the percentage of fits is higher than at other seasons.

[1] Lancet, May 20, 1857, vol. iv. p. 528.

[2] On Epilepsy and Epileptiform Seizures, by Edward Henry Sieveking, M.D., etc., 1861, p. 292.

[3] Lectures, chiefly Clinical, by T. King Chambers, M.D., etc., 1864, p. 371.

My own experience of the effects of the bromide of potassium in epilepsy has been much more limited, but has still been sufficient to satisfy me that this agent has not been too highly extolled by those who have recommended it. I shall only notice one case where it was used with great success.

J. D., æt. 16, of strumous habit, has been an inmate of the Asylum for two years. Was a healthy child until 1861, when she accidentally fell into a stream and was much terrified The fits first occurred within a few weeks of this fright. Since their first appearance they have gradually increased in number and severity. They have been generally ushered in by a scream. The convulsions have affected both sides of the body, but the tongue has not been bitten. After eighteen months the mental powers began to give way. No evolution of the faculties took place, but she fell into a childishness of manner and expression unlike her years. At the same time delusions took possession of her; she maintained that some time previous to the fit "a little man" got into her head and gave her much annoyance, and that just before the fit he got on to her chest. He frequently also got into her head during the night, and prevented her from sleeping. The symptoms were as described on the 26th of July last, when she was ordered the bromide of potassium 20 grains twice a-day. The medicine was continued for two months, and during that time a great amelioration of her state took place. This was marked not so much by a diminution in the number of the fits, which stood 36 from 26th July to 26th September, against 40 from 26th May to 26th July, but in a complete change of their character. From being severe and long continued, they became short and slight; they became so trifling that the patient did not require to be removed from the chapel or the recreation-hall on their occurrence. They were characterized merely by momentary pallor, and by startings and twitchings of the arms, and were not followed by the drowsiness or stupidity which formerly succeeded them. At the same time the little man left her head and chest, and no longer interfered with her rest. She likewise improved in personal appearance. When the medicine was omitted after the end of September, she relapsed, and the fits resumed their former type.

It is not in cases of this description, however, nor in any such cases as are met with in the wards of our lunatic asylums, that we are entitled to look for permanent benefit from the use of the bromide of potassium. In them the repeated congestions of the head, the distention of the bloodvessels, and pressure upon the cortical substance, have induced centric changes enough in themselves to operate as exciting causes of the disease. The medulla oblongata, too, is perhaps degenerated, or has at any rate become habituated to the morbid action. On the contrary, it is in the very earliest stages of the disease, when it is only dynamical and temporary, when the nutrition changes are only too rapidly performed, when there is no persistent alteration of texture, that real advantage may be anticipated. Then the sedative effects which I believe it to possess upon the medulla oblongata act forcibly in checking the development of the phenomena and the establishment of the pathological condition.

Previous observers have not, so far as I know, explained the action of the bromide of potassium in epilepsy in this manner. Some seem to have thought it dependent upon an influence which this drug is supposed to possess over the sexual organs. They seem to have considered it operative through the generative focus, and this theory I believe to have done harm, by limiting the employment of the drug in the hands of many to cases where sexual symptoms were present. That it is incorrect is shown by the fact that the bromide of potassium has been found equally beneficial where no sexual complications are present. In Dr Williams' cases it did more good to the males than the females, and in them the sexual system is surely less dominating. Another explanation of the action of the remedy is offered by Dr Chambers. He holds that it has a peculiar restorative action over the white fibrous tissues. But an action of this sort can be but slowly developed, and it will not therefore account for the immediate operation of the bromide which I have remarked in its alleviation of hysterical spasm, however true it may be when applied to fibroid tumours of the uterus.

The manner in which the bromide of potassium exerts its influence is, I think, very significant of its *modus operandi*. It does not merely postpone the discharge of nervous energy, allowing of its internal accumulation and subsequent explosion with aggravated violence, but it arrests its generation. Where postponement is merely accomplished, it is clear that it is a remote or sympathetic irritation that is reduced or removed. The attacks, indeed, are rarer, because their exciting cause is less vehemently operative, but the habitual tendency of the ganglionic cells to increased activity remains the same. The overflow of morbid activity occurs seldomer, but it is sharper and more afflictive. Thus the flowers of zinc were found to have excellent effects in many cases for a time. The fits became fewer and milder. Some patients appeared to recover entirely. But a time invariably arrived when a formidable penalty was paid for this complete or comparative immunity. The convulsive paroxysms returned with unwonted force, and often endangered life. No such risks, however, have been known to wait upon the bromide of potassium. When its use is omitted where it has been beneficial, the fits return in a quiet and gradual way; and even when they resume their former frequency during its administration, the resumption is slowly and safely accomplished. This I believe points to a directly sedative effect upon the medulla oblongata, and not to any mere moderation of the exalted sensibility of the sympathetic nerve or of certain of the viscera.

II. It has a sedative effect upon the action of the heart in certain cases. The heart is obviously placed in connexion with the medulla oblongata by the fibres which it derives from the sympathetic and from the pneumogastric nerves. Their functional relations are demonstrated by the phenomena of shock, of compression or injury of the brain, of joy, grief, and mental emotions, of dyspepsia and other bodily diseases. Weber's experiments tended to prove that the stimuli conveyed by the pneumogastric retard its action, and that those conveyed by the sympathetic accelerate it; but Lister has shown that while "gentle irritation of the vagus increases the heart's action,"[1] a stronger application has the opposite effect. Whatever may be the influence of the degree of the irritation, I feel persuaded that this condition of the medulla oblongata does occasionally increase the force and frequency of the cardiac contractions. The palpitations of hysteria, the primary effects of the inhalation of chloroform, and the unnatural frequency and fulness of the interparoxysmal pulse in epilepsy, may be referred to as illustrations of this. It is to its power over this irritation, situated in the medulla oblongata, that I have attributed the calmative effect which I have undoubtedly seen the bromide of potassium exercise upon an excited heart. In this observation I am supported by Dr Duckworth Williams, who noticed that in seven cases, "after using it for a few days, the action of the heart became slow and fluttering." I have myself recorded an appreciable influence upon the heart in eight cases, to some of which I may briefly refer.

W. G., subject to frequently recurring paroxysms of epilepsy, always preceded and followed by acceleration of the pulse. In the February paroxysm, the day after the fits, the pulse was 120, full and bounding. Digitalis was ordered, and on the next day the pulse was still 110; on the fourth day it had reached its ordinary standard, about 80. On the day after the March paroxysm, which was of equal severity with that of February, the pulse was 112, full and bounding. Bromide of potassium was ordered. The next day the pulse was 93; the fourth day it had fallen to 72, and was somewhat feeble.

W. B., a chronic epileptic, whose pulse, after a series of observations was found to average 86, was ordered the bromide of potassium. When he had taken this for a week, another series of observations gave an average of 76.

M. A. was always healthy until four years ago, when she sustained a severe shock, her life being attempted by her husband when in a state of intoxication, she being at the time advanced in pregnancy. She experienced intense terror, and ultimately fainted. Since then she has been an invalid, nervous and irritable, liable to palpitation, breathless on slight exertion, and apt to be thrown into a state of feverish excitement on being startled or disturbed by any noise. There is a distinct bruit with the second sound, loudest at the base. M. A. was accommodated in the quietest ward, but was of course even there sometimes exposed to loud sounds. These invariably brought on the nervous attacks alluded to, in which the pulse always rose to 90 or 100 from 78 its average number, the respirations at the same time amounting to 28 or 33. The attacks, when left to themselves, would continue for five or six hours; when treated with 20-grain doses of the bromide of potassium, they were cut short in an hour or an hour and a half. Again and again have I traced the progress of attacks left to nature, and of others thus treated; and again and again have I noted the curtailment of the latter, the rapid descent of the pulse to between 70 and 80 from its abnormal exaltation after the administration of the medicine, with a corresponding diminution of the number of the respirations. These generally fell from 30 to 21. The patient herself was keenly alive to the soothing secured by the physic, and often bore spontaneous testimony to its power in calming her perturbations and her laboured breathing, and in dissipating those bewildering terrors, those dark distorted dreams which so often break in upon the rest of persons labouring under organic disease of the heart.

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[1] Proceedings of the Royal Society of London, vol. ix. p. 374.

It will be seen that in this instructive case there was a sedative influence exerted over the respiratory process, as well as over the cardiac action. In two other cases in which the circulation was lowered, I have remarked a consentaneous reduction in the respiration; and in one case of chronic asthma, in which I have prescribed the bromide of potassium, I have seen it give considerable relief. There can be no question that if this remedy is really a sedative to the medulla oblongata, it will have important relations to that great reflex respiratory act which is dependent upon it; but upon this branch of the subject my observations are not yet sufficiently matured.

III. It lessens and mitigates that rapid and preternatural excitement of spasm, tremor, and other outward manifestations which in some forms of nervous disease follow upon any emotional or moral disturbance. It may be stated as a fundamental condition of emotion, that every state of feeling or of subjective consciousness which it is possible to experience is, through its operations upon the cerebral hemispheres, the primary step in a kind of reflex act. The diffusive action which Professor Bain has so lucidly described as an accompaniment of emotion consists simply in the propagation of "an agitation," beginning in the brain, to various organs united to it by nervous communication. The passions and sentiments of the mind, through and by means of their reflected effects upon the muscles, are at all times expressed to the external world. Whether they are of an elating or depressing order, they find in this system first and then in the secreting and excreting apparatus the channels by which they may make themselves known. An appropriate play of feature, of attitude, and of voice, we expect to see associated with every thought; and nothing is more marked under ordinary circumstances than the delicate agreement which subsists between states of pleasure and pain and their concomitant bodily variations. The intensity of an inward stimulus we are able to calculate by observing the energy of contemporaneous outward movements, while the regularity with which these rise and fall together leads us irresistibly to the conclusion that they are connected by a link of causation. It is only when this equilibrium is deranged, when the expression is disproportioned to the emotion, when some slight vexation produces the gesticulations which might be expected to denote profound calamity, when some trifling apprehension occasions the startings or paralysis of terror or the shudderings of horror, that morbidity is really indicated. Now among the insane it is common to find persons prone to an exaggerated expression of the emotions. That vivacity and animation which constitute one of the chief charms of a nervous temperament are often displayed in a lunatic asylum in grotesque and overgrown proportions. Carefully distinguishing and excluding from consideration those lunatics in whom the violent workings of the frame are only adequate exponents of the impetuous feelings of the mind, in whom, in fact, a ratio is maintained between the expression and that which is to be expressed, there still remain many in whom there is a strong diffusive excitement without an equally strong emotion, or in whom the will is impotent in placing any restraint upon the responses made by the muscles in gestures, cries or traits of countenance, to the calls of inward emotion. And wherever we discern this condition, we may infer that there is disorder of the medulla oblongata. The researches of Schroeder Van der Kolk have made it evident that to the corpora olivaria must be traced those complicated actions which we are discussing. Intimately united as these bodies are with the nuclei of the hypoglossus, of the accessory and of the facial nerves, and also to each other by transverse fasiculi, they present to us that point where alone the varied bilateral manifestations of emotion could originate. It is impossible here even to epitomize the arguments derived from physiology, comparative anatomy, and pathology, by which Van der Kolk establishes his conclusions. It is enough, however, to state, that he has succeeded in referring the expression of the passions to the small multipolar ganglionic cells which are characteristic of the olivary bodies. The actions thus referred are really reflex, and not direct nor voluntary. That they are so was proved by a case related by Romberg, where, though there was total loss of voluntary power, the patient was still capable of laughing. And even where the same results are attained after a voluntary effort, it is probable that they are still reflex, and that they are preceded by a forced excitation of the feelings which they represent. Like other reflex acts, however, they are to a certain extent under the authority of the will, which can suppress or direct their development, and which is supposed to exert its control over them in the superior portions of the corpora olivaria. Like other reflex acts, also, they may be emancipated from this allegiance and changed in nature, by disordered states of the structures upon which they are dependent. When this is the case, when the multipolar ganglionic cells of the olivary bodies are irritated, those changes in the brain with which the emotions are correlative are transmitted as usual to the medulla oblongata, whence, with exaggerated facility and force, they are reflected as motor impulses upon the muscles of physiognomic expression, the heart, the respiratory muscles, the muscles of the body generally, and upon the glands. In patients thus affected, liable to unreasonable outbursts of passionate action, and effusions of sentiment, I have several times seen the calmative effects of the bromide of potassium. It has seemed to place a check upon the passage of mental into motorial activity, or in some way to assist volition, in softening down any disparity between the outer life and the inner existence. In one young woman (H. L.), labouring under peculiar sexual delusions, and believing that her body was permeated by the poison of syphilis, there was at the time of commencing the use of the drug that proneness to inexplicable agitation which has just been sketched. She took unaccountable antipathies to certain persons around her, and deported herself in their presence in such a manner as could only be supposed to indicate deep-rooted and murderous aversion. Her face was distorted by anger, now flushed, now pale, her breathing hurried and irregular, her heart's action was tumultuous, her frame trembled, her articulation was impeded; yet in her calmer moments she explained that her enmity towards them was by no means so deadly as her conduct would lead one to believe. She disliked them without any assignable cause. When she thought about them she felt restless and uncomfortable, and when she saw them she lost all self-command. The existence of excitement of the medulla oblongata was shown by the occurrence of violent twitchings of the muscles of the face and upper extremities on the application of a sinapism to the chest and throat. After she had taken the bromide for some days, there was a remission in the severity of her symptoms. She felt better able to bear the presence of the obnoxious persons without exhibitions of fury, her deportment became more tranquil and reserved, her speech more cool and collected. She begged for a continuance of the medicine when its omission was spoken of, declaring that it kept her temper within bounds, and that she should "certainly fly off" if it were withheld. In another case of impulsive insanity (E. B.), I have seen singular benefit accrue from the same remedy. The patient, who had a hereditary tendency to insanity, had been originally of good and amiable disposition, had been seduced, had led a depraved life, had been committed to prison for breaking windows, and had ultimately found her way into an asylum. She was liable at intervals to paroxysms of impulsive madness, in which she smashed, and screamed, and sobbed, and rushed frantically about without provocation or feasible object. Between these, however, she was intelligent, tractable, and good-tempered, and even during their continuance she was sometimes in a penitential frame of mind strangely in contrast with her outward demeanour. After assaulting a nurse, breaking the furniture, ripping up her clothing, and tearing out her hair, she has been known to express her poignant contrition while weeping bitterly, and to deplore these ebullitions and her inability to prevent them. One of these paroxysms was certainly cut short by the bromide of potassium, and another was apparently averted by it. She herself attached a value to it which she had never allowed to any other medicine. She described it as producing a feeling of serenity and repose; and it is worthy of notice that a longer period of uninterrupted quietude has elapsed since the attack which was treated by it than has yet separated any two attacks during her residence in the asylum.

It may perhaps be said that in cases like these the soothing influence of the bromide of potassium was exerted on the cerebrum itself. Even allowing this to be the case, the action might still be traceable to the medulla oblongata, and its power over the capillary circulation. I am inclined to think, however, that it is more directly involved; for in the case of H. L. the muscular movements were suspended when the emotive condition remained unchanged, and I therefore ascribed the power of the remedy over them to its sedative effect on the medulla oblongata.

IV. It acts as an anodyne, under certain circumstances relieving hyperæsthetical sensations. That sensation has its seat in the medulla oblongata is a proposition which, according to Van der Kolk, scarcely admits of doubt. The passage downwards of the trigeminus and upwards of the nerves of sensation of the cord to that organ, the insensibility of the brain itself, and the facts that in fishes the sensitive nerves here terminate, and that there is no higher place to which sensation can be referred, and that in the fœtus development begins with the corpora quadrigemina and medulla oblongata, all seem to confirm this view, and to fix in this centre the seat, at least, of the corporeal feelings of pleasure and pain. If this be so, there can be no difficulty in comprehending how irritation here should produce an impression of suffering in parts connected with it by afferent nerves, or how pain, dependent upon eccentric causes, should be appreciated just in proportion to the acuteness and sensibility of this centre.

That the bromide of potassium does possess anodyne properties I feel assured, though its exercise of them has in my experience been arbitrary and uncertain. Remembering the innumerable sources, however, from which pain may arise, I am not surprised at this result. I only conclude that its power in this respect is limited, and that I have not yet been able to trace the boundaries which circumscribe it. Dr Garrod has pointed out that it produces a sensation of dryness in the throat and neighbouring parts, together with an anæsthetic condition of the larynx and pharynx, which has led to its being usefully employed in operations upon these parts. But while recognising this property, and regarding it as highly significant, I venture to attribute to the bromide of potassium more extensive anæsthetic powers. In two cases of neuralgia in which I have tried it, it has been efficacious; in another it has totally failed. A medical friend whom I induced to take it for this complaint, writes to me that he swallowed 15 grains twice a-day punctually and faithfully for a week, and observed no effects whatever. One of the successful cases of neuralgia which came under my own care was that of T. H., a tall well-grown man, suffering from hemicrania. After feeling out of health for a few days, he was suddenly attacked by acute pain, which he compared to toothache, in the right temporal region. It soon spread over the whole of that side of the head. It was increased by movements of the head, and along with it there was a sense of weight, and wide dilatation of the pupils of both eyes. There was constant pain, but an exacerbation regularly took place every afternoon about four o'clock. On the third day of the pain, 35 grains of the bromide of potassium was given at 3 P.M. The exacerbation recurred as usual, but the pain was much less acute than on the previous days, and the night was passed in greater comfort. In subsequent attacks the bromide was employed, and always with unmistakable alleviation. Recovery took place under a course of tonics.

Another case in which the bromide of potassium was useful as an anæsthetic was that of S. B., suffering from rheumatic fever. The pains in the joints were metastatic and were worse at night. There was a loud systolic bellows murmur and great uneasiness at the heart. The patient was wrapped up in flannel, was ordered 20 grains of the bicarbonate of potash every two hours, and poultices to the precordial region. For the first two nights of the fever, suffered great pain and got no sleep. On the third night, had 30 grains of the bromide of potassium. Still got no sleep, but felt less pain. On the seven following nights the draught was repeated, and although the amount of sleep obtained was very variable, the pain was notably diminished, and was sometimes scarcely felt at all; it was now worse by day than by night. On the second day of the administration of the bromide the pulse fell from 120 to 105; on the seventh day to 72. In two cases of chronic rheumatism in which I have pursued the same treatment, I have also had to record very decided alleviation of suffering.

I am far from founding any general statement upon such a narrow experience as mine has yet been regarding the anæsthetic power of the bromide of potassium. I am impressed with the idea, however, that this power which I have found it to possess is due to a sedative action on the medulla oblongata.

V. It promotes sleep. The ordinary phenomena and peculiarities of sleep alike require that we should look to the medulla oblongata for an interpretation of its origin and nature. Lowered vitality in the organs of sense and in the motor nerves, and impeded spontaneity in the mental processes, its most prominent features, at once direct attention to that great junction of psychical and vital forces; and a careful study of the mode of its incursion leaves but little doubt that it is due to such a condition of that organ that "the access of affinitive impressions is arrested."[1] This theory, which was first proposed by Professor Laycock, is borne out and confirmed by the sense of fatigue which precedes sleep, by the yawning, the drooping of the eyelids, the diminished tension of the tympanum, and the loss of reflex co-ordinating power which usher it in, and by the suspended communion of the mind with the external world, and the reduced respiration which accompany it. The action of chloroform, of a voluntary yielding to reveries, of the occlusion of light, and of a montonous sound in the induction of sleep, and of all stimulating impressions in dissipating it, are also corroborative of this opinion. When the transmission of sensory impressions is interrupted, the higher nervous centres being no longer incited to activity by external images, gradually sink into repose. Accepting this view, we have no difficulty in understanding how wakefulness should be so common a symptom of bodily disease. A change in any bodily organ having a nervous connexion with the medulla oblongata, or a change in the blood such as to have a poisonous influence upon it, would, by creating irritation, keep up the flow of affinitive impressions which would normally be arrested there. And any agent which would act as a sedative on the part thus irritated and restore it to its ordinary sensibility, would in this way indirectly promote sleep. It is in this way that I account for the undoubted power of the bromide of potassium in the production of drowsiness and relief of insomnia. This action of the drug seems to have been discovered by Dr Brown-Séquard. It has been announced by Dr Garrod, and further set forth by Mr Henry Behrend; and the statements of these gentlemen have been amply verified by my own experience. In twelve cases in which it has been employed in this asylum, with the view of combating partial or complete wakefulness, it has been successful in eight, and has totally failed in four instances; while in ten other cases in which it has been used for a different immediate purpose, improved sleep has been also recorded. Of the four cases in which it proved a failure, the first was a man in whom there was every reason to suspect the existence of an intercranial tumour; the second was a woman in whom the bromide was substituted for morphia, which had been taken as a night-draught for months; the third was a woman suffering from hepatic disease and nervous debility, to whom opium failed to give better rest; and the fourth was a man in one of the congestive paroxysms of general paralysis. In the eighteen cases in which sleep was improved, the causes of the pervigilium were very various, including general paralysis, mania (acute, chronic, and puerperal), melancholia, epilepsy, nervous debility and excitement, pulmonary tuberculosis, valvular disease, rheumatism, and dyspepsia. It was for the insomnia of nervous irritability, springing out of mental exertion, anxiety, or distress, that the bromide was first recommended, and it is certainly a very precious instrument—under such circumstances. Where the mind is harassed by the oppressive cares of business, by the vicissitudes of fortune, by real or imaginary sorrows, a febrile and restless condition is apt to supervene, in which sleep cannot be procured, or in which it is broken, dream-tormented, and unrefreshing. Then may the bromide of potassium be applied to, either to increase the quantity of sleep or to refine its quality. It soothes and composes the irritable brain, effecting this, as I believe, by controlling the capillary circulation, and by toning down the vivid intensity of those impressions which are perpetually bearing in upon the mind from the outer world and from the bodily organism; but where the wakefulness is symptomatic of other disorders, it is equally beneficial. Without attempting an enumeration of these, I shall briefly recount a few cases in which I have employed it for the relief of insomnia with a satisfactory result.

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[1] Mind and Brain, by Thomas Laycock, M.D., etc. etc., vol. ii. p. 301.

A. E., female, æt. 64, of arthritic diathesis, labouring under melancholia, probably the result of poverty and inanition, and a papular eruption of the skin of the hands and arms, complained of constant sleeplessness. On going to bed she felt uneasy and anxious; she could discover no endurable posture; she was distracted by forebodings and cares, and she did not procure above two hours' sleep in the course of the night. Her statements were confirmed by the reports of the night-nurse. On 17th June she was ordered 20 grains of the bromide of potassium twice daily,—one dose to be taken at four in the afternoon, the other at bedtime. On the nights of the 17th and 18th there was no improvement, but on that of the 19th she calculated that she enjoyed about four hours' sleep. After this the amount steadily increased, till at the end of a week it had reached seven hours. The character of the sleep at the same time underwent a change; it became sound and invigorating. As the patient was unwilling to get into the habit of taking night-draughts, she at once concurred in the propriety of the suggestion now made that the medicine should be stopped. Whenever it was omitted, however, sleeplessness returned, so that its use had to be resumed and continued for another fortnight. It was then again withdrawn without the recurrence of insomnia. The patient shortly after left the asylum fully recovered.

J. T., male, æt. 51, of sanguine temperament, while working in the fields on one of the hottest days of last harvest suffered sunstroke. He and another man who was working along with him, on returning home at night were seized with giddiness, sickness, and stupor. His fellow-labourer died, and J. T. became insane. Six weeks after this occurrence, on his admission to this asylum, he was found to be labouring under hydro-pericardium and hypochondriacal melancholia. He believed that his body was in a state of putrescence, that he had the foul disease, and had sold himself to the devil. Complained of distressing sleeplessness. In the whole twenty-four hours he had only two hours' rest, and was out of bed for the greater part of the night. The bromide of potassium was prescribed, 30 grains twice daily. The insomnia was relieved at once by this prescription. On the first night of taking the medicine he slept for seven hours, and subsequently he regularly obtained excellent nights. The medicine made him so drowsy that he was constantly yawning and falling asleep in his chair. As he himself expressed it, he could "drop off anywhere." This inconvenience was obviated by reducing the quantity to one dose at bedtime. In a month the medicine was omitted without any return of sleeplessness. The patient is still mentally unimproved.

H. G., æt. 42, a woman of highly nervous temperament and deeply melancholic, had been for months unable to obtain any sleep without the use of morphia. When taking $\frac{3}{4}$ gr. of the acetate of morphia as a night draught, the bromide of potassium was substituted for it. She was directed to take 20 grains twice daily. She lost one night's sleep, but after that, while taking the bromide, slept as usual. Since then the acetate of morphia and the bromide of potassium have been several times alternated with each other. The patient always loses one night's rest when the change is effected, but maintains that both kinds of draughts have an equally somniferous effect. She prefers the bromide, however, because it ensures to her as much soothing and comfort, and none of the headache, sickness, and loss of appetite which invariably attend upon the use of the morphia.

E. W., female, æt. 33, suffering from mania brought on by anxiety and shock during lactation. For some time after admission was restless and noisy at night, getting out of bed, singing and talking in unknown tongues. When in this state was ordered bromide of potassium. After taking 30 grains of it she was quieter and less excited. Next night, after the repetition of the draught, got some hours of tranquil sleep. During the maniacal attack similar draughts were frequently administered along with brandy, and always with a composing effect. As she became convalescent, E. W. herself came to recognise their action, and expressed her obligations to them.

A. R., female, æt. 50, labouring under monomania of suspicion, on her admission, 14th July, complained of prolonged insomnia. Reported as sleepless. On 16th July was ordered 30 grains of bromide of potassium twice a-day. On the night of the 16th had no sleep, but on that of the 17th slept well, and subsequently continued to enjoy good nights. Although altogether ignorant of the intention with which the draughts were given, she stated that they had a calmative effect, and that she felt better after taking them. In order to ascertain how much fancy and faith had to do with the benefit, a solution of common salt and bicarbonate of potash was sent up instead of the bromide. The fraud was not detected, but A. R. became querulous and sleepless. Contentment was restored by a return to the bromide. This medicine was ultimately stopped because the patient was getting thin and out of health.

These cases are sufficiently indicative of the hypnotic action of the bromide of potassium. Those engaged in lunatic asylum practice, and called upon to cope with nervous diseases, will most fully appreciate the importance of such an action. In contending with the different forms of pervigilum, it is no slight advantage to have at hand a remedy possessing some of the most esteemed properties of opium with other good qualities peculiarly its own. The bromide of potassium apparently induces sleep and a feeling of tranquillity without subjecting the system to any preliminary stage of excitement, and without incurring that risk of serious ulterior consequences which is so often a source of apprehension in the employment of narcotics. It exerts its effects without interfering with the secretions or confining the bowels, or leading to periodic attacks of nervous depression. It maintains its sleep-inducing power for a prolonged period without any increase in the dose, and its use may be at any time intermitted or abandoned without any fear of those refined and exquisite tortures which the confessions of an English opium-eater have so graphically portrayed.

Sleep is often in itself an object to be diligently pursued by the practitioner, altogether apart from the treatment of the derangements of which it is the sign. The absence of it is not only destructive of happiness, but is very prejudicial by tending to retard recovery, to prevent remedies from having their due effects, and to engraft new disorders upon those already established. The brain is less than any other organ in the body capable of sustaining continuous functional activity. A loss of elasticity, dilatation, and atheroma of its vessels are among the frequent consequences of defective rest. It is therefore of great moment to be able to confer repose upon it in a simple and harmless way. The bromide of potassium seems to offer us the means of attaining to this very desirable end, and thus supplies a desideratum which has been long felt. It is necessary to add, however, that there are many cases in which it would be rash to trust to it. It is feebler than opium; its range of action is more limited; it is less universally and tyrannically sleep-compelling. I would doubt its power over some intense states of cerebral disease. I would regard it as comparatively useless in the insomnia of some acute diseases, where opium, that confederacy of alkaloids, is of tried service in other ways as well as an hypnotic; still I believe it to be a valuable agent in promoting sleep and widely applicable. That it possesses this action is an incontestable fact, and that it does so in virtue of a sedative influence on the medulla oblongate is at least a probable hypothesis.

VI. It exercises a sedative influence over the sexual functions. There can be no doubt that the medulla oblongata is in close relation with the sexual organs. Experiment has shown that irritation of the medulla oblongata is rapidly followed by excitation of the ovaria and testes, and the frequent course of an epileptic seizure illustrates this fact. Regarding the cerebellum as the final centre of those afferent impressions developed in the nervous system by functional changes in the generative glands, we must still remember that these have to pass through the medulla oblongata, and thence along the restiform commissures, before arriving at their destination. Professor Laycock thinks that the unifying process, by which the teleorganic changes of the cerebellum are converted into desires and passions, with their concomitant acts, may be effected in the medulla oblongata, though he thinks it more probable that it is carried out in the corpus dentatum. However this may be, the great facts remain, that irritations limited to the medulla oblongata awaken irritation in the sexual organs, and perhaps in the kidneys, and that derangements of the sexual organs are very often accompanied by coincident derangement in the functions of the medulla oblongata. Romberg attended a woman in whom attempts to replace a tumid uterus, which had got wedged under the promontorium, were followed by instant and most violent respiratory spasms. It may therefore be, that any sedative action which the bromide of potassium may be shown to possess over the sexual functions may be exerted through and by means of the medulla oblongata.

The lowering action of the bromide of potassium upon the sexual functions has been a matter of observation for some time. Many years ago, a German physician intimated that he had known it to produce temporary impotence. Sir Charles Locock found it useful in epilepsy connected with sexual excitement, and many physicians have found it serviceable in the treatment of nymphomania, and have thus satisfied themselves of its anaphrodisiac action. A correspondent of the Lancet, however, in July last,[1] disputes the generally received opinion, on the ground that he has been informed by ladies who have been patients at Kreutznach, that the waters there, which are rich in the bromide of potassium, produced effects precisely the reverse of anaphrodisiac, and in a very marked degree. The explanation of this seems to me to be, that the quantity of bromide of potassium taken by these ladies was quite insufficient to have any anaphrodisiac power, and that the improved health which they were enjoying, in consequence of the absorption of fibrous tumours of the uterus, from which they had been suffering,

[1] Lancet, 9th July 1864, vol. ii. p. 57.

and which were in some instances reduced to one-fourth their size, allowed of a freer play of the sexual appetite, and therefore of an apparent excitement of it. At any rate, I cannot, without much stronger evidence, set aside the conclusions of many independent observers who have convinced themselves of its anaphrodisiac properties.

I have not yet had an opportunity of trying the bromide of potassium in simple nymphomania. There was one patient in this asylum last autumn who was in a state of mania following upon melancholia, and in whom there was presumed to be excitement of the sexual feelings, on account of the extraordinay obscenity of the language which she used. To her the bromide was given in very large doses, and without perceptible effect. It is only fair to add, however, that this was a case of great inveteracy, which passed through many phases, setting all remedial measures at defiance. Insanity had sprung up in a mind congenitally weak, and still further enfeebled by a dissolute life, had first assumed an impulsive form, leading to an attempt at murder, and had passed through a stage of deep depression before the maniacal type set in. The most energetic sedatives did not subdue her excitement. Against this case I have to place the statement of a sane gentleman who had experimented with the bromide of potassium in his own person, and who told me that it not only lowered animal desire, but produced a feeling of disgust with all sexual ideas. Under ordinary circumstances there must be great difficulty in arriving at anything like reliable information upon this action of the drug. The elements upon which it is supposed to operate are so volatile and variable, that nothing but a very broad and decided effect could be appreciated. Then the majority of persons are so little accustomed to introspection, or to any kind of self-analysis, that the mere attempt to estimate their own conscious experiences is destructive of accuracy. We cannot therefore place implicit reliance in the revelations of even the most honest observers. Least of all could we depend upon the statements of chronic epileptics; so I do not attach much weight to Dr Williams' remark, that he has never seen any symptoms leading him to believe in any power of the bromide of potassium over the generative organs. Whatever may be its power over the organs, I have little doubt that it affects the functions. I have seen it modify several cases of hysteria,—a disorder only developed during the years of sexual maturity, and essentially sexual in its nature. That hysteria is accompanied by hyperesthesia of the medulla oblongata has been before suggested, and is almost demonstrated by the globus and clavus hystericus, by the pathognomic laugh, by the weeping, sighing, imperfect deglutition, spasms, and cramps which constitute its most prominent phenomena. An irritation of the uterus or ovaries, occurring in a person of susceptible temperament, acts upon the cord like a traumatic irritation, increasing its excitability by the constant appeals made to it through the centripetal nerves. The irritation thus set up, which may be periodically augmented or diminished, is more or less local or general. For the most part, however, it involves the medulla oblongata in a state of heightened sensibility, and thus exaggerated reflex action prevails throughout the organism. The first case of hysteria in which I employed the bromide of potassium was that of S. R., æt. 22, single, domestic servant, of highly nervous temperament. Had convulsions when an infant. Became insane after the birth of an illegitimate child, giving way to despondency, and attempting suicide. Soon after her admission she recovered from the melancholia, but continued in an unhealthy mental state, prone to emotional excitement, but void of volitional energy. She was readily affected to laughter or tears, was sentimental in her friendships and religious professions, but destitute of all sense of truth. While in this state, menstruation became irregular. For one month the catamenia were absent, and at the next period there was a vicarious discharge from the stomach. About half a pint of dark-coloured blood was vomited at intervals. There were at the same time great prostration and sickness, loss of appetite, thick coating of the tongue, globus hystericus, palpitation of the heart, and profuse diaphoresis. The bedclothes were literally soaked with fluid, which could be wrung out of them. This condition recurred with three monthly periods, and regularly lasted for seven days. For the fourth time it returned. The hæmatemesis and diaphoresis reappeared, and the attack was pursuing its usual course when the administration of the bromide of potassium in 30 grain doses was commenced. An immediate improvement was inaugurated. The tongue cleaned, the vomiting ceased, the globus gave little annoyance, the fits of sobbing were infrequent, the pulse fell from 105 to 80, and the patient was restored to average health on the third day. Since then, and five months have elapsed, there has been no hysteria, though menstruation is still irregular. Two threatened attacks have been apparently warded off by a timely administration of the bromide. S. R. has since been discharged fully recovered. A second case of hysteria in which I tried the bromide of potassium was that of M. A. F., a girl, æt. 25, of neuro-fibrous temperament, suffering from advanced pulmonary tuberculosis with melancholia. She was liable to hysterical attacks, marked by globus, short metallic cough, hiccup, flow of limpid urine, and clonic spasms of the voluntary muscles. The bromide was ordered in 30 grain doses twice daily. The attacks were increasing in frequency when it was begun, but the intervals between them were gradually lengthened as the influence of the drug was established, while their severity was also notably diminished. Its action in this case was as decided as that of assafœtida or valerian, and was certainly more permanent. Dr Russell Reynolds has also found it of much use in cases of hysteria.

VII. It exercises a beneficial influence over certain mental diseases. We know that the vesaniæ are frequently dependent either upon functional defects in the cells of the vesicular neurine, upon altered nutrient activity in the tissues of the brain, or upon irregular distribution of the blood, owing to changes in the vaso-motor centres. We know also that the medulla oblongata and cerebellum, if they do not actually and imperatively preside over the intercranial circulation, are very influential in impressing a character upon it, and modifying it according to their own states for the time being; and we can therefore understand how exaltation, perversion, or debasement of the mental operations should often occur coincidently with disorders of these organs. We know again that local and remote morbid changes, in virtue of the law of incident or exciter action, may involve certain portions of the encephalon in morbid changes, and even implicate the whole brain, and that visceral affections, through their influence on the vaso-motor centres, disturb the capillary circulation and the metamorphosis of the tissues in those great and elaborate centres which the cranium encloses. We have thus presented to us a variety of ways in which the medulla oblongata may control the origin, progress, and issue of mental diseases. Attributing to it this power, it follows, as a natural corollary, that those agents which act on the medulla oblongata may also indirectly influence the mental processes; and when we find a substance soothing emotive excitement at the same time, and just in proportion as it calms other excited functions indisputably dependent upon the medulla oblongata, we are justified in concluding that it achieves this through and by means of its action upon the organ in question.

Without minutely considering here the uses of the bromide of potassium in the treatment of insanity, I may mention that I have seen it of signal service in orectic and thymic derangements. I believe I have seen it moderate excessive manifestations of the instincts and appetites, and afford assistance in subjugating degraded and vicious impulses and propensities. I have certainly seen it efficacious in alleviating melancholia, simple, suicidal, akinetic, hypochondriacal, sexual, emotional, etc. It is in these forms of insanity enumerated that I anticipate that it will be of most utility.

The separate links in a chain of circumstantial evidence are of little significance when individually regarded. It is only when they are brought together and viewed as a whole that they are coherent and strong enough to enforce conviction. And so it is with the observations and arguments here set forth. Most of them are in themselves compatible with various explanations besides the hypothesis proposed. It is only when they are contemplated as a whole and in their mutual relations that they are discovered to be only consistent with one theory.

ARTICLE IV.—*Case of Puerperal Convulsions successfully treated by the Inhalation of Chloroform.* By JAMES CUTHILL, M.D. Edin., L.R.C.S. Ed., Parochial Surgeon, Denny and Dunipace.

ON the evening of the 7th December last, I was sent for to see Mrs B., æt. 19, a primipara, whose confinement was not expected before the end of the month, the catamenia having last appeared in the middle of March. I found her suffering from œdema of the feet and legs, and complaining of great pain in the region of the vulva, the labia being hot, swollen, and painful to the touch. Convinced that these symptoms arose from mechanical pressure by the gravid uterus on the iliac veins, I enjoined the recumbent posture, cold applications externally, and a dose of castor-oil. On the 8th and 9th, during my ordinary visits, I found her progressing favourably, the pain and swelling having very much diminished.

At 6 P.M. on the 9th, I got an urgent summons to attend my patient, which I immediately obeyed. In not more than five minutes I was at her bedside, in time to witness the onset of a tremendous convulsive attack. When the spasms reached their height, which they did in about two minutes, the spectacle presented was truly frightful. There was complete fixation of the respiratory muscles, such as I once witnessed in a dog poisoned by strychnine; the face was literally black and hideously distorted, bloody foam issuing from the mouth; while so complete was the opisthotonos, that the body bent like a bow, rested on the heels and occiput, producing such curvature and tension of the distended abdomen as to threaten rupture of the uterus. I at once sent off for chloroform, but before the messenger arrived, which was in about ten minutes, a third attack, equally severe, came on,—the one I describe having been the second. I immediately put her under the influence of the chloroform, administered from a folded towel, and by periodical re-applications kept up the anæsthesia for three hours, all the while there being no return of the spasms. In the interim I had ascertained that there was no dilatation of the os, or other symptoms of labour. However, as the case seemed likely to prove a serious one, I desired a consultation with Dr Hamilton of Falkirk, who arrived at 9 P.M. The inhalation was now stopped, and for three quarters of an hour there was no return of the convulsions, when a comparatively mild attack supervened. It was agreed to keep up the anæsthesia at intervals; at the same time, if any symptoms of labour occurred, or if the tendency to the recurrence of the attacks remained persistent, to puncture the membranes and hasten on the labour, as the only way of radically removing the primary cause of the ailment. I waited with my patient the whole night, mild convulsive attacks recurring at irregular intervals, which I as regularly combated by repeated exhibition of chloroform. At 10 A.M. on the 10th, indications of labour pains came on, and examination detected dilatation of the os uteri, to the size of a shilling. As the tendency to the convulsions still continued, I punctured the membranes with a silver catheter. The labour-pains gradually increased in frequency and intensity, until at 11.30 they had acquired a regular bearing-down character, and the patient began to complain of pain in the back. No more chloroform was given, and as the labour-pains increased in severity, the covulsions did not return. At 12 the first stage was completed, and at 12.30 P.M., as the patient seemed worn out and exhausted, I applied the forceps, and assisted the natural expulsive efforts, until at 1.10 P.M. she was delivered of a son. The placenta was expelled an hour afterwards. The patient remained in a dreamy confused state for three days, but her recovery was rapid, and unaccompanied by a single bad symptom. On the 20th, I found her in her room thoroughly convalescent, and with girlish pride trimming a cap for her baby, in thorough oblivion of all her past troubles.

The baby, although rather small for a boy, is lively, and seems likely to do well. I may also remark that the patient's urine throughout was normal in quantity and specific gravity, and showed no trace of albumen. The propriety of giving a purgative was discussed, with the view of removing any intestinal irritation, but as the bowels had been regular before her illness, another dose of castor-oil was only given on the day after her delivery.

The prompt and decisive way in which the convulsive attacks were controlled by the chloroform, justly suggests the possible impropriety of, under any circumstances, resorting to bleeding, tartar emetic, opiates, and the other remedies recommended by the older writers. It at the same time shows the tolerance of anæsthetics under such circumstances, twelve ounces of chloroform having been expended in the case from first to last.

The propriety of the induction of labour is also an important question. As the fœtus, having only arrived at the eighth month, was most probably not viable, and as there were no symptoms of labour, interference would have been injudicious, but when labour did supervene, with the os in a dilatable condition, the course pursued seems to have been a proper one.[1]

ARTICLE V.—*On a New Remedy for Dysentery.* By WILLIAM KERR, Surgeon, Corresponding Member of the Medical and Physical Society of Calcutta, and of the Medico-Chirurgical Society of Glasgow.

TWELVE years ago, an accidental circumstance led me to attempt an improvement in the treatment of dysentery. Commencing with camphor and henbane, added to opium, I experimented on every officinal narcotic, coming to the conclusion, that of these the most efficient combination was one of opium, henbane, hemlock, stramonium, and digitalis. I had cause to be better satisfied with this than with any previous combination; but from time to time failures or tardy success induced the conclusion that something was still wanting,—that something, if to be found at all, was therefore to be discovered in plants not yet admitted into the Pharmacopœias. After a long search, Cicuta maculata, Sium lineare, and Conio-selinum canadense, indigenous to the swamps and woods of Canada, supplied the deficiency better than any others I happened to try. Sium lineare supplanted hemlock (Conium maculatum), on account of the combination containing the latter occasionally producing pain in the bowels and failing, while that with Sium lineare gave relief; and dulcamara supplanted henbane, as experience showed it to be better adapted to act beneficially along with the other members of the combination. Its constituents when the investigation was concluded were as follows:—four officinal, viz., opium, stramonium, dulcamara, digitalis; three non-officinal, Sium lineare, Cicuta maculata, Conio-selinum canadense. All are more or less narcotic; and digitalis, dulcamara, and Sium lineare are also diuretic. So many are necessary evidently from each possessing some peculiarity in the way in which it affects the system: the combined effect of these peculiarities being required to combat the disease.

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[1] Churchill's Midwifery, 3d Edition, p. 494.

Without opium the combination is slightly aperient, improves appetite, promotes sleep, and, according to experience gained in dysentery and other diseases, heals ulceration of the mucous membrane. In dysentery, opium is necessary apparently to check the frequent motions of the bowels, the strictly curative power depending chiefly, if not altogether, on the other ingredients. In infants generally, and also in a few adults, digitalis does not act favourably. In such instances I have substituted squills with great benefit. Adults generally require the combination with digitalis; of a very few infants the same may be said; and to many adults the combination with digitalis, or that with squills, may be given indifferently. Excepting opium, the part employed is the leaf. Digitalis and squills are combined in the proportion of half a part each,—all the others in that of one part. For infants, opium is reduced to a half-part. The usual dose to adults is six and a half grains, digitalis or squills being each half a grain, and all the others one grain each.

Between five and six years were spent in determining the components. Beginning with three, I never afterwards, either in adding or subtracting, changed more than one plant, till I had as fully as lay in my power ascertained the result of each change. In this manner I have experimented on thirty-two plants or their products. I have pulled down the combination, and built it up again, and thus done my best to ascertain the necessity for each component. For upwards of seven years the combination has been used with very great success; but as my own experience may be suspected of being biassed, I shall confine myself to the reports of others.

Dr Brown of Berlin, C.W., had a very severe attack, of which he published an account in the Montreal Medical Chronicle for December 1858.

Of this paper the following is a copy, slightly abridged:—"In August last I was seized with epidemic dysentery. The usual remedies were promptly administered—opium, the quantity of which speedily rose to twenty-one and even twenty-four grains daily, together with mercury, acetate of lead, and ipecacuan, but no amendment took place. I vomited incessantly, and, though tormented with thirst, could retain no fluid. In my case the effect of large doses of opium was prostrating and overpowering. I did not sleep, but could scarcely be said to be awake, except to the consciousness of severe pain, agonizing tenesmus, and frequent vomiting. I had been ten days ill, nature was sinking, collapse was to be feared, when Dr Kerr visited me. He immediately gave three and a half grains, or half a grain of each of the seven ingredients." (The recipe is here given by Dr B.)

"I was very restless from a sensation of sinking and severe pain. In half an hour, after dosing a few minutes, I became aware of a great change. I could lie quiet; the distressing tenesmus was less, pain in the body and limbs less severe, the sensation of sinking relieved, a glow of warmth was supplanting the cold of threatened collapse, and an inclination to sleep, not before experienced during my illness, was stealing over me. The first thought was amazement at the change, then a faint recollection of a new medicine crossed my mind, and I resigned myself to its influence. I was immediately asleep, and for an hour and a half had a comfortable and refreshing sleep, unaccompanied, comparatively speaking, with sensorial disturbance. When I awoke all the symptoms were relieved. Seven grains were given every six hours; but the quantity of digitalis being too great, this drug was reduced from a full to a half proportion, making each dose six and a-half grains, which were given every four hours.[1] I spent twenty-four hours almost wholly in sleep; calls to rise were still frequent, but the tenesmus was less severe, and, though I retched a few times, vomiting ceased. In a few days appetite began to return."

"During twelve years' practice, I never in the treatment of dysentery met with a narcotic to be compared with Dr Kerr's combination, in relieving general irritability, pain, and, above all, nausea and vomiting. It produces a wonderful degree of comfort, unattended by sensorial disturbance. From thirty minutes after the first dose was taken my suffering was comparatively nothing. Little hope was entertained of my recovery previous to the first dose, but became sanguine before I had taken the third."[2] Dr Bingham,

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[1] Prior to this all the ingredients were equal.

[2] The possibility of any future report from Dr Brown was cut off by his accidental death a few months afterwards.

Dr Brown's medical attendant, in a supplement, vouches for the accuracy of the narrative, and relates six confirmatory cases from his own experience.

Dr Bingham, supplied with medicine by me, treated successfully the sporadic cases which occurred in the following years:—In August 1862, he, with Dr Bell, by this time his partner, applied to me, making the following statement:—Dysentery had broken out epidemically in their locality, but not having any of my medicine, they had treated it with the usual remedies; a woman had died the preceding evening, her husband was dangerously ill, and other two were apparently dying. Furnished with a supply, they hastened to their patients. The husband just mentioned, though previously ill for five days, was relieved in less than an hour, and had a rapid recovery. One of those believed to be dying recovered readily, though upwards of seventy years of age; the other died, time to administer only a single dose being afforded. During the remainder of the epidemic there was not a death, though, judging from the severity of the attacks, six or seven would have proved fatal under ordinary treatment. In the autumn of 1863, dysentery was again epidemic at Ayr, C. W., where Drs Bell and Bingham resided. Without delay they applied to me for medicine, and treated successfully every case; while the only other medical gentleman in the same village adhered to the ordinary treatment, and out of a smaller number of patients lost five by death.

Dr Mackintosh of Hamilton, C. W., has employed the combination in dysentery since 1861, and in all cases with success. From his notes I give the following, on account of the epidemic and generally severe character of the attacks:—

"1864, 15*th July.*—A child, four years of age, seized two days ago, bloody stools every half-hour, accompanied with vomiting and severe pain. Applied hot fomentations, and gave three grains of the squill combination with opium every four hours. These were speedily followed by relief; the child had a pretty good night, and on the 19th is reported quite well.

"17*th July.*—A girl in the same house, nine years of age, was seized during the night with severe dysentery. Applied hot fomentations, and gave five grains of the digitalis combination with opium every three hours. Immediate relief followed, and next day she was convalescent. Six doses in all were given.

"22*d July.*—A boy, seven years of age, seized yesterday, and now severely affected. Gave three grains every three hours, and by evening he was much relieved. Nine doses completed the cure.

"6*th August.*—A man, aged thirty, attacked during the night with rigors and vomiting, followed by dysentery. In the morning seven grains were given every three hours. In two days he was quite well.

"12*th August.*—A man, aged sixty-four; bloody motions every half-hour, with nausea. Same doses given as to last patient; next day almost well.

"15*th August*.—A woman, aged fifty-six, during the night was attacked with severe dysentery. Same doses given. By evening was much better; and by the third day complained merely of weakness.

"17*th August*.—A woman, suddenly seized with very severe dysentery, visited shortly after; she was then cold and faint, and stools passed without control. Gave seven grains of the digitalis combination, with one-fourth of a grain of morphia (instead of opium), every two hours. After the third dose relief was so great that morphia was altogether omitted, but seven grains of the other combination were continued thrice a-day.

"19*th*.—Almost well."

Dr Philip of Galt, late assistant-surgeon, H. M. 18th Regiment, has furnished me with the following statement:—"Your remedy was administered by me in six severe, besides a number of slighter cases of dysentery during the autumn of 1862. Relief was uniformly obtained after one or two doses, and recovery completed within a few days. One of the cases was characterized by profuse sanguineous discharge, and, occurring in a delicate female, would probably have proved fatal but for the timely administration of this medicine. In contrasting the success of treatment in these instances with the fruitless and unfortunate attempts made by myself and others at relief in the severe dysentery of the Crimea, it is impossible not to be struck with the readiness and efficacy of this remedy. Every known system of treatment, I believe, was tried there, and the medical history of the campaign shows with how little benefit. Many of the cases which in the Crimea ended fatally were not apparently of a more severe character than some of those which yielded rapidly to your medicine."

Dr Merritt, who at the time the following occurred was chief of the Medical Department of the Confederate Army of the Mississippi, thus writes to me:—"In August 1863, when in charge of Camp Jackson, I came into possession of a quantity of your medicine for dysentery. The rapidity of relief and of cure was exceedingly striking. The men were on their feet in a few days, and in the worst cases I did not give more than eight doses of six grains each. My supply lasted ten days, and was administered to about sixty patients, only one of whom died. For some time before I obtained the medicine the deaths ranged from one to three daily, and as soon as it was all expended the mortality resumed the same rate."

A child of the Rev. Mr Robb, Calabar, Western Africa, in the latter part of July 1863, was seized with dysentery. At this time the favourite treatment at the mission was large doses of ipecacuan; but this illness resisted every prescription of Dr Hewan, the medical attendant. By the middle of August the child was so reduced, and death so impressed on the visage, that recovery was regarded by all to be hopeless. At this juncture the parents recollected a packet of the combination which I had given to them, After the second dose the child awoke from a refreshing sleep, easy and tranquil, and the medicine being continued, recovery went on rapidly, without a single untoward symptom. A second attack of dysentery, a few months afterwards, was stopped in a single day. A native African was cured of what threatened to be a severe illness by ten doses.

Fifteen medical men besides myself have used this combination in dysentery; it has been given in the warm region of California, amid the privations and discomforts of a camp in the hot summer of the Southern States, at sea on the Atlantic, in the tropical and pestilential climate of Calabar, and there is a remarkable uniformity in the testimony of all. Relief generally in an hour, restoration to health in a few days, and the great majority cured within a week. A few cases were a little tedious, and a still smaller number lingered for three or four weeks; none lapsed into chronic dysentery; and out of about *four to five hundred patients*, though several of the attacks were very severe, as severe as some of the reporters had ever witnessed, *only four died.* One of these was a delicate child; the second, an infant on whom the medical attendant had previously exhausted all the ordinary medicines; and the third and fourth have not been specially reported to me. I have been told of some instances, and a few have occurred in my own practice, of that generally fatal variety of dysentery characterized by profuse bloody discharges, usually attended with severe pain, all of whom were cured without difficulty. The combination fails in chronic diarrhœa, possibly because this disease is usually unaccompanied by lesion of the mucous membrane. I have not seen or had reported to me any disagreeable effect from this remedy, though, judging from the character of its constituents, such is possible, were the doses unreasonably large. The *medicinal* power is certainly greatly increased by the combination, but not the *poisonous.* Relief speedy and great of pain, far sounder and more refreshing sleep than that from opium, and cessation of discharges, are the usual effects. The nearly uniform success has not given either my correspondents or myself opportunities of trying the treatment by large doses of ipecacuan.

In the course of my experience, several persons afflicted with chronic dysentery have been restored to health,—some by the combination containing opium, others by that without. Dr Ogden, lecturer on Materia Medica, Toronto, tells me of a case of acute dysentery where, from idiosyncrasy, opium disagreed, but which was speedily cured by the combination, leaving out this drug.

Cases of cholera infantum have been reported to me by medical friends as treated successfully by the combination containing opium. Of summer cholera I select the following on account of its severity. A young man was seized during the night, and visited by Dr Bingham in the morning. At this time he was violently cramped, skin cold and clammy, voice husky, and pulse feeble. Eight grains of the combination containing opium were given; from this time he vomited no more, a glow of warmth (as in Dr Brown's case of dysentery) supplanted the cold of threatened collapse, and cramps rapidly abated in severity, though all day he had muscular twitchings. Four more doses completed the cure. Summer cholera has been for some years a rare disease, but all treated by my medical correspondents or myself have readily recovered.

In conclusion, I beg to suggest the medicine[1] now recommended as worthy of a trial in Asiatic cholera. If a remedy for this disease is ever to be found, it clearly must be one which shall take almost immediate effect.

DOON GRANGE, NEAR GALT, CANADA WEST, *5th April* 1865.

Part Second.

REVIEWS.

The Works of Sir Benjamin Collins Brodie, Bart., D.C.L., Sergeant-Surgeon to the Queen, President of the Royal Society, etc., with an Autobiography. Collected and arranged by CHARLES HAWKINS, F.R.C.S.Eng. In three volumes. London: Longmans: 1865.

Autobiography of the late Sir Benajmin C. Brodie, Bart. London: Longmans: 1865.

A GREAT physician or surgeon can have no more suitable memorial than a complete and carefully edited collection of his works. Sir Benjamin Brodie occupied for many years one of the most prominent positions in the medical profession. Universally respected both by his brethren and by the public, he died full of years and of honours, and has left behind him a name which has taken its place among the great masters of English surgery; and he had fairly earned his position. He had a mind of great natural acuteness; his faculties had been carefully cultivated; his habits of observation were minute and accurate. His writings did much to promote the progress both of physiology and surgery; while his skill as a practitioner secured him a very large and very lucrative practice. The respect in which he was held by the scientific world is sufficiently indicated by his having been raised to the position of President of the Royal Society; while his professional brethren declared their esteem for him by electing him the first President of the General Medical Council.

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[1] The medicine as recommended in this paper may be obtained from Messrs Murdoch Brothers, Chemists, Glasgow; and Messrs Duncan, Flockhart, and Co., Edinburgh.

Instead of making any criticism on Brodie's works, which have taken their position among the medical classics, we shall give a brief sketch of his career, as drawn from his very interesting autobiography.

Benjamin Brodie was of Scotch extraction; his grandfather, a native of Banffshire, came up to London towards the middle of the last century, and appears to have been an army tailor, and to have held a post in the Stamp-Office. He seems to have been fully alive to the benefits of a good education, for his son was brought up at the Charterhouse, and afterwards at Worcester College, Oxford. His son took holy orders, and after one or two changes was presented to the rectory of Winterslow, in Wiltshire, where Benjamin was born in the year 1783. Benjamin and his brothers were educated at home by their father, who was a man remarkable for his talents and acquirements. He was not rich enough to be able to send all his sons to public schools, and as he would not send them to inferior establishments, he determined to instruct them himself. Sir Benjamin's views of this method of education we give in his own words:—"There were undoubtedly disadvantages belonging to the kind of life which my brothers and myself led at this period, having so little acquaintance with those of our own age and station. We had much to learn when we came into the world which others learn as boys at Eton or Harrow or Rugby. In my own case, one was a shyness in general society, which for a long time was very oppressive, and which it took many years for me to overcome; and another was that, not having sufficient opportunities of comparing myself with others, I formed no right estimate of my own character, overrating myself in some things, and underrating myself in others. Yet I am inclined to think that what we gained in some respects was fully equal to what we lost in others. In my solitary walks through Lord Holland's woods, or over the Wiltshire downs, I early acquired the habit of reflection, and of thinking and judging for myself, and the consequence has been that through the whole of the after part of my life I have never been inclined to adopt opinions on the authority of others, nor until I had looked at both sides of the question. I also learned to be independent of others for occupation and amusement, and of this I have felt the great advantage ever since. In the many years previous to my marriage, during which I was climbing up-hill in my profession, when I passed what is called the empty season in London, with very few of my acquaintance left in the great city, time never hung heavy on my hands."

It having been determined that Benjamin should enter the medical profession, he was sent to London in 1801, and during his first season attended the lectures of Mr Abernethy on anatomy, and of

Dr Crichton on chemistry. In 1803, he entered at St George's Hospital as a pupil under Sir Everard Home, who at that time was the leading surgeon of the West-end of London. It was only now that Brodie began to feel a real interest in the studies upon which he had entered; up to this time, though very diligent, he had worked, as he says, "as a matter of duty, or I rather ought to say of necessity, than because I felt any great interest in what I was doing; and most willingly, if I could have afforded it, would I have turned my back on anatomy, and returned to literary pursuits." But on commencing his hospital duties a great change took place; a true spirit of enthusiasm was kindled; every case which came under his notice was an interesting subject of inquiry. Brodie from a very early period took notes of all cases, and the observations he makes on this subject are so excellent, that we lay them before our readers:

> "My custom has been to take short notes at the bedside of the patients in the day, and to expand them with the aid of my memory in the evening. Thus they became an exercise of the memory, and, instead of weakening, tended to strengthen that important faculty. After an experience of nearly fifty years, I am satisfied that no one can be well acquainted with his profession, either as a physician or surgeon, who has not studied it in that manner. It is only by these means that a case can be thoroughly and scientifically investigated, or that that minute and accurate knowledge of it can be obtained which is necessary to a right diagnosis. For one who is to occupy hereafter the situation of a consulting practitioner, to whom younger or less experienced persons will apply for assistance in cases of doubt or difficulty, it answers another purpose, as it enables him to express himself with greater facility, and especially to give written opinions with a degree of clearness and precision with which he could not give them otherwise. I have always, during the many years in which I was a teacher and a hospital-surgeon, endeavoured to impress on the minds of my pupils the necessity of making and preserving such written records of their experience; and I have often been pained to observe how small a proportion have followed the advice which I gave them."

In 1805, Brodie was appointed house-sugeon of the Hospital, and after holding the office for six months, he resigned it in order to undertake the duties of teacher of anatomy in the Windmill Street School; at the same time he became assistant to Sir Everard Home, his duties being to assist him in his private operations, and to attend a few of his patients. In 1808, Brodie had the good fortune to be elected assistant-surgeon to St George's Hospital; at this time he was living in lodgings, without a name-plate on his door, and was still in fact one of the senior students of the Hospital. The junior surgeon to the Hospital was at that time attached to the staff of the Commander-in-Chief, as Surgeon-in-Chief of the British forces; consequently, the two assistant-surgeons had for several years the charge of his cases, and in this way Brodie had opportunities for observation at a time when, having no private practice, he was enabled to devote a great deal of time to his hospital duties. Shortly after being appointed assistant-surgeon, Brodie began to lecture on surgery in conjunction with Mr Wilson. The following year, Brodie left his lodgings, took a house in Sackville Street, placed his name on the door, and "began to think seriously of private practice." In the course of the next year his returns for private practice amounted to the respectable sum of between £200 and £300. His success was now assured, and from this time forward his autobiography contains the records of the life of one who, having firmly planted his feet upon the ladder, gradually but surely ascended to the highest round.

Sir Benjamin Brodie never had a very high reputation as an operative surgeon; he was chiefly appreciated for his powers of diagnosis, and for the skill with which he conducted the medical and constitutional treatment of his patients. The following passage is interesting as showing that he had what may be called a constitutional disinclination for operative surgery.

> "I had anxieties of other kinds. I had now a large share of operative surgery; far more than fell to the lot of any other individual in the metropolis. Sir Astley Cooper's practice was beginning to decline, and he finally quitted London for a considerable time in the year 1828, and the greater number of patients, who would otherwise have applied to him, now resorted to myself. I was never much attached to this department of my profession, which I considered as requiring far less of intellectual accomplishments than the diagnosis of disease and the treatment of it in other ways. However, I could not venture to refuse what was offered to me, and I hope that I did justice to those who reposed confidence in me by sparing neither time nor trouble, and by neglecting nothing that could in any degree contribute to bring a case in which I was engaged to a successful termination. The only operation that gave me any real concern was that of lithotomy. Among the affluent classes of society, lithotomy is very rarely required for children, and hence those who form the very great majority of patients in the hospital, form a very small proportion in private practice. But lithotomy in adults is always dangerous, and among what are called the higher classes of society it is more dangerous than among the labouring classes; as those belonging to the former are apt to defer applying for relief to the last moment, when the extension of disease has made them less fitted to undergo an operation than they would have been at an earlier period. After the year 1835, except in the hospital, I scarcely ever had recourse to lithotomy at all, substituting for it that of lithotrity, of which my experience leads me to believe that, in the hands of one who has taken the necessary pains to understand it, it is attended with less risk as to life than almost any other of the capital operations of surgery."

Mr Hawkins's edition of Sir Benjamin's works is in three very handsome volumes, which contain all his published writings, and in addition a number of detached observations never before published, many of which are of very considerable interest. The work is one which should be in the library of every surgeon. Of the autobiography we cannot speak too highly; it contains the narrative of the life of one who, starting without any very special advantages, was enabled by diligence and perseverance to attain the highest eminence in his profession. We should wish to see the autobiography in the hands of all our medical students.

We conclude with a quotation which shows Sir Benjamin's opinion of the present very extended system of medical education.

> "I must not pass over this part of my life without noticing a very great advantage which I possessed during the period of my professional education,

compared with what I should have had if I had lived in these latter times. No rules were then laid down as to the number of lectures which I was required to attend. The examination at the College of Surgeons was sufficiently good, as far as it went, but it was of a very simple and elementary kind. It was no more than a diligent student might pass without any special preparation for the purpose.

"The consequence was, that I was enabled to take my education very much upon myself; and I soon found that I could nohow obtain so much useful knowledge as by a diligent attendance on the dissecting-room, and on the wards of the hospital. I cannot say that I neglected the use of books, but it was more in the way of reference and illustration than by a regular course of reading. I attended lectures on Anatomy, and, during one season, Dr Crichton's lectures on the Practice of Physic, Materia Medica, and Chemistry, the latter especially with some advantage. During my first season in London, I had entered as a pupil to Mr Abernethy's lectures on Surgery; but having at that time seen no surgical practice, I did not understand them, and soon ceased to attend them. I afterwards entered to some other lectures on Surgery, at the west end of the town, but found that I learned nothing from them, so I ceased to attend there also. Mr Home was accustomed to give an annual course of twelve surgical lectures gratuitously to the pupils of the hospital. These were excellent, and I attended them, year after year, with great advantage. Altogether, I do not suppose that I attended one-fourth of the number of lectures which the unfortunate students are now required to listen to under the direction of the constituted authorities. But I was acquiring knowledge in other ways, and much more substantial knowledge than can be acquired from such dull and humdrum discourses as lectures usually are; and, which is better still, I had leisure to make my own observations, to think and reflect. Nor was this style of education peculiar to myself. I remember when Mr Abernethy complained that Lawrence would not attend lectures. My friends and contemporaries, Jeffreys and Lawrence, took the same course; and so it had been with Nicolson, who was some few years in advance of us. I can easily conceive that, if I had been compelled to sit on the benches of a theatre four or five hours daily, or tempted to compete for prizes as students are, and to get crammed for various examinations, my position in life afterwards would have been very different from what it has been in reality."

Varicose Veins: Their Nature, Consequences, and Treatment, Palliative and Curative. By HENRY T. CHAPMAN, F.R.C.S., sometime Lecturer on Surgery at the School of Medicine adjoining St George's Hospital. Second Edition. Pp. 103. London: John Churchill: 1864.

FEW diseases do more to swell the numbers in a dispensary waiting-room or the out-patient department of a hospital, than does the condition of the veins we call varix, with its various concomitants and sequelæ of pains, eczematous and other chronic eruptions, ulcers, and hæmorrhages.

And it is unsatisfactory and aggravating to find how little good we can do in treating such maladies, chiefly from the fact that our patients frequently *cannot* follow our instructions. The industrious washerwoman, with seven small children, cannot lie up and rest her leg on a sofa. The artisan with the varicose ulcer must ply his trade. Almost always painful and weakening, sometimes fatal from hæmorrhage, and constantly tending to recur, varicose ulcers are among the opprobria of our art.

The modest unpretending treatise at present under review is divided into two parts. The first very briefly discusses the nature, causes, and consequences of varix. The second describes more at length its treatment.

In the first part, the views of many of the leading continental pathologists are quoted and compared, and the researches of Hodgson, Lee, Herapath, and Mackenzie discussed. Without anything specially original, Mr Chapman has here succeeded in compressing an immense amount of information into a very small space.

The chapter on treatment commences with a very thorough and conclusive exposition of the principle—that varix is not cured by the obliteration of the diseased vein. That obliteration is possible, and even that it is not very dangerous we may allow; but that in principle this procedure cannot be expected to effect a cure, and that in practice it *does* not do so, is here very clearly shown.

Fatal cases after ligature or division of varicose veins are by no means rare. Mr Chapman reminds us in a footnote of the one that occurred in Sir Astley Cooper's own practice, some months *after* he had declared that a surgeon who performed such an operation deserved to have a ligature applied round his own neck. When we think of the transitory nature of the improvement, even in successful cases, we cannot but agree with Mr Chapman in the following sentence:—

> "A single example of such an operation terminating fatally ought, in my judgment, to outweigh five hundred cases attended by no immediately disastrous results."

The chapter on palliative treatment by properly selected elastic stockings and properly applied bandages, is full and sensible. Various devices are described; one is specially ingenious, which provides at once for the support of the dilated internal saphena vein, and also for the free passage of the blood, by the external one, by protecting it from pressure.

The last chapter is on "the curative treatment of varix by wet strapping and bandaging," and is original and interesting. The late Mr Vincent, senior surgeon to St Bartholomew's, is almost the only surgeon who has written confidently of the actual curability of varix—under absolute repose and means calculated to subdue inflammation. Mr Chapman has obtained great success by the use of wet lint applied in stripes to the limb, and kept constantly moist, cold bathing, especially cold shower-baths, and friction to the limb in an upward direction.

Some cases supplement the work in which this treatment was followed by most encouraging results.

Part Third.

MEDICAL NEWS.

INTRODUCTORY LECTURE IN THE SUMMER CLASS OF CLINICAL SURGERY,

DELIVERED AT THE ROYAL INFIRMARY, ON 4TH MAY 1865, BY JAMES D. GILLESPIE, M.D., F.R.C.S.E.

THE ARRESTMENT OF HÆMORRHAGE AFTER OPERATION.

In commencing my Summer Course of Lectures on Clinical Surgery, it appears to me that I cannot go much out of my way, if I give you a few introductory remarks on a subject which has been attracting much attention lately, and which is of much practical importance; I allude to the arrestment of hæmorrhage after operation. When the bleeding vessels are small, the application of cold, or torsion of the end of the artery, is often sufficient for the purpose; but when the arteries are of larger size, more active measures are requisite. For many years (above three hundred in fact), surgeons have been content to arrest obstinate hæmorrhage by the deligation of arteries, as advocated and practised by Ambrose Paré; but since the end of the year 1859, Professor Simpson, who has already been a benefactor to surgery by his suggestion of chloroform as an anæsthetic agent, has brought prominently before the public another method of accomplishing the same end, and this has been christened Acupressure. The new procedure consists in making use of needles in various ways to control the bleeding points; the chief advantage alleged to result being a much better chance of union by the first intention. I must refer you to Professor Simpson's elaborate work for a graphic description of the different ways in which acupressure may be tried; my present object being to give you the results of my experience during the past winter. I may, however, show you the different needles employed. Here you see a needle several inches in length, intended to transfix the flap near the bleeding point, passing over and compressing the vessel, and again brought out through the flap. This was the first method suggested, and it has been thrown into the shade by subsequent experiment. The next method is by introducing this needle, about an inch and a half long, with a twisted wire-tail to it, through the soft parts close to a bleeding orifice, passing it over the vessel, then into the soft parts, the twisted wire being brought outside the wound with a view to its removal. The third plan is to pass this same needle behind the bleeding vessel, then twisting a loop of wire above it so as effectually to compress it. In pleading the cause of acupressure, which he has done in such forcible and apparently logical language, Professor Simpson pointed out what he thought the failings of the ligature, the evils resulting from its employment,—to wit, the presence of a small slough of artery likely to produce irritation and suppuration, while the silken or hempen thread, saturated by the discharges, kept up and increased the flow of pus, all exercising an adverse tendency to union by first intention. Secondary hæmorrhage and pyæmia were also thus likely to be encouraged. Acupressure was to put an end to all these complications and dangers, save in exceptional cases, and the problem of union by the first intention was solved. Metallic substances having been shown to be comparatively inert when shut up in living tissues, the conclusion was jumped at, that needles could not possibly do any harm when used to control a bleeding vessel, while a vegetable substance, by imbibing the discharges, actually did; moreover, it was found that they might be removed with impunity after a much shorter period of time, seventy hours or so being reckoned the extreme limit for the largest arteries. Cases illustrative of these good results were soon published; and were the question to be settled by the evidence adduced in favour of acupressure, and were practical surgeons satisfied that the evils attributed to the ligature actually resulted therefrom, I have no hesitation in saying that deligation of arteries would lose the confidence of the profession.

The following case led me to reflect seriously on the new proposal of Dr Simpson, for I must confess that my experience of the use of the ligature had previously been so satisfactory that I saw no good reason for departing from the practice.

William Somerville, æt. 28; admitted 27th December 1864, with the right leg crushed to pieces by the wheels of two heavily laden coal-trucks, which he had been endeavouring to uncouple while in motion. The thigh was amputated at its lower third, by the long anterior flap. Eight vessels required ligature. All the ligatures save that on the femoral came away by the fifth day after the operation. All went well till 3d January, when the patient had a rigor, and on the 4th another more severe one, the pulse rising in frequency and the tongue becoming dry; the discharge from the wound, which had previously been healthy pus, becoming greenish and thin. Copious clammy perspirations were also frequent. On the evening of the 5th, the ninth day after the operation, profuse hæmorrhage took place, which was not discovered, either by the patient or those around him, till he was almost moribund, and he shortly thereafter died. No post-mortem was permitted by the friends, but I removed a portion of the stump for the purpose of examining the source of the hæmorrhage. Mr Turner of the University kindly dissected it for me, and I now show you what we discovered.

The ligature had almost entirely cut through the femoral artery, and the portion below the noose was in a sloughy state; a small vein was lying with its open mouth close to this sloughy mass, and it passed up immediately behind the femoral artery to join the large vein. At its point of juncture a purulent clot was visible. The artery itself above the ligature was shut up by a firm clot, but a small collateral branch was open, and evidently the source of the bleeding. From this dissection I was led to conjecture that the open-mouthed vein lying in contact with the slough from the main vessel had ushered in all the subsequent bad consequences, producing first the pyæmic condition, and secondarily ulceration at the seat of the hæmorrhage. Acupressure professing to do away with these dangers, I resolved to give it a fair trial, and I shall now proceed to enumerate some of the most important cases in which I have employed it.

Case I. *Excision of the Knee-joint.*

Margaret Blaikie, æt. 16; admitted 22d November 1864, with all the symptoms of incipient ulceration of the cartilages of the right knee-joint. Various remedies were tried, in conjunction with perfect rest, to stop the disease, but without much effect; so, on 24th February 1865, the knee-joint was excised in the usual way by a semilunar flap, the patella being also removed. Hæmorrhage from two small vessels was arrested by what is called the Aberdonian method: the needles passed below the bleeding points, and then turned on themselves, so as to press the mouths of the vessels against the surrounding soft parts.

The patient, on recovering consciousness, became very unruly and hysterical, moving her right limb so much, in spite of all precautions, that considerable effusion took place. This effusion was gradually discharged as sero-purulent matter. The needles were removed on the 28th, not because it was necessary to retain them so long, but owing to the extreme nervousness of the patient, who would not permit them to be touched sooner. As the secretion of pus had taken place soon after the operation, the chief indication for their removal,—to wit, union by the first intention,—was not to be attained

In less than four weeks after the operation, the whole of the deep-seated wound had healed; and on 27th April, nine weeks after the operation, she was able to rise and make use of crutches.

I have alluded to this case, not because it is a good illustration of acupressure, but because it was quoted elsewhere as an argument against its employment.[1] It could not be expected that the compression by needles of two small superficial vessels could have any bearing whatever in preventing suppuration. All we can gather from the case is, that the bleeding was as easily restrained by acupressure as it would have been had ligatures been the hæmostatic agent.

CASE II. *Amputation at the Ankle.*

Margaret Ferguson, æt. 23; admitted 16th January 1865, with caries of all the smaller tarsal bones of the right foot. The disease had existed for a year and a half. The ankle-joint was unaffected.

24th February.—Amputation at the ankle-joint performed. Only two arteries, the anterior tibial and external plantar, required to be secured. Acupressure needles were introduced below the bleeding points, and loops of wire were tightened upon these. The stump was kept open a considerable time, and sponged with hot water, with the view of inducing other vessels to bleed, but without attaining the desired effect.

Some hours after the operation reactionary hæmorrhage set in, and the stump required to be undone, the clots cleaned away, and the peroneal artery, which was bleeding freely, secured by acupressure. As there was considerable oozing from the bone, the flap was not adjusted for some time, cold water being applied.

25th February.—About an inch in depth of the free surface of the flap was cold, and evidently going to slough. *27th.*—Suppuration freely established.

1st March.—Acupressure needles removed. *6th.*—The slough separated. *27th.*—Secondary abscesses formed higher up the leg, requiring to be freely opened.

12th April.—Another large abscess was opened at the lower part of the calf, and a considerable portion of the back part of the tibia was felt bare. As the patient was taking her meals well, suffering no irritation, and the stump rapidly healing, it was deemed advisable not to interfere, as it was possible the bone had been exposed by the abscess, and was not actually diseased.

1st May.—The discharge of pus has materially diminished from all the abscesses, healthy granulations are filling them up, and the stump is cicatrizing satisfactorily, notwithstanding the loss of a portion of the flap. The exposed bone is covered by granulations, and the wound opposite it is rapidly filling up.

This is all that can be said with regard to the case at present, but I am justified in calculating on a speedy recovery.

This is another of the cases to which allusion has been made;[2] and in such a way as to make it possible for you to suppose that sloughing of the flap, or, more truly, a portion of it, was somehow or other connected with acupressure. The history of the case shows that sloughing must have been caused by the stump requiring to be taken down and kept open for a considerable time, with cold applied, till oozing from the bone had ceased. If to this we add the circumstance, that the foot was remarkable for being much blanched, and that there was no unusual activity at the ankle, owing to the absence of disease there, we can easily account for the extent of sloughing, without saddling acupressure with the evil consequences.

What I wish you particularly to notice is the fact, that whether deligation or acupressure had been resorted to, the subsequent hæmorrhage would have taken place. The peroneal artery, having become retracted, did not bleed at all during the operation, and it was only after reaction set in that the hæmorrhage occurred. This was at once easily arrested by acupressure.

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[1] Lancet, April 1, 1865.

[2] Ibid.

I may direct your attention to a somewhat similar case, alluded to by Professor Simpson at page 287 of his book on Acupressure.

Mr Edwards had removed the foot above the ankle, and tied several vessels, but the fibular artery had retracted, so that he could not seize it with the forceps. An acupressure needle, however, speedily effected the object. The needle was left in the stump for a fortnight, causing no suppuration.

CASE III. *Amputation of the Thigh.*

George Hogg, æt. 23, railway guard; admitted 23d March 1865, the left leg having been crushed to pieces by the wheels of a luggage train about an hour previously. The limb was amputated at the lower fourth of the thigh by a long anterior flap. Five vessels were secured by transfixing the arteries at their bleeding points, turning the needles and sticking them into the adjacent soft parts. 25*th*.—Suppuration commenced. 27*th*.—Two needles removed, both bathed in pus. Owing to the quantity of suppuration, the stump required to be dressed twice daily. 28*th*.—Two more needles removed, the needle controlling the femoral alone supposed to remain; but this was doubtful, as the wire which had connected it with the outside of the stump, and which had protruded about an inch, had disappeared.

2*d April*.—Secondary hæmorrhage occurred, requiring the stump to be opened up. No bleeding point could be discovered, but there was general oozing from the torn granulations, most profuse at the inner side of the stump. Cold applications arrested the hæmorrhage. The missing needle was lying loose in the stump. From this date, the 2d, up to the 9th, the patient's general state of health was not so satisfactory; his pulse was high, his appetite bad, his expression of face anxious, and the skin sallow; no rigors could be noticed, but his high spirit kept him up, and he seemed to think himself doing well.

9*th April*.—Slight hæmorrhage occurred from the inner side on two separate occasions, which was easily arrested by cold applications. The pulse rose to 132, and diarrhœa was somewhat urgent. Though the attacks scarcely went the length of rigors, he had occasional cold exacerbations.

10*th April*.—Slight hæmorrhage again twice supervened, which was easily arrested by cold. At visit I opened the stump, and searched for the point, where these repeated small attacks of hæmorrhage were taking their origin, but only found a few granulations on the periosteum disposed to bleed. The bone was evidently becoming necrosed, and its medullary canal had been attacked by myelitis.

There was a small pulsating tumour, which seemed like the end of the femoral artery, but in which no clot could be detected, lying a short way up on the bone. No bleeding whatever was taking place from it. I passed a long acupressure needle on the cardiac side of this swelling, so as to arrest all pulsation in it.

In the evening, at 7.15, my house-surgeon, Dr Hughes, was again summoned, as hæmorrhage was now urgent. He at once applied a tourniquet, which stopped the flow of blood effectually, and then examined the interior of the stump. He found an opening in the femoral artery immediately above the little tumour previously alluded to, and above the acupressure needle. Two unsuccessful attempts were made to apply a ligature, but both failed owing to the friability of the textures; so two long needles were passed across the bleeding point, and wire loops twisted on them, with the effect of completely arresting the hæmorrhage. More blood was lost on this than on any previous occasion.

No further hæmorrhage ensued; but the pyæmic symptoms more fatally showed themselves, and he sank exhausted two days thereafter.

I have had the stump very carefully dissected by my friend, Dr Jolly, and I now show you the preparation.

You see the end of the femoral artery, with a small friable slough attached to it. The mouth of the vessel appears to be quite open, but when the preparation was recent, there was a small clot observable. Unfortunately the nurse, in preparing the body for removal to the deadhouse, displaced the two acupressure needles, but there is a trace of pressure on the vessel above its orifice.

The femur is a very fine specimen of osteo-myelitis affecting the bone to its head. Purulent clots were found in the iliac vein, and secondary abscesses in the lungs and liver.

The case proves that leaving a needle on the artery too long may be productive of mischief, ulceration being induced; but the fatal result cannot be ascribed to secondary hæmorrhage, as not much blood was lost on the various occasions when bleeding took place, but rather to pyæmia, which is at present, I am sorry to say, the scourge of my male wards.

CASE IV. *Amputation of the Arm.*

George Macall, æt. 14; admitted 30th March 1865, with lacerated compound fractures of the arm and forearm, the skin being stript off the limb from the wrist to within a short distance of the shoulder. The accident was caused by the driving-belt of a printing machine. Amputation was performed above the insertion of the deltoid by skin flaps taken from the parts least injured, the torn muscles being cut circularly down to the bone, so as to leave as little as possible of them in the stump. Eleven bleeding orifices were secured by acupressure needles, all inserted according to the Aberdonian method.

1*st April.*—I tried to remove the acupressure needles, but could only extract one, the others being firmly wedged in. The attempt at removal caused great pain. There was a considerable discharge of sero-purulent fluid. 3*d.*—Suppuration fairly established. I again attempted to withdraw the needles, but caused so much pain that I was obliged to give chloroform, opened up half the stump, and, after some trouble, got out four; no reasonable amount of force could induce the others to come away. 4*th.*—I again unsuccessfully pulled at the wires attached to the acupressure needles. 6*th.*—By catching with forceps the point of a needle, which lay across the others, and drawing it out, wire and all, through the stump, I disentangled the others, and got all of them away readily. 27*th.*—All went well subsequently, and he was dismissed with the stump healed.

This case illustrated well some of the difficulties which may be met with by trusting exclusively to acupressure. Eleven needles crowded into such a small space, though great care was taken to keep their accompanying wires in the axis of their insertion, got wedged in, so as to cause much pain and trouble ere they were removed. You must also remark that suppuration showed itself at as early a period as usual, so no gain whatever, but rather the reverse in this instance, seems to have followed the substitution of acupressure for the ligature.

I shall not enumerate cases further, but may mention that I have used acupressure in excision of the elbow-joint, and many of the minor operations, such as partial amputations of the hand or foot, excision of the breast, and removal of tumours.

The deductions I have arrived at from my experience of it hitherto are,—

I. That acupressure may be employed advantageously in the ordinary flap amputations, where the integuments and muscles are attached,—these will be for the most part secondary operations,—and also in most of the minor operations; but it is not apparently superior to the ligature.

II. In primary amputations, where the shape and substance of the flaps have often to be improvised, acupressure by means of small needles, though quite easily applied, may not be found so satisfactory; for the needles cannot be inserted with the same regularity, and if many are required, may, when sutures bring the parts together, become disagreeably entangled.

III. Acupressure is especially useful in cases where the tissues are much softened by disease, or the arteries much retracted in situations where forceps can with difficulty reach them.

IV. It is not so applicable where many are required in a small space.

V. So far as my experience as yet goes, I have seen no appreciable diminution in the purulent discharge in cases treated by acupressure, nor has the tendency to pyæmia been got rid of.

VI. Secondary hæmorrhage from ulceration of one of the larger vessels may take place under both methods of hæmostatic treatment. I have not yet seen a sufficient number of cases to determine which is most likely to be followed by it. My impression with regard to the cases of secondary hæmorrhage, which have unfortunately been met with occasionally, though rarely, in my practice, is, that they were the result of pyæmic poisoning having already taken place, the ulceration of the artery being secondary to the constitutional affection.

VII. I believe cases in private practice, or in healthy localities, are much more likely to experience the good effects of acupressure, as illustrated by Professor Simpson's cases, than in the Surgical Wards of the Royal Infirmary.

As a corollary to this deduction, I may draw your attention to the fact that pyæmia has been as deadly among patients in my Wards who have neither had ligatures nor acupressure needles applied.

NOTE.—Since writing the above, I have had several cases in which acupressure was successfully used,—one, an amputation at the lower third of the thigh for disease of the knee-joint. The needles were all removed on the third day. In this case I employed a needle to pin down the periosteum, which had been retracted from the bone previous to using the saw, over the cut end of the bone. This was done in order to prevent, if possible, osteo-myelitis, by covering up the divided bone. The patient, who was in a very unfavourable state for operation, is doing remarkably well.

In the following case, acupressure was not so satisfactory, hæmorrhage occurring on removal of the needles.

A. M., æt. 16; admitted 18th May 1865, with his hand hopelessly shattered by the explosion of a pistol, his left hand having been over the mouth of the barrel. He was sent in by railway from Peebles, without any precaution to prevent hæmorrhage, and in consequence nearly bled to death. Amputation by flaps was performed immediately above the wrist. Six vessels were secured by acupressure. The radial and ulnar were cut close to the edge of the palmar flap, and it was found rather difficult to get sufficient pressure applied to arrest the bleeding. Reaction, owing to the previous loss of blood, was excessive, and about twenty hours thereafter it was found necessary to take down the stump and secure the interosseous, which had not bled at the time of the operation, and two other small vessels.

Forty-four hours after the operation the needles were removed, when a jet of blood, forcing out a clot, came from the radial, and it bled most fiercely. The stump was taken down and the artery tied. The ulnar was beating actively, and at the side of a clot at its mouth a small oozing of arterial blood was perceptible, which made me think it more prudent to secure it by ligature. Some general oozing was arrested by acupressure. The flaps seemed in a very healthy state, and were covered with plastic exudation. They were again brought together by sutures. The boy is doing well. I used the ligature on the radial and ulnar arteries latterly, because it could be more readily applied, and appeared much more suitable for the margin of a wound than acupressure.

J. D. G.

23*d May* 1865.

ON FETID EXPECTORATION.

To the Editor of the Edinburgh Medical Journal.

Sir,—An article appeared in the last number of the Edinburgh Medical Journal, where the views which I recently expressed on some points connected with the characters of fetid expectoration, are so totally misrepresented, that, in justice to myself, I feel compelled to state exactly what I did say. Before doing so, I cannot, however, refrain from protesting against the gratuitous and erroneous assumptions of the writer, that I had not observed a case of "fetid bronchitis," or even mastered the literature of the subject. Both of these assumptions are incorrect; as, in addition to having observed several cases of bronchitis accompanied with fetid expectoration, it was only after a most complete bibliographical study, that I ventured to express the opinion from which Dr Laycock so strongly dissents.

It had been stated, most assuredly upon insufficient evidence, that butyric acid was probably very intimately connected with the production of the fetid odour of the sputum in certain cases of chest disease, and upon this insufficient evidence certain hypotheses had been originated, in which the presence of butyric acid in the sputum was made to play no unimportant part in the explanation of certain *assumed* morbid conditions. Starting upon the investigation with the most unprejudiced mind, I found that in a case which was by Dr Laycock considered to be an example of fetid bronchitis, butyric acid occurred in the sputum, although I arrived at the conclusion, after a very careful inquiry, that the odour of the expectoration in no way depended upon this constituent. Examining subsequently, with great care, and on many occasions, the sputum of a case of gangrene of the lungs, I obtained absolute proof that it contained considerable quantities of butyric acid. Directing my inquiries, subsequently, to the sputum of a great number of the most diverse forms of chest disease, I obtained evidence of the presence in almost all, of butyric acid, although I found that in one or two cases, instead of butyric acid other volatile acids were present. Having collected this evidence, I considered that I was perfectly justified in stating that, so far as our present researches extend, it is impossible to attach any importance whatever to the presence of butyric acid in the sputum.

The processes which I employed in my observations were of the simplest and most unobjectionable description; the terms in which I expressed the conclusions to which I had arrived were sufficiently clear to enable any chemist to check my results without difficulty. The mere distillation of almost any sample of sputum, acidified with sulphuric acid, sufficed to yield, I stated, a small quantity of butyric acid. This statement, I venture to say, was not liable to misconstruction, nor was it, I hope, so utterly devoid of interest as to be considered unworthy of publication; yet upon it Dr Laycock has based the following sentence:—"Dr Gamgee has, however, demonstrated, what was plain enough to all acquainted with the facts, *that we can get stinks* from any pulmonary excretion whatever, if we subject them to the same chemical torments of distillation, acidification, neutralization, redistillation, and the like."

The inquiries, to which I have alluded, had caused me to examine with care the opinions of authors who had placed on record cases of chest disease, accompanied with fetid expectoration, and I arrived at the opinion that no distinct and uniform series of phenomena had been shown to characterize those cases of bronchitis in which the expectoration was fetid. This I stated merely as my own personal opinion. That the opinion was not without foundation, will, I think, be admitted by all who have read Dr Laycock's very elaborate article; for, in addition to having shown how diverse may be the causes of fetor of the breath and expectoration, he has narrated six most interesting cases of pulmonary affection accompanied with fetid sputa, of which only one can, with any degree of fairness, be called a case of bronchitis at all.

In conclusion, I must refer to the charge which has been brought against me of having misinterpreted Dr Laycock's views. Nothing, certainly, could have been further from my intention than to misquote or misinterpret. Havin reperused, with great care, the paper by Dr Laycock in which the views to which I referred are expressed, I have failed completely in perceiving any ground for the charge, that, "from misapprehension or ignorance," I had attributed to him views which he does not hold.—I am, Sir, your obedient servant,

ARTHUR GAMGEE.

REPORT OF THE TRIAL OF ALEXANDER THOMSON FOR THE CRIME OF MURDER,

IN WHICH THE DEFENCE OF INSANITY AT THE TIME OF THE ACT WAS SUSTAINED.

Tried at Jedburgh Circuit, May 1865, *before the Hon. Lords Ardmillan and Jerviswoode.*

Reported by HUGH COWAN, Advocate.

THE prisoner, Alexander Thomson, was placed at the bar, charged with the crime of murder, in so far as on the 4th or 5th days of December 1864 (Sunday or Monday), in or near the dwelling-house or premises in or near a lane commonly called or known by the name of "Church Lane," or "Back Lane," situated in Coldstream, in the parish of Coldstream, and shire of Berwick, then or lately before occupied by you the said Alexander Thomson, you the said Alexander Thomson did, wickedly and feloniously, attack and assault the now deceased Margaret Dickson or Thomson, your wife, and then or lately before residing with you, and did, with an iron poker and tongs, and an earthenware jug, or one or more of them, or with some other weapon or weapons to the prosecutor unknown, strike her many or one or more severe blows on or about the head, breast, back, arms, hands, and legs, and other parts of her person, and did knock or throw her down and drag her along the floor, and did otherwise maltreat and abuse her; in consequence of all which, or part thereof, the said Margaret Dickson or Thomson had eleven of her ribs fractured, and was severely cut and bruised, and was mortally injured, and immediately or soon thereafter died, and was thus murdered by you the said Alexander Thomson.

The counsel for the Crown were Mr J. A. Crichton, advocate-depute, and Mr Francis Deas, advocate. The counsel for the prisoner were Messrs D. Mackenzie and A. Mure, advocates.

The pannel pled generally not guilty, and, in particular, that at the time when the alleged crime was committed he was insane. The medical witnesses were allowed to remain in Court during the trial to hear the evidence as to facts.

The general evidence led in support of the charge established the following facts. The prisoner, who was usually a sober intelligent man of a very kindly disposition, very kind to children and animals, having a number of pet animals, which he sometimes gave away, but always stipulated they should be returned to himself if his friend parted with them; a man also of great integrity and uprightness of character; had for the last ten years been subject at varied intervals to fits of drinking. During the intervals he was strictly sober. These fits of drinking had brought on upon various occasions attacks of delirium tremens, which, on two occasions, in October 1863 and April 1864, were preceded by sudden and violent outbursts of acute mania, during which the prisoner, who even when drunk was foolish and stupid, rather than noisy and quarrelsome, became outrageous and violent to a degree. One of these drinking fits began on 17th November 1864, and the prisoner continued to drink more or less down to the night of Sunday, 4th December, when he and his wife were left alone in the house. He had been put under restraint some days before by having a strait-waistcoat put on, as much to prevent his going out to get more drink as to prevent him injuring himself or others. But this had been removed as it irritated him, and his wife thought she could manage him without it. Early on the morning of 5th December he roused the medical men of the town, stating that he had killed his wife, and subsequently gave himself up to a constable on this charge. The body of deceased was found in the house lying on the floor of the kitchen in a pool of blood, fearfully mangled, wounds on the head, bruises all over the body, several of the ribs broken. The head was laid on a pillow, and the face and part of the floor had apparently been washed. The prisoner was himself covered with blood, his face had scratches, and there were two slight wounds on his head which had bled.

The only question of importance in the case related to the prisoner's sanity. A point was indeed raised as to whether the act might not have been culpable homicide, but it was hardly insisted on seriously; and the presiding judge was clearly of opinion that the act, if committed by a sane man, was an act of murder. The following is a note of the evidence of each witness, so far as it bore on the question of sanity:—

George Dickson, advocate, sheriff-substitute at Dunse, proved the prisoner's declarations, as having been emitted freely and voluntarily, after being judicially cautioned, and when in his sound and sober senses. *Cross-examined.*—Before taking the prisoner's declaration, as I understood that he had been drinking, I desired the procurator-fiscal to obtain a report from a medical gentleman as to his mental state. I was so far guided by the report received from Dr Tait, that if he had said the prisoner was insane, I would not have examined him. He seemed dejected and depressed. I knew nothing of prisoner having been insane before; I had merely heard that he was frequently intoxicated.

Andrew Donaldson, sheriff-officer in Coldstream.—At a quarter before 7 on Monday morning, 5th December last, prisoner came to my house, half-dressed, and knocked at the window. I opened the door; he came in and said, "I've come to give myself up, Andrew—I've killed Margaret." I thought it was nonsense. He came into the kitchen. I noticed blood on one of his shirt-sleeves. I thought perhaps they had had a shine. I agreed to go with him. On the way, I asked prisoner if it was really true that he had killed Margaret? He said, Yes. On reaching the door I said, Have you candles burning? He said, Yes. I said, Go up stairs. He went and I followed. We went into the kitchen; deceased was lying there with her feet towards the bed, and her head near the fireplace. I said, Good God! she is dead. Prisoner said, It's ower true, Andrew. Prisoner sat down near the fire. I told him to sit there till I got Dr Brown. Prisoner said he had tried all the doctors already, but couldn't get any. The doctor wasn't in, so I left a message and went back; prisoner still where I had left him. Went for some of the neighbours. I observed a pair of tongs near the fireplace, with a mark as of blood on the handle; a poker also, blood and a hair on it. There was blood on the bed and blood on the floor, a large stain near the body, about three feet broad. I thought it had been washed; the floor was damp as if recently done, not nearly dry; a piece of cloth beside it, quite damp. I thought it had been used to wash the floor. The face of deceased seemed to have been washed. There was a rope tied about the right leg. The prisoner was in the other room. I had some conversation with him. I asked him about his wife's death. He said he had done it with that jug, pointing to the fragments of a broken one in passage, an ordinary earthenware jug; there was no blood upon it. Saw prisoner's shirt taken off him; it was saturated with blood. One of the arms appeared to be wet with water. Prisoner had two cuts on his head. He said, "The one of us had the poker and the other the tongs." The police came, and I asked no more questions. I had asked him when it was done; he said we begu'd the game about 10 last night. He appeared to be sober. Did he seem to know what he was saying? I don't know; he was shivering very much. I had seen him a few days previous; he came out by back door; he had a strait-jacket on, and nothing but his shirt and stockings. The strait-jacket wasn't tied and didn't restrain him, his hands were free; he had a pint bottle in his hand, and came forward, saying, Andrew, can you not save my life? I said, No; go into the house, sir. The bottle appeared empty. When sober he conducted himself well enough. When intoxicated he was silly-like. I never saw him riotous but once—it may be seven years ago; he was swearing and noisy, and threatened to strike his brother. *Cross-examined.*—The morning that he came his face was covered with scratches and blood; he was kind of shivering and crimped up; his voice was feeble and half crying, quite different from usual; his eyes were staring like, just fixed like two stars. The hair on the poker was a grey hair like prisoner's. I saw deceased on Saturday night; asked her how she was to manage with him. She said, What do you think I should do. I advised her to get some beer, which she said she would do,—adding, I'll just man myself to him, I'm no feared.

Dr Alexander Brown, physician and surgeon in Coldstream.—There was a ring at the bell early on Monday morning, 5th December last. The girl said it was Alexander Thomson. Donaldson came afterwards. I went to prisoner's house about 7; went into kitchen; prisoner there sitting on a chair in the middle of the floor. Examined deceased; she had been dead three or four hours. There was a broken mug in passage; the furniture was all in disorder; a large earthenware dish, broken, was standing near the window. There was a great deal of blood all about the room and in the bed, and under it observed some hair of a terrier dog on tongs; none on poker. When I went in, I said to prisoner, How have you done this? Prisoner, We were quarrelling; I made a blow at her with a mug, the handle broke and it flew against her head and cut her severely. I examined jug; no blood on the fragments. I said, Oh! nonsense, that could never do it? Did you not strike her with the poker? Prisoner, No, I never did; she struck me with the poker across the head, and I took the tongs. Dr B., This is a bad job for you, Alexander. Prisoner, You don't think it's murder, doctor, do you? Prisoner was vomiting, and I asked him what it was. He said it was some whisky he had drank during the night; he had got it on Sunday; he said his wife had given him two bottles of beer. There was a rope round deceased's leg. I asked prisoner what the rope was doing there; he said he had put it on to enable him to draw wife out from underneath the bed. Prisoner's face was scratched; there were two short wounds on the top of his head, which had bled. I made post-mortem examination of deceased. The report shown me is a true report. Witness read the report, which, after describing two parallel wounds on the scalp over the left temporal region, contused and lacerated, and penetrating to the bone, there being, on removal of the scalp, a very large and general extravasation of blood over the whole head, and also a great many bruises over the arms, shoulders, hands, and right breast, some of them very severe, and a variety of minor bruises and wounds all over the body; ascribed death to violence, adding, "We farther consider that the various wounds might either in the aggregate, or those of the head or chest separately, prove quite sufficient to cause the fatal issue." The wounds might have been by poker or tongs—not, I think, by the jug. The fracture of the ribs must, I think have been inflicted when the woman was lying down. The injuries on the head and on the body were either of them sufficient to cause death. I saw prisoner on the Friday before this in his house in bed, very nervous. I told him if he didn't stop drinking, he would bring on delirium tremens. He was quite rational. I saw him in his shop on Saturday night. I have assisted Dr Turnbull to chain prisoner when in a fit of drinking to the bed, and once put a strait-waistcoat upon him. This was to keep him from getting more drink. This restraint was sometimes for two or three days, then he would be sober a while. When sober, he was very intelligent; when in drink, very noisy and abusive in language; but I never saw him violent to injure any one except once, in 1863, on a Sunday morning. Dr Turnbull told me that prisoner had tried to strangle him; prisoner was then restrained. I never saw him in delirium tremens. On the Monday morning, prisoner was not sober, but not drunk; he seemed recovering from drink. *Cross-examined.*—Prisoner had a look of horror on that morning. He looked like a man startled out of some horrible dream, awaking to the consciousness of something dreadful—trembling very much. There was no concealment, except the washing on the floor round where the body lay. The pillow where her head lay was soaked with blood. Prisoner said his wife had asked him to pull her out from under the bed; that he tried it with his hand and couldn't, and then took the rope. There was no smell of spirits in the vomiting. The injuries on the body were dreadful—must have been by repeated blows continued for some time. Prisoner and his wife lived peaceably. When coming out of a drinking bout, prisoner was shaking and low. In 1863, I went about half-past 9 to prisoner in morning—prisoner in a state of great fury—tearing and smashing—his shirt torn off his back—he had smashed his bed—behaved like a man in a state of acute mania—was almost naked. We held him and put him into bed, binding him down; a towel pinioned him, and a sheet was crossed over him and fixed to the bed—his feet were also tied. He just lay and roared loud and furious, like an insane person—roaring very quick and continuous—eyes glassy and staring—he didn't know me—he spoke as if he was drowning in a river with a tree on his breast, and called his dog Topsy to take the tree off him. Again he spoke as if he had got into a meadow and couldn't get out—asked why we didn't help him—was angry. He fancied he was captain of a ship which was on a sandbank, and we were the sailors, and wouldn't obey orders. He said if we didn't do what he told us he would send us all to hell. He roared out all this loud and fast, and threatened violence. His mood did not alter during my visit, which was from half-past 9 till 2, when he got quieter. At that time I thought him thoroughly insane, totally deprived of reason. I heard from his wife that he had been drinking for some days before. At 4 P.M. I saw him again—he was quite calm—the fancies were gone—he knew me. Saw him again at 10 P.M.; he then was low, as if taking delirium tremens. On 17th November last, I saw prisoner on street; I didn't visit him then—saw him when he was bound with strait-waistcoat on 25th November. The sleeve-ends of it were burnt, and the hands out. I thought he was going to have an attack of delirium tremens. He was shaky and nervous; the tongue very much furred; rather tremulous. I prescribed tartar-emetic and opium to ward off the attack. On the Friday before deceased's death, prisoner was much in the same state. *To the Court.*—There were no delusions on the Monday. When he said, You don't think it murder? I think he knew the meaning of what he said. In 1863, the attack was one of mania, not of delirium tremens, but of mania caused by drinking. *On being recalled afterwards, Dr Brown further deponed.*—I can form no medical opinion as to the state of the prisoner on the night between 4th and 5th December. *Cross-examined.*—I saw prisoner, on 2d December, much excited. I think an attack of delirium tremens on the Sunday night very possible—it was more likely in the case of a man who had had a previous attack. The symptoms before and after the act are quite consistent with an attack during the night of the 4th.

Helen Shields, a neighbour, went to the house on Monday morning, 5th December, being called by Donaldson. Prisoner said it was a bad job. Said he was sorry for what he had done, for she ill-deserved it. I didn't hear him say why or how he did it. He seemed sober and collected and sensible. He knew what he was doing and saying. When drunk, prisoner very foolish. Never riotous that I saw. Have seen him with a strait-waistcoat on, anxious for more drink. *Cross-examined.*—On the Sunday night, deceased brought me prisoner's watch to keep till the Monday. The bed in prisoner's room on Monday morning seemed to have been lain on.

William Brown, sergeant of police, went to prisoner's house on the Monday morning. I saw the dead body. Part of the floor was recently washed. Blood on other parts of the room, and in kitchen-bed and under it. I saw poker and tongs; blood on them; broken fragments of a jug. Prisoner said, "I wanted out; she wouldn't let me, and I struck her with that jug." He then said, "She took the poker to me, and I took the tongs to her." Witness then spoke to condition of the room, and identified different articles. I took prisoner to Dunse, getting there about 10 A.M. Prisoner asked what charge I had made out. I said none but causing the death of his wife. He asked if he would be allowed an agent. I said no, not when examined before the Sheriff. Prisoner seemed coming out of drink; but, so far as I knew, he was aware what he was about. The witness stated that, on being applied to by Dr Turnbull, he had refused the leg-irons.

John Tait, surgeon, Dunse.—I saw prisoner on Monday morning at Dunse between 9 and 10; he was like a person who had a cold, tremulous. I felt his pulse: moderate; tongue tremulous. Put some questions to him: he was sensible and coherent in his answers; his manner was quite calm and collected; he had no delusions; was satisfied he could be examined. Saw him again in the evening on my return from Coldstream, about 10 at night: much in same state; hands tremulous; prisoner wished an opiate; he feared he mightn't get sleep; I didn't give it to him. Saw him on Tuesday morning, and found he had only slept three-quarters of an hour in previous night. *Cross-examined.*—From what I saw, I thought prisoner was just recovering from a debauch of drink, but the nervous system was more unhinged than usual. I refused the opium because I thought it best to leave nature to herself; I thought an attack of delirium tremens was impending, and that it might be well for the prisoner's interest to let it develop itself.

Cecilia M'Lachlan, a neighbour.—Prisoner came to our house the day before his wife died: he wanted a boy sent for drink; he gave sixpence, another man gave sixpence; the boy brought a shilling's worth. My husband just tasted it; prisoner and the other man drank the rest; he came back in the evening and got more. *Cross-examined.*—Prisoner complained of a sore head, and held it down; did the same at night.

Nicolas Dodds, druggist.—On the Sunday night, about 6 P.M., the prisoner came and asked for a draught to give him rest and sleep; he said he was not well; I could not say he had been drinking; I gave him a draught. *To the Court.*—I did so to allay nervous irritation; it was about 25 drops of chlorodyne; his hand was shaking; he said he was going home to bed.

Peter Thomson, brother of the prisoner.—On Sunday, 4th December, I was in prisoner's house about 5 P.M.: I spoke to him; asked what he had been at; prisoner looked very wild, and said that was his business,—"Don't speak to me; I'm distracted (or distressed)." He was not drunk then, but as if he had been drinking,—wild-looking and staring. I was there at night, and getting no answer when I knocked, was going away: the wife came out, and said *he* had gone to bed and was asleep, and she had hidden his clothes and locked the door, to prevent the doctor putting on the strait-waistcoat, which she would not have, as she would manage him herself. This witness corroborated previous witnesses as to the prisoner's state of excitement and drinking during week preceding wife's death. *Cross-examined.*—Prisoner of the kindest disposition, quiet and obliging, fond of children and animals, and kept pets; a man of integrity; a regular church-goer. He took fits of drinking; the intervals varied; was strictly sober in intervals; after an attack he was very sorry. I think he fought against the fits: when drunk he was in despondency,—

mourning, melancholy; never quarrelsome; abusive with his tongue, but not violent. After prisoner's marriage in 1852 up to 1863, he had some attacks of delirium tremens; a bad one in 1855; he was then delirious; he had been drinking for two or three days; Dr Turnbull attended him; he was raving; I held him in bed he was so wild; I thought him insane then; it lasted all night; he fell asleep about 2 in the morning; the wildness lasted six or seven hours at that time. Before October 1863, prisoner threatened to destroy himself with a razor, or by drowning or hanging himself: sometimes the attack of wildness lasted a night, sometimes a day or two. The witness spoke to the attack in 1863, adding that, on prisoner's recovery, he said to his wife, "Ah, but you struck me with the poker!" and she said nothing, and afterwards admitted to witness that it was quite true. He also spoke to attack in April 1864, and prisoner's state before wife's death. Saw prisoner on 29th November; Dr Turnbull there; I thought prisoner mad; I remained two hours; he was restrained; he got very wild; the wife didn't wish the strait-waistcoat put on; it was put on; prisoner bit me on the arm while assisting. Saw him next day at 9 P.M.: he said, "Peter, I'll never taste drink while I live again." On next day, 1st December, I saw prisoner with strait-waistcoat on in his house: prisoner in a stupid state, trembling and shaking, not drunk; he said, "The only thing for me now is to draw a razor across my throat!" Witness further stated, that on the 3d December he saw deceased about 9 at night, who said prisoner had been sitting crying, and that he had gone out, and she was afeared something would happen, that he might do some ill to himself, for he wasna right in his mind. *To the Court.*—After these fits, prisoner never remembered what he had done. If he did remember, that would be unlike what was usual after his fits. He never could tell me what had occurred.

Agnes Finlayson and *Helen Gray*, servants respectively to Dr Turnbull and Dr Brown, proved prisoner having come early on the Monday morning asking for their masters, saying his wife was dead, and he had killed her. To one of them he said it was a bad job, and asked for a snuff.

Matthew J. Turnbull, M.D.—I have known prisoner many years. I knew his wife. Prisoner kept a shop in Coldstream. He acted as book-keeper for me and Dr Brown. He has been subject to fits of drinking for about ten years. When he took fits we had to use restraint. He once gave up drinking without any treatment. He was absolutely sober when not on a fit of drinking. The last fit began on 17th November, and continued with very little interval till his apprehension. He attended to the books, but did so ill. On 24th November I spoke to prisoner, and found he was not correct, and stopped the writing. I had to restrain him that night; I saw him very drunk; I put on the strait-waistcoat; his wife on that occasion assisted. I was out of town, and didn't see him till next afternoon. Dr Brown had prescribed for him. The strait-waistcoat was on, but not tightened. I saw him on 26th: he was better, and got up and went into shop. On 28th November, I saw him again; he had been asleep, but woke up; I saw he had been drinking. I saw him on 29th: he had been drinking; same night I found him very well, but he had been drinking to excess; he was in bed, and knew me, but was speaking incoherently. His brother came: I put on the strait-jacket, and fastened his feet down in the bed. I saw him next morning: he was depressed; he promised not to go out, and I loosened the jacket, but left it on. Saw him again at night: drunk and very excited; I went to get leg-irons, but police-sergeant refused them; I then put on the jacket. I saw him on 1st December: still in bed, excitable and haggard. Saw him on Monday 2d December: like a man who had had a long fit of drinking. Saw him on Saturday the 3d: better; and, as he promised better behaviour, his clothes were brought back. On Saturday night I saw his wife, not himself: she said he was in bed. I saw him on the evening of Sunday, 4th December, on the street: I asked where he was going; he answered home; he had had more drink; I led him home; wife there as usual; I think I told prisoner to go to bed; I told the wife to take off his clothes, and I saw him into bed. This would be about a quarter past 7. I did not see him again. There was no attack of delirium tremens or mania from and after 17th November. *To the Court.*—On the 11th October 1863, I thought prisoner perfectly insane from 9 till between 2 and 3. He was calm at 4, and in the evening, between 7 and 8, quite calm. He said then that he had no recollection whatever. *Examination resumed.*—I think prisoner would know the difference between right and wrong up till the Sunday night, 4th December. I think he was not mad up to that Sunday night, so far as I saw. There was no attack of delirium tremens, and no acute mania. He was dazed and confused. The chlorodyne[1] would, I think, tend to increase his excitement. It is probable he was delirious during the night. My opinion is not affected by hearing that, on the Monday morning at 7, he was calm and coherent. I don't think prisoner would have remembered next day what he did if he had been insane during the night, or if he had had an attack of delirium tremens during the night. My experience as to acute mania is, however, limited. *Cross-examined.*—Prisoner was kind-hearted, obliging, and honest. I never knew of him using violence to any one. He had an attack, in 1855, of delirium tremens: he was very ill. I consider delirium tremens to be mania arising from drink. In 1855, he was maniacal: the attack was short: he was well next day. In 1863, I saw prisoner, as above: he had been drinking hard. We had to restrain him, tying him to the bed. His wife came for me on Sunday, 11th October 1863. I think she said to me she had struck him with the poker, and he was then very violent, suddenly so. I found him on the floor nearly naked, awfully violent: he was quite insane: he bit a piece out of the cup in which we gave him water. He had then several hallucinations: there was a tree on his breast; he was in a meadow or on board a ship: he seemed about to attack us. That morning he tried to strangle me, and he was very violent; he nearly succeeded, but I escaped from him. He knew me at 4 o'clock, and then knew right from wrong. This attack in 1863 didn't last above seven hours. At 7 or 8 P.M. he was like a man coming out of delirium tremens, with creamy tongue and trembling. Prisoner also had an attack in April 1864, but not so violent. In November 1864, his wife told me prisoner had burned the sleeves of the strait-waistcoat. On 25th November, his tongue was creamy as if from drinking. On the 29th, he resisted me violently; an attack of delirium tremens would not then have surprised me. When I was refused the leg-irons, I wrote the letter now shown me to Mr List. On Thursday morning, December 1, prisoner asked me who put on the waistcoat, and I said I did: he seemed surprised. On Sunday, the 4th, his tongue was creamy and tremulous, and an attack of delirium tremens seemed to me probable. His wife didn't wish me to put on the jacket that night: she asked me not. I think she said she could manage him herself. I cannot say if he knew right from wrong then. I would have put on the leg-irons then if I had had them, (1.) to prevent him going out; and (2.) to prevent his doing harm either to himself or any one else: delirium tremens seemed to me highly probable. I have only once seen a case of mania coming on and going away so quickly: it lasted seven or eight hours, during which the man was insane. I understood that the cause in that case was drink. The symptoms in December last were very similar to those I saw in October 1863, when I thought the prisoner insane. So transient an attack was not, I think, a very probable result. I cannot say it was at all improbable. I do not think I can express a medical opinion on that point. *To the Court.*—It was my opinion in December that an attack of mania or delirium tremens on the night of Sunday, the 4th, was probable; and all that I have heard to-day does not tend to qualify my opinion of that probability. I still think so.

The prisoner's declarations, taken before Sheriff Dickson on the 6th and 9th December, were then read. In the first he stated,—"I was drinking heavily for three weeks previous to Sunday last, 4th current, and was also drinking on that day. I do not recollect of going home after I got the last supply of whisky on Sunday night. After dark in the evening I went to Dodds the druggist's for a dose, and to the best of my recollection, on coming out, I was met by Dr Turnbull and taken home. It is like a dream to me that Dr Turnbull was in my house. I don't recollect his leaving. I do not recollect of going to bed at all that night. I had no quarrel with my wife that night that I recollect of, nor am I aware of having struck her; indeed, certainly I did not strike her. I partially came to myself between ten and eleven o'clock on the Sunday night, and I was then standing on the floor looking at the clock. I had nothing on but my shirt and stockings; my wife was then lying in bed. I spoke to her and said that I was quite delirious, and would get worse if she did not come beside me. She came and gave me some ale. I drank two bottles of it. I then lost my recollection for a while, and when I came to myself, I was going for a drink of water with a jug in my hand. My wife, who was then sitting in the same room, the kitchen, jumped up and said she would leave the house. I said to her it would never do to leave me, as I could not be left myself. She still persisted, and I said that if she did persist I would strike her with the jug. I said this, although I did not intend to strike her. She then rushed forward to me, and I struck her on the head with the jug. It was so dark at the time that I did not see where she was, and I did not intend to strike her at all. On being struck with the jug, wife said she would brain me, and then she struck me a dozen times with the poker on the head. I took the poker from her, and laid it down at the fireside. I did not strike her with the poker. I believe I went to bed after laying down the poker, but I am not certain. I am not aware that my wife went to bed. I lost my recollection after laying down the poker. I was fairly done out, and don't recollect what further occurred. I think, but I am not sure, that I was in bed when I came to myself. I think this would be about 5 o'clock yesterday morning. I got up and lighted the candle, but could not at first find my wife. At last I found her underneath the kitchen bed,—a different bed from the one I was sleeping in. My wife said she was very unwell, and could not come out from under the bed. I said it was not good for her lying there, and she should come out. I then tied a piece of rope round her ankle and pulled her out. She said she was very poorly, and I lifted her into bed. I went into the same bed and lay down beside her. She was then lying on the front part of the bed, and in a little fell over. I got over the bed, and tried to lift her up, but could not. She said she was very poorly, and asked to be allowed to lie still. I then got a pillow and put it below her head. I sat down on a chair beside her, and asked her repeatedly if she was any better; she said no. At length she did not speak, and I got alarmed. I then went out for Dr Turnbull; but I was so confused that I thought the servant-girl was the doctor. I do not know what hour this was. I believe, but I am not certain, that I went to Dr Brown's after leaving Dr Turnbull's. I don't recollect if I saw Dr Brown. I went home. I was still alarmed at wife lying still, and went to get Dr Purves." In his second declaration he identified various articles found in the house, as being those referred to in the first declaration.

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[1] In the course of the trial it was proved that chlorodyne had been prescribed to prisoner.

This closed the case for the Crown.

The following medical evidence was led for the defence:—

Dr Skae, physician to the Edinburgh Lunatic Asylum.—There are between 600 and 700 patients under my charge. I have been nearly nineteen years there. Excessive drinking tends to produce insanity, both mania and delirium tremens. These often run into each other. I have known cases of mania from drinking where the attack was very sudden and very brief, passing away in a very short time. Cases of acute mania may come very suddenly, and pass away very quickly—more or less quickly. Such attacks are likely to recur, and more likely after each attack. I recollect the case of Murray—the attack there came on very suddenly, and lasted a very short time. He came to himself very soon, and was very soon quite sane. I know another case of a woman who killed her child while under a delusion—she was not conscious she had killed the child—she soon recovered her reason. I don't think remembrance of what is past is inconsistent with insanity—more generally there is none—the recollection may vary even in the same person. I think a violent shock may restore calmness. What is your medical opinion as to the state of the pannel on the night of 4th December? I think a transient attack of mania very probable under all the circumstances. The previous symptoms indicate a tendency to delirium tremens or mania. The conduct of the prisoner, and the savage atrocity or unnecessary violence of the act, with the want of motive, are circumstances in favour of the probability of his having an attack of mania. I think it is highly probable that he had.

Dr John Tuke, superintendent of the Fife Asylum now in course of construction, and at present assistant in Morningside Asylum.—I had charge of troops in New Zealand, and have seen cases of mania caused by drinking. I knew an officer who from that cause got suddenly mad. I have known such attacks to be very violent and very dangerous. I have seen such cases in persons brought to the asylum, who became calm on admission. A shock may dispel the mania. The recurrence of such attacks is very probable. A smaller quantity of drink will bring on an attack the second time. I can only speak as to probabilities; but so speaking, I think it very probable prisoner was in a state of mania or violent delirium on the Sunday night in question. *To the Advocate Depute.*—What a man would remember after such an attack may vary. I see no reason why memory should be completely destroyed in this case; probably it would be impaired. *Re-examined.*—Sometimes there is very vivid recollection—sometimes very little. I think a violent act or shock would remain on the memory. *To the Court.*—The symptoms and conduct of the prisoner lead me to the opinion I have expressed, though no delusions at the time were proved. I think the symptoms described by Dr Turnbull and others such as I would expect. The symptoms on the Monday morning are not just such as I would have expected, but they are not inconsistent with an attack of mania on Sunday night, which I think probable.

Esther Robertson, a neighbour, corroborated by her husband.—On Sunday 4th December, prisoner came to our house between 11 and 12; he lay down in the bed, and staid about an hour; his wife came and took him home. He was shaking very much and talking to himself. He was hanging over the bed; he did not appear like a man filled with drink. When he was going away he looked back very wildly, his eyes working in his head; I was afraid of him.

Andrew Guthrie, shoemaker.—Saw deceased about 10 on the Sunday morning looking for prisoner; she said she was afraid, for he had been threatening to destroy himself, and she had hidden the razors. She said that prisoner had had a rope round his neck tied to the top of the bed, and she had loosed it, and he put it in his pocket—that this was on Sunday morning.

Agnes Guthrie, his wife, spoke to the deceased having told her on Sunday week that she had taken away the knives and razors from prisoner, but that it was a bad job and would have a bad end.

Hugh Munro, police constable, spoke to prisoner's violent attacks in 1863, and in April 1864.—I have only seen one man as violent, and he was locked up about 6 at night, and was quite well next morning. I thought the man in that case mad, but he had recovered his reason next morning. I saw prisoner at Dunse on Monday, 5th December; I was with him from 9 P.M. till 3 A.M., and from 8 A.M. next day till 2 P.M. Prisoner did not sleep, he stared with his eyes; he started up in his sleep, and said, O, sergeant, I have had another attack of them.

Dr Patrick Kynochan, surgeon at Greenlaw Prison.—On 7th December, prisoner had an attack of delirium tremens; tongue tremulous; furred behind, and in front hot and dry. He imagined there were persons in his cell, when in fact there were none. *Cross-examined.*—I cannot give any opinion as to whether he could have had an attack of delirium tremens on Sunday night, 4th December. *To the Court.*—I think there might have been two attacks of delirium tremens, and yet calm behaviour between—that is possible. I haven't seen an instance of it. When two days elapsed without the person taking any alcohol, I think it possible and probable that there may have been an attack on the night of the 4th, and another on the 7th. That is my opinion, but I never saw it happen.

David Alcoran, one of the constables at Greenlaw.—Prisoner was brought to the prison about 11 o'clock on Monday, 5th December, shaking and excited. He said if he had a chance he would do himself an ill turn. During the night he started up, saying he had had another visiter; he stared occasionally at the roof; his eyes red. Once he said, "Oh, its awfu', Davie"—he was shaking and restless. I thought it delirium tremens.

William Smith the governor, and *James Grant* the superintendent, at Greenlaw, also give similar evidence.

The Advocate-depute on the part of the Crown, and Mr Mackenzie on behalf of the prisoner, having addressed the jury,

Lord Ardmillan charged the jury. In doing so his Lordship observed:—

The defence—a most relevant and important plea—is that the prisoner, though now sane, was insane at the date of the act. This is a most important question in our law, and it is necessary that you should understand the law on this subject. It has been said that there is a presumption of innocence in favour of a person accused of crime; and that is most true. If there is any doubt of the act having been committed by the accused, this presumption of innocence requires you to give him the benefit of the doubt. But the case is different in a question of sanity. There the law presumes that every man who is going about his ordinary business, and who stands at the bar a sane man pleading to the charge against him, was also sane at the time the act was done. The burden, therefore, of showing that he was not sane rests with the prisoner. If the defence is not proved to your satisfaction, you cannot give the prisoner the benefit of any doubt you may entertain; for unless you are satisfied beyond a doubt of his insanity, you cannot find it proved.

As to the nature of the insanity, I have to state in the most explicit manner, that the law does not recognise drunkenness as a defence. It would be very hazardous if it did. Again, if a man has been a habitual drinker, and if, in consequence, he becomes liable to be more violent when in liquor, or liable to be affected by a quantity of drink that would not otherwise have so affected him, this will not serve as an excuse for crime, any more than present immediate drunkenness. I state these two propositions as matters of plain law and plain sense. The defence must go far beyond these. But the defence is conclusive if, at the date of the act, there was real insanity, such as I shall now describe to you in the words of the English Judges in 1843,—"To establish a defence on the ground of insanity, it must be clearly proved that at the time of the commission of the act the party accused was labouring under such a defect of reason from disease of the mind, as not to know the nature and quality of the act he was doing, or, if he did know it, that he did not know that he was doing wrong,"—a statement which, having the authority of the whole English Judges, was approved of and adopted as law in this country in the case of *Gibson* in 1844, again in the case of *Lillie Smith* in 1855, and more recently still in the case of *Bryce*. Baron Hume puts it thus,—"The disorder must amount to an absolute alienation of reason." You can at once perceive the distinction between such a state as is here described and an uncontrollable passion. In popular language, a man is sometimes said to be *mad* with drink, or *mad* with jealousy, revenge, lust, or passion. But the dominion of passion never can excuse from crime. The fact that a man is in such a state, whether from drink or any other cause, that he cannot control his passion, will not excuse him. You must be satisfied that there is actual insanity,—a real and absolute alienation of reason. It is a most melancholy fact, that the habit of drinking does sometimes result in such a state; but whatever be the cause of insanity, if the man was at the time of the act actually insane, if he was the victim of disease of mind, producing absolute alienation of reason, then you cannot find him guilty of a wilful act of violence done when he was totally deprived of reason. In that case the defence is established.

The only other point on this head is, that the question of insanity at the time of the act, is a question for the jury on the whole facts,—not a mere question for the opinion of medical men. Evidence of their opinion is important, and may be highly important, but its weight varies with circumstances. A medical man who has long known the prisoner, who has attended him professionally, and has had frequent opportunities of seeing him and judging of his state of mind, is in a different position from one who has only seen him recently, and, perhaps, only at the bar. In the words of the Lord Justice-General in the case of Bryce—"the question of insanity—of insanity to the effect of relieving a party from responsibility—the question of whether a man is insane or not, is a question for you to decide. It is a question on the whole facts of the case; it is not a medical question. The medical gentlemen have opportunities of observation which make their testimony frequently very important in reference to such matters, but the question is not a medical question." You will give due attention to the opinions of the medical gentlemen. They are for your consideration along with the whole facts of the case. But you must yourselves be satisfied; the decision is with you.

His lordship proceeded to a review of the general evidence in the case. It is of great importance in this case that you make yourselves well acquainted with the particulars of that attack in 1863, which has been proved—[reads evidence of Dr Brown]. The evidence shows that according to the opinion of two medical men, of the prisoner's brother, and the constable, the prisoner at that time laboured under an attack of acute mania, but that the lapse of about five hours brought that man down from this state of acute mania to a state of calmness and quietness. He was in mania till two o'clock, improving at four, and calm and rational soon after seven. This is of great importance. Again he had a similar attack in April 1864, though not so violent. On the 17th November preceding the fatal act, a fit of drinking began, and the evidence discloses a most melancholy picture of its continuance and its results—[reads evidence of Dr Turnbull].

On the whole evidence the facts which seem to me favourable to the defence of insanity are these:—The previous mania—the suicidal threats, so seriously made that his wife took away and hid the razors and knives—the fact that he was found by his wife on the Sunday, standing on a chair with a rope round his neck, the other end being fastened to the top of the bed, and you will remember that this rope was used in the murder, so that you have here something like a connexion between the suicidal threat and the homicidal act, which is important for your consideration. In addition to these there is the nature of the violence, strongly relied on by Mr M'Kenzie, who has urged that its excessive character, far beyond what was necessary, indicates a frantic state of mind. You will judge whether the fury of passion may not explain it. Then the washing up, to some extent, is said by the public prosecutor to be an evidence of conscious guilt, an observation which you will seriously consider; but, on the other hand, why should the man, who was not interrupted, partially wash up a bit of the floor, leaving pillows, clothes, and bed soaked with blood, and blood in other parts of the floor, and then give himself up to the Sheriff-officer, wearing his own bloody shirt as witness against him? These are circumstances more or less in favour of the plea. But, on the other hand, there are circumstances which strike against it. The prisoner states that the quarrel, or at least the serious part of it, began with a blow received by him from his wife, and his head bore marks of blood. This is quite as consistent with the rising of angry passion as with mad frenzy, and I rather think it would have been better for the plea of the prisoner, if it did not appear that the woman had struck him. The introduction of a natural human passion appears to me unfavourable to the plea of insanity. Again, the nature and extent of the prisoner's recollection is unfavourable. You heard the medical evidence on this point, and the evidence of what the prisoner said, not only at the time, but also in his declaration. You will judge whether the statements of the prisoner are such as to show that the man had or had not such a recollection of what passed as to be inconsistent with a state of insanity. The jug with which he struck was not the instrument with which he killed her, but he said to two of the witnesses that it was the only thing he used; and he was only brought to any further confession by the doctor saying that that was nonsense. Then he admitted more. It is for you to say whether that sort of memory is consistent with his having been in a state of acute mania—totally deprived of reason within a short time before.

The result of the medical evidence—of the testimony, as matter of opinion, of the medical gentlemen—is that an attack of acute mania during that night is highly probable. It is my painful duty to tell you that if, on the whole facts, you can go no further than that, you cannot acquit the prisoner on the ground of insanity. The burden of this proof of insanity lies upon him, and he must make it clear to you beyond a doubt. But if you, from all the evidence in the case, including the medical evidence, should come to a stronger conclusion, if you think that the whole evidence satisfies your minds, then the prisoner has proved his defence. You cannot find the defence proved if you do not go beyond this that insanity was highly probable. But you may find it proved if you think that the insanity of the prisoner at the date of the act is the true inference from the whole case. The verdict of "culpable homicide" is within your power; but I cannot support the suggestion of such a verdict in this case.

The jury then retired, and after a short absence returned a verdict of acquittal on the ground of insanity, in terms of the statute.

GENERAL COUNCIL OF MEDICAL EDUCATION AND REGISTRATION.

Royal College of Physicians, London.

Minutes of Meeting, *Monday, April* 10, 1865.

The Minutes of last meeting were read and confirmed.

1. Moved by Dr Andrew Wood, seconded by Dr Fleming, and agreed to,—"That the Council resolve itself into a Committee of Education, and that the President be requested to take the chair."

2. Moved by Mr Rumsey, and seconded by Mr Hargrave,—"That the observations and suggestions on the Report of the Select Committee on Education from the various Bodies, be printed for the use of the Council."

Amendment moved by Dr Corrigan, and seconded by Dr A. Smith,—"That the several communications from the several Licensing Bodies on the question of education and examination be accepted as read, and printed on the Minutes."

The amendment was carried; and having been put as a substantive motion, was also carried.

3. Moved by Dr Andrew Wood, and seconded by Dr Embleton,—"That a Committee be appointed to consider and report on what should be the minimum course of professional study through which all candidates should be required to pass before receiving any qualification entitling them to register." The motion was negatived.

Moved by Dr Corrigan, and seconded by Dr Sharpey,—"That it seems impossible to lay down any scheme of education and examination comprising details which would be applicable to, or could be uniformly carried out by, all the licensing bodies enumerated in the Medical Act; and that this Council is of opinion that the Committee on Education, leaving all details to be carried out in such a manner as may appear fit to them by the several licensing bodies, should confine itself, in considering the question of education, to the following points, viz.: "1. Registration and adequate preliminary examination in arts. 2. The time to be interposed between the passing of the preliminary examination and the final examination. 3. The mode of subdivision of the examination, the period of study at which each part of the examination shall be gone through, and the subjects to be comprised in each part."

Amendment moved by Dr Quain, and seconded by Mr Hargrave,—"That the report of the Select Committee on education of the last session be now proceeded with."

The amendment was carried; and having been put as a substantive motion, was also carried.

5. Moved by Dr A. Smith, seconded by Dr Fleming, and agreed to,—"*a.* That the registration of medical students be placed under the charge of the Branch Registrars."

Moved by Dr A. Smith, seconded by Mr Hargrave,—"That every student be registered at the commencement of professional study, the date of such registration to be considered as the date of commencement of professional studies."

Amendment moved by Mr Syme, and seconded by Dr Sharpey,—"*b.* That every medical student shall be registered at the commencement of his professional study; and not until he has passed the preliminary examination."

The amendment was carried, and having been put as a substantive motion was also carried.

Moved by Dr A. Smith, and seconded by Dr Alderson,—"That each of the Branch Registrars shall open a register of medical students from the 1st April to the 15th of May, and from the 1st of October to the 1st of December in each year.

Amendment moved by Mr Rumsey, and seconded by Mr Hargrave,—"*c.* That after the words 'medical students,' the resolution read as follows: 'and that application for registration be made by every such student within fifteen days after the commencement of professional study; and that the table be adopted, with addition of a column for place of study.'"

The amendment was carried; and having been put as a substantive motion, was also carried.

Moved by Dr A. Smith, and seconded by Dr Apjohn,—"*d.* That every person desirous of being registered as a medical student shall apply (in writing) to the Branch Registrar of the division of the United Kingdom in which he is residing, according to a form to be had on application, and shall produce or forward to him a certificate of his having passed a preliminary examination in arts recognised by the General Medical Council, and of his place of study; whereupon the said Branch Registrar shall enter his name and other particulars in the students' register, and the Registrar shall give him a certificate of such registration accordingly."

Amendment moved by Dr Fleming, and seconded by Mr Rumsey,—"That after the words, 'General Medical Council,' the resolution read 'and of his having entered on medical study.'"

The amendment was negatived. The motion was then put to the vote, and carried.

Moved by Dr A. Smith, seconded by Dr Corrigan, and agreed to,—"*e.* That each of the Branch Registrars shall supply to the several qualifying bodies, medical schools, and hospitals in that part of the United Kingdom of which he is Registrar, a sufficient number of blank forms of application for the registration of medical students."

Moved by Dr A. Smith, seconded by Dr Corrigan, and agreed to,—"*f.* That a copy of the register of medical students so prepared by the Branch Registrars be transmitted to the Registrar of the General Council, who shall, under direction of the Executive Committee, prepare and print an alphabetical list of all registered students, and supply a copy of such authorized list to each of the bodies enumerated in Schedule (A) to the Medical Act."

Moved by Dr A. Smith, seconded by Dr Corrigan, and agreed to,—"*g.* That the several licensing bodies be requested not to admit to examination, after October 1869, any candidate for license or degree whose name does not appear on the authorized list of medical students, or whose name is not already on the *Medical Register*."

Moved by Dr A. Smith, seconded by Dr Corrigan, and agreed to,—"*h*. That the several Branch Councils shall have power to admit special exceptions to the foregoing regulations as to registration, for reasons which shall appear to them satisfactory."

Moved by Dr Andrew Wood, seconded by Dr Fleming, and agreed to,—"*i*. That the Branch Councils be desired to take means to make these regulations known to the medical students at the various medical schools."

The Council resumed.

6. Moved by Dr Embleton, seconded by Dr Andrew Wood, and agreed to,—"That the returns from the licensing bodies, in compliance with Recommendation 23 of the Committee on Education, and the registers of medical students in England and Ireland, be referred to a committee; the committee to consist of—Dr Embleton, *Chairman;* Mr Cooper, Dr Thomson, Dr Leet, Dr Apjohn, Dr Stokes"

MINUTES OF MEETING, *Tuesday*, 11*th April* 1865.

The Minutes of the last meeting were read and confirmed.

I. Moved by Dr Andrew Wood, seconded by Mr Hargrave, and agreed to,—"That the Council resolve itself into a General Committee on Education, and that the President be requested to take the chair."

The consideration of the Report of the Select Committee on Education was resumed.

SECTION II. OF THE REPORT.

Age for License to Practise.

2. Moved by Mr Syme, and seconded by Dr Paget,—"That the age of twenty-one be the earliest age at which a candidate for any professional license shall be admitted to his final examination; and that the age shall, in all instances, be duly certified; and that a return of any exceptions to this recommendation allowed by the licensing bodies, together with the reasons for such exceptions, be transmitted to the Branch Council of that part of the United Kingdom in which they have been granted."

Amendment moved by Dr Apjohn, and seconded by Dr Corrigan,—"That the age of twenty-one be the earliest age at which any professional license shall be obtained; and that the age shall, in all instances, be duly certified."

The amendment was negatived.

The original motion was then put to the vote and carried.

Mr Syme required that the numbers of the majority and minority be taken down. Majority, 14; minority, 6.

SECTION III.—PROFESSIONAL STUDY.

3. Moved by Dr Andrew Wood, and seconded by Dr Fleming,—"(1.) That the first clause of Section III. be adopted, viz.,—'That no license be obtained at an earlier period than after the close of the last winter session of the fourth year of study, after the registration of the candidate as a medical student.'"

Amendment moved by Dr Paget, and seconded by Mr Arnott,—"That no license be obtained at an earlier period than after the expiration of forty-eight months subsequent to the registration of the candidate as a medical student."

The amendment was carried; and having been put as a substantive motion, was also carried.

Moved by Mr Hargrave, and seconded by Dr Paget,—(2.) "That Clause 2 of Section III. be adopted, viz.,—'That the course of study required for a license shall comprehend attendance during not less than four winter sessions, or three winter and two summer sessions, at a recognised medical school; and that evidence shall be produced that the remaining period of the four years has been passed in the acquisition of professional knowledge.'"

Amendment moved by Dr Embleton, and seconded by Dr Andrew Wood,—

"That the Committee, before proceeding further, determine what are the requisites for constituting a medical school under their recommendations."

The amendment was negatived.

Amendment moved by Dr Christison, and seconded by Dr Parkes,—"That the course of professional study required for a license shall comprehend attendance during not less than four winter sessions, or three winter and two summer sessions, at a school recognised by any of the licensing bodies mentioned in Schedule (A) to the Medical Act."

The amendment was carried; and having been put as a substantive motion was also carried.

Moved by Dr Parkes, seconded by Dr Storrar, and agreed to,—(3.) "That Clause 3 of Section III. be omitted."

Moved by Mr Syme, seconded by Dr Storrar, and agreed to,—"That Clause 4 of Section III. be adopted, viz.,—(4.) 'That it be recommended to the several licensing bodies that the courses of instruction required by them should be framed in such a manner as to secure a due share of attention, both to preparatory branches, and to those more strictly connected with the Practice of Medicine and Surgery; and that it be suggested accordingly to these Bodies, that their regulations should be such as to prevent attendance upon lectures from interfering with hospital and clinical study.'"

Moved by Dr Thomson, and seconded by Mr Hargrave,—(5.) "That Clause 5 of Section III., on professional study, be omitted."

Amendment moved by Mr Syme, and seconded by Dr A. Smith,—"That Clause 5 of Section III. be adopted."

The amendment was negatived.

The original motion was then put to the vote and carried.

Moved by Dr Storrar, and seconded by Dr Embleton,—(6.) "That the Council will view with approbation any encouragement held out by the licensing bodies to students to prosecute the study of the Natural Sciences before they engage in studies of a strictly professional character."

Amendment moved by Dr Apjohn, and seconded by Dr Corrigan,—"That Clause 6 of the Recommendations relating to professional examination be omitted."

The amendment was negatived.

The original motion was then put to the vote and carried.

Moved by Dr Andrew Wood, seconded by Mr Hargrave, and agreed to,—"That the Council now resume."

4. A document, declared by the solicitor to be a regular certificate of the conviction, at Castlebar, County Mayo, of John Carter Barrett, of felony, was submitted to the Council.

Moved by Dr A. Smith, seconded by Dr Stokes, and agreed to,—"That the name of John Carter Barrett be erased from the Medical Register."

MINUTES OF MEETING, *Wednesday*, 12*th April* 1865.

The minutes were read and confirmed.

The Council then resolved itself into a General Committee on Education, and resumed the consideration of the Report of the Select Committee.

Dr Stokes moved the omission of Clause VII. of the Report on Professional Education,—"That the several licensing bodies be requested to furnish a short statement of the mode in which their examinations are now conducted,—whether by written, oral, or practical examination; and of the length of time a candidate is under examination in each or all of these ways."

Mr Syme seconded the motion.

Dr Parkes moved as an amendment,—"That those licensing bodies which have not already done so, be requested to furnish a statement of the dates of their examinations, and of the modes in which such examinations are conducted,—whether by written, oral, or practical examination; and of the length of time a candidate is under examination in each or all of these ways. And that the Registrar transmit these reports to the members of the Council, in order that they may be taken into consideration at the next meeting of the several Branch Councils."

The amendment was put to the Council and carried, fourteen voting for it and four against it. It was then put as a substantive motion, and carried.

EXAMINATIONS.

Mr Syme moved, "That the first clause of the recommendations be adopted:—'That the licensing boards be advised to encourage the periodical examination of students at their several classes.'"

Dr Andrew Wood seconded the motion.

Dr Paget proposed the following amendment, which, he said, would reconcile the difficulties in some degree: "That, while acknowledging the great educational value of the periodical class-examination of students, the Council decline to issue advice or instruction upon the subject."

Dr Thomson seconded the amendment.

The amendment was then put to the Council and lost, six voting for it and twelve against it.

The motion, as amended, substituting "the" for "their," was then put to the Council and lost, seven voting for it and twelve against it.

Dr Wood asked that the names might be taken down.

Majority,—Dr Alderson, Mr Arnott, Mr Cooper, Dr Acland, Dr Paget, Dr Thomson, Dr Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Corrigan, and Dr Stokes.

Minority,—Dr Embleton, Dr Wood, Dr Fleming, Mr Syme, Dr Sharpey, Dr Parkes, and Dr Christison.

Declined voting,—The President, Dr Storrar, and Dr Quain.

Clause I. of Section IV. (Examinations) was consequently omitted.

Mr Hargrave moved the omission of Clause II.,—"That the final examinations of the licensing boards be so carried on as to be an efficient test of the practical acquaintance of candidates with the several branches of medical knowledge, and especially with the practice of Medicine and Surgery."

Dr Storrar seconded the motion.

Dr Andrew Wood moved, as an amendment, that Clause II. should stand.

Dr Parkes seconded the amendment.

The amendment was negatived, seven voting for and eleven against it.

The motion was then carried, eleven voting for and seven against it.

Clause II. was therefore omitted.

On Clause III.—"That the professional examination for any license be divided into two parts; the first embracing the primary or fundamental branches of Medicine, and the second the branches directly connected with the Practice of Medicine and Surgery; that the former be not undergone till after the close of the winter session of the second year of professional study, and the latter, or final examination, not till after the close of the winter session of the fourth year of such study."

Mr Hargrave proposed—"As the Council has decided the important questions of registration, age for license to practise, and professional study, that the rest of the clauses on education be postponed until the next session."

The motion was not seconded.

Dr Fleming moved as an amendment, "That Clause III. of the report be adopted as thus amended,—viz.: 'That the professional examination for any license be divided into two parts; the first embracing the primary or fundamental branches of Medicine, and the second the branches directly connected with the Practice of Medicine and Surgery; that the former be not undergone till after the close of the winter session of the second year of professional study, and the latter, or final examination, not till after the close of the prescribed period of professional study.'"

Mr Arnott seconded the amendment.

The motion was withdrawn and Dr Fleming's amendment stood as the substantive motion.

It was put to the Council and carried *nem. con.*

On Clause IV.,—"That the examination in Chemistry, Botany, and Natural History may be undergone at an earlier period."

Dr Storrar said the word "the" implied that there must be an examination in natural history. He proposed that the clause be adopted with the omission of the word "the."

Dr Sharpey seconded the motion.

Dr Acland suggested that the word "Physics" should be added, making it, "examination in Physics, Chemistry, Botany, and Natural History."

Dr Storrar said in the scheme suggested by the committee they contemplated the introduction of Physics into the preliminary examination.

Dr Apjohn was opposed to the examination in Chemistry being placed so early. It would require at least two years before a student could pass the examination.

Mr Syme thought that chemical studies should be commenced early, but the students should not undergo examination till a later period in the course of study.

Dr Acland proposed, as an amendment, that the word "Physics" should be added.

Dr Storrar was quite willing to insert the word in the clause as proposed.

The amendment was then withdrawn.

Dr Wood proposed, as an amendment, that Clause IV. be adopted, omitting the word "Chemistry" and adding the word "Physics." He said he considered Chemistry to be the fundamental study in the course of a professional career, and it would be an injury to the students if it was thought they could attain to even a modicum of Chemistry before they began their professional study.

Dr Sharpey said Dr Andrew Wood objected to encouraging students to pass a Chemistry examination before they commenced medical studies. The motion was not to that effect at all: it was that they might be examined in Chemistry before the first professional examination, and not before the commencement of professional study.

Dr Wood said his amendment would stand thus,—"That Clause IV. stand as follows: 'That the examination in Physics, Botany, and Natural History may be undergone at an earlier period than the first professional examination.'"

Dr Apjohn seconded the amendment. He was very much in favour of excluding Chemistry altogether from the resolution.

Dr Thomson thought they might say, "examinations in Elementary Chemistry;" and then an examination upon the application of Chemistry to Medicine would be afterwards undergone. He was quite satisfied that it was impossible for a student to obtain from the course of General Chemistry the sort of information which was most required by him. It would be much more advantageous for medical students to study a short course of Elementary Chemistry, and to pursue the study afterwards.

Dr Acland thought it would be a positive hardship on the students to attempt to lay down a sweeping clause that persons who had ability, energy, and industry beyond the average of medical men in certain parts of scientific study should be debarred from choosing the time for passing examinations in those studies.

The amendment was put to the Council and carried, eleven voting for it and seven against it.

The amendment then stood as the substantive motion.

Dr Thomson moved as an amendment,—"That the words 'Elementary Chemistry' be inserted."

Dr Paget seconded the amendment. He thought that if the student were required to wait till the second year before being allowed to pass an examination in Elementary Chemistry, it would prove a great discouragement to the study of Chemistry throughout the country as a subject of general education.

Dr Apjohn objected to the proposition on the ground that it involved as a matter of necessity a second examination.

Dr Christison asked if it was Dr Thomson's intention to move that there should be a subsequent examination in advanced Chemistry.

Dr Thomson said he would leave that to the Council.

Dr Thomson's amendment was then put and lost, seven voting in its favour and nine against it.

The motion was then carried.

On the motion of Dr Andrew Wood, the Council resumed for the purpose of receiving reports from the Committee on General Education and the Committee on the Amendment of the Medical Acts. These reports were read, and ordered to be entered on the minutes.

The Council resolved itself into a Committee on Education, and proceeded with the consideration of the report of the Select Committee.

Dr Storrar moved the adoption of Clause v. of Section iv., relative to examinations,—"That the professional examinations be conducted both in writing and orally, and that they be practical in all branches in which they admit of being so."

Dr Sharpey seconded the motion, which was carried.

The following clauses were then successively adopted without discussion:—

"That the professional examinations be held by the several licensing bodies, except in special cases, at stated periods, to be publicly notified."

"That returns from the licensing bodies in Schedule (A) be made annually, on the 1st of January, and in the subjoined form, to the General Medical Council, stating the number and names of the candidates who have passed their first as well as their second examinations, and the number of those who have been rejected at the first and second examinations respectively; and that the Registrar forward a sufficient number of forms, with a notice for their being returned, in due time."

The Council then adjourned.

Minutes of Meeting, *Thursday, 13th April* 1865.

The minutes of the previous meeting were read and confirmed.

Dr Andrew Wood brought up the report of the Committee on Amendment of the Medical Acts. The report stated that the Committee had taken into consideration all the important clauses of that Act, and that Mr Ouvry, the Solicitor of the Council, had prepared, in accordance with the resolutions of the Committee, the following draft of an amended Bill.

Draft of Bill to Amend the Medical Act of 1858.

Whereas the Medical Act, 1858, has been found ineffectual to enable persons requiring medical aid to ascertain who are qualified practitioners, and it is desirable to amend the said Act in several respects:

Be it therefore enacted by the Queen's most excellent Majesty, by and with the advice and consent of the Lords spiritual and temporal and Commons in the present Parliment assembled, and by the authority of the same, as follows:—

1. This act may for all purposes be cited as "The Medical Amendment Act, 1865."

2. This Act shall take effect from the passing thereof.

3. The following sections of the Medical Act, 1858, shall be and the same are hereby repealed,—viz., Sect. 14 and Sect. 40, but without prejudice to any proceedings which may be pending under such sections or any of them.

4. It shall be the duty of the Registrars to keep their respective registers correct, in accordance with the provisions of this Act and the orders and regulations of the General Council, and to erase the names of all registered persons who shall have died, or who, having ceased to practise, shall desire to have their names removed from the Register, and from time to time to

register all alterations in the addresses or qualifications of the persons registered under the Medical Act, 1858. The registrars, in these respects, may act upon such evidence as may appear to them sufficient, subject to any rules or regulations which the General Council may make thereon. And, for the purposes aforesaid, the registrar may write a letter to any registered medical practitioner, addressed to him according to his registered address, to inquire whether he has ceased to practise or has changed his residence; and if the answer to such letter be that the person has ceased to practise and desires his name to be removed from the Register, or if no answer be returned to such letter within the period of six months from the sending of the letter, it shall be lawful to erase the name of such person from the Register. Provided always that any name which may be erased by the registrars may be restored by the direction of the General Council. No person whose name has been erased from the Register by order of the General Council or any Branch Council shall be re-registered, either on the original or any new qualification, except by the direction of the General Council or Branch Council, as the case may be.

5. Any person practising Medicine or Surgery, or being engaged in the treatment of diseases or injuries, not being registered under this Act, who shall take or make use of any of the titles or designations enumerated in Schedule A to this Act, or that of Physician, Surgeon, Doctor, Professor of Medicine, Professor of Surgery, or any other title, name, or designation used by or used to distinguish duly qualified practitioners in Medicine or Surgery, shall, upon a summary conviction, be liable to a penalty not exceeding £20 for each offence.

6. The words "qualified to be," in the 7th section of the Medical Act, 1858, shall be and the same are hereby repealed.

7. It shall, notwithstanding anything in the Medical Act, 1858, contained, be lawful for Her Majesty, by charter, to grant to the Royal College of Surgeons of Edinburgh, the Royal College of Surgeons in Ireland, and the Faculty of Physicians and Surgeons of Glasgow, or any or either of them, power to institute and hold examinations for the purpose of testing the fitness of persons to practise as dentists who may be desirous of being so examined, and to grant certificates of such fitness.

The committee call attention to the changes in the present Medical Act which are proposed in the Draft Amended Bill:—

Preamble.—The preamble to the Act as it at present stands says: "Whereas it is expedient that persons requiring medical aid should be enabled to distinguish qualified from unqualified practitioners."

[After explaining the reasons which induced the Committee to adopt the various alterations proposed, the report went on to state:—]

In reference to the various questions specially referred to them by the Council, the Committee have to report:

In reference to the amending of Sections 15 and 31, with the view of securing that all persons to be placed on the Register should have been duly educated, tested, and qualified in all banches of the profession, whether medical or surgical, the Committee are of opinion that it is highly desirable that every medical practitioner should have a complete or double qualification. They see difficulties, however, in the way, for the present, of framing compulsory clauses which would secure that important object. They would, however, point out that by Section 19 of the Medical Act, which is a permissory section, powers have been provided by which any two or more of the licensing bodies may unite and co-operate in conducting examinations required for qualifications to be registered under the Act. Were this course carried out in London and in Dublin, as it has already been successfully carried out in Scotland, the Committee believe that in this way, without the necessity of compulsory legislation, the great object would be effected of combining and concentrating the licensing bodies, to the relief of the student, and the advantage of the profession and the public.

It has been proposed to amend Section 18 by adding to it the following words, or words to the same effect,—viz.:

"And may also inspect the written answers of the candidates, and report concerning the examinations and answers to the General Council; and to the persons deputed by the General Council as aforesaid, in such numbers as may be determined by the General Council, with the approval of one of Her Majesty's principal Secretaries of State, there shall be paid such fees for services and such reasonable travelling expenses as shall from time to time be allowed by the General Council and approved by the Commissioners of Her Majesty's Treasury; and the said payments shall be made out of the residues of the moneys annually received for carrying this Act into execution, after defraying the expenses of the General Council and the Branch Councils, and, if necessary, out of further moneys to be provided for the said purpose by vote of Parliament."

The Committee do not consider this amendment either necessary or expedient. The Branch Councils have the power under the Act as at present to visit the examinations of the licensing bodies, or to depute any person or persons to do so; they have also the power, as the Committee are advised by Mr Ouvry, to remunerate the visiters of the examinations from the funds contributed under the Act. The Committee consider that it would be inexpedient to go to Parliament to ask a supplementray money grant for the purpose. They trust that erelong the expense of the meetings of the Council will be diminished, and that there will be sufficient funds to meet the expenses of visitation in any form that may be deemed necessary.

The Committee, after considering the penal sections of the Act, do not recommend that the application should be made to Parliament for stronger powers to be given to magistrates administering the Act, or for higher penalties. The Committee believe that if the clause proposed in lieu of Section 40 prove effective, the present powers and penalties will be found adequate.

In regard to the question of the registration of licentiates in dental surgery, the Committee, having given that consideration to the memorial of the dental surgeons to which it is justly entitled, are of opinion that it is inexpedient to place upon the Medical Register dental practitioners not otherwise qualified; holding as they do that that would constitute a further obstacle, in addition to those at present existing, to the securing that all persons whose names shall be inserted in the Medical Register shall be fully qualified to practise all branches of their profession.

The Committee, in conclusion, suggested a form of memorial to be addressed by the president, in the name of the Council, to the Home Secretary, requesting his attention to the proposed alterations, and asking for an interview at an early period.

Dr Andrew Wood moved the adoption of the report.

Dr Embleton seconded the motion.

Dr Corrigan moved as an amendment,—"That the consideration of the report on the proposed amendments of the Medical Act be postponed till the next meeting of this Council ("Oh! oh!"), inasmuch as the resolution of this Council of May 4, 1864, directed the attention of the Branch Councils only to Clauses 20, 31, and 40, while in the amended Bill now proposed for adoption, amendments are introduced which have never previously come under the consideration of this Council, and no opportunity is afforded to the several licensing bodies to take these ammendments into consideration."

Dr Smith seconded the amendment, which was put to the Council and negatived, three voting in its favour and thirteen against it.

On the motion of Dr Quain, seconded by Dr Apjohn, it was resolved—"That the report of the committee be considered clause by clause, except such clauses as have been already decided on by the Council."

The preamble of the amended Act and the first three clauses were successively adopted without discussion.

Dr Wood moved, and Dr Quain seconded, the adoption of the 4th clause in lieu of Section 14.

The President suggested that the registrar should be required to send two letters instead of one, within the prescribed period of six months, before erasing any name from the Register.

The proposal was assented to, and the clause was adopted with the suggested alteration.

Clause 5 of the amended Act, having been previously adopted by the Council, was not made the subject of any further motion. Clause 6 was adopted without discussion.

With reference to Clause 7, Dr Wood said he was quite willing, if Dr Corrigan still objected to it, to withdraw it from the amended Bill. The clause was accordingly withdrawn.

The report of the Committee was then adopted, with the exception of that portion relating to the granting of dental diplomas, and the latter part of the clause in reference to the double qualification, commencing with the words, "were this course carried out in London and in Dublin," &c.

MINUTES OF MEETING, *Saturday*, 15*th April* 1865.

The Minutes of the last meeting were read and confirmed.

1. The President informed the Council that the deputation appointed at the last meeting of the Council had waited upon the Under Secretary of State for the Home Department, and found that he had given his close attention to the Report adopted by the Council on the Amendment of the Medical Acts, including the Draft Bill; and, after some further explanations, he promised to forward immediately the Report, with the Memorial signed by the President, to the Secretary of State, Sir George Grey.

2. The Report of the Pharmacopœia Committee was considered.

REPORT.

The Pharmacopœia Committee beg to report that, after much careful consideration, they requested Mr Warrington, of the Apothecaries' Hall, and Dr Redwood, of the Pharmaceutical Society, to undertake the preparation of the next edition of the "Pharmacopœia," under the supervision of the Committee. The gentlemen named accepted the duty, and they are engaged actively in its performance. The Committee, in the first instance, prepared an outline of the subjects which seemed to them to require revision; and these subjects are made the basis of careful inquiries by Messrs Warrington and Redwood, who have submitted, and will continue to submit, their reports thereon, together with such suggestions as they think proper to make for the consideration and decision of the Committee. The Committee have also received valuable assistance from Dr Farre, appointed by the English Branch Council, from Dr Moore, appointed by the Irish Branch Council, and from Dr Christison, appointed by the Scottish Branch Council, to report on the improvement in the progress of Pharmacy; and they hope that the services of these gentlemen may be continued.

Taking the first edition of the "Pharmacopœia" as a basis, compiled, as it has been, with great labour and expense, the Committee hope that, without making any very extensive or fundamental changes, the next edition will be found acceptable to the Profession.

GEORGE BURROWS, M.D., *Chairman*.

3*d April* 1865.

Moved by Dr Quain, seconded by Dr Apjohn, and agreed to,—"That the Report of the Pharmacopœia Committee be adopted."

Moved by Dr Corrigan, seconded by Dr Andrew Wood, and agreed to,—"That it is desirable to have a proof copy of the new 'Pharmacopœia' in the hands of the Members of the General Medical Council at least one month before the meeting of the General Medical Council at which the opinion of the Medical Council is to be given relative to its being published, in order to afford to each member of Council the opportunity of making such suggestions to the Committee as may appear desirable."

Moved by Dr Quain, seconded by Mr Hargrave, and agreed to,—"That the Executive Committee be authorized to obtain estimates, and make contracts for printing and binding the new edition of the Pharmacopœia, when the same is ready for the press."

Moved by Dr Quain, seconded by Mr Cooper, and agreed to,—"That the sum ordered to be placed at the disposal of the Pharmacopœia Committee by resolution of 7th May 1864, be extended from £100 to £300."

5. The Report of the Committee on the subjects of General Education was considered.

REPORT.

The Committee on the subjects of General Education having duly considered the subject remitted to them, resolve to recommend to the Council—

That after the year 1867, the examinations in subjects of General Education be left entirely to the national educational bodies recognised by the Medical Council,—the Council confining its regulations on General Education to publishing a list of the Examinations which may from time to time be approved of by them, it being understood that no certificate which does not affirm the proficiency of the candidate in Latin, and also his knowledge of the elements of Geometry, and of the elements of Mechanics and Hydrostatics, be accepted.

W. STOKES, *Chairman.*

7. Moved by Dr Embleton, seconded by Dr Leet, and agreed to,—"That the Report of the Committee appointed to arrange and digest the Recommendations as to Education and Examination agreed on during the present session by the General Committee on Education be received, and entered on the Minutes."

REPORT.

Recommendations of the General Council of Medical Education and Registration of the United Kingdom, on the subjects of Preliminary Education, of Registration of Medical Students, and of Professional Education and Examination, of 15*th April* 1865.

I.—PRELIMINARY EXAMINATION.

That testimonials of proficiency granted by the national educational bodies according to the subjoined list, may be accepted, the Council reserving the right to add to, or take from, the list.

1. A degree in Arts of any University of the United Kingdom, or of the Colonies, or of such other Universities, as may be specially recognised from time to time by the Medical Council.
2. Oxford Responsions or Moderations.
3. Cambridge Previous Examinations.
4. Matriculation Examination of the University of London.
5. Oxford Middle Class Examinations (Senior).
6. Cambridge Middle Class Examinations (Senior).
7. Durham Middle Class Examinations (Senior).
8. Durham Examinations for Students in Arts, in their second and first years.
9. Durham Registration Examination for Medical Students.
10. Dublin University Entrance Examination.
11. Queen's University, Ireland, two years' Arts course for the diploma of licentiate in Arts.
12. Preliminary Examinations at the end of A.B. course.
13. Middle Class Examinations.
14. Matriculation Examinations.
15. First Class Certificate of the College of Preceptors.
16. "Testamur" granted by Codrington College, Barbadoes.
17. Degree of Associate of Arts granted by the Tasmanian Council of Education, with a certificate that the student has been examined in Latin and Mathematics.

II.—REGISTRATION OF MEDICAL STUDENTS.

1. That the registration of medical students be placed under the charge of the Branch Registrars.

2. That every medical student shall be registered at the commencement of his professional study; and not until he has passed the preliminary examination.

3. That each of the Branch Registrars shall open a Register of Medical Students, according to a specified form; and that application for registration be made by every such student within fifteen days after the commencement of professional study.

4. That every person desirous of being registered as a medical student, shall apply (in writing) to the Branch Registrar of the division of the United Kingdom in which he is residing, according to a form to be had on application to the several qualifying bodies, medical schools, and hospitals, and shall produce or forward to the Branch Registrar a certificate of his having passed a preliminary examination in Arts recognised by the General Medical Council, and of his place of study: whereupon the said Branch Registrar shall enter the said applicant's name and other particulars in the Students' Register, and the Registrar shall give him a certificate of such registration accordingly; and that each of the Branch Registrars shall supply to the several qualifying bodies, medical schools, and hospitals in that part of the United Kingdom of which he is Registrar, a sufficient number of blank forms of application for the registration of medical students.

5. That a copy of the Register of Medical Students so prepared by the Branch Registrars be transmitted to the Registrar of the General Council, who shall, under direction of the Executive Committee, prepare and print an Alphabetical List of all Registered Students, and supply a copy of such authorized list to each of the bodies enumerated in Schedule (A) to the Medical Act.

6. That the several licensing bodies be requested not to admit to examination, after October 1869, any candidate for license or degree whose name does not appear on the authorized List of Medical Students, or whose name is not already on the *Medical Register*.

7. That the several Branch Councils shall have power to admit special exceptions to the foregoing regulations as to registration, for resaons which shall appear to them satisfactory.

8. That the Branch Councils be desired to take means to make these regulations known to the medical students at the various medical schools.

III.—PROFESSIONAL EDUCATION.

1. That the course of professional study required for a license shall comprehend attendance during not less than four winter sessions, or three winter and two summer sessions, at a school recognised by any of the licensing bodies mentioned in Schedule (A) to the Medical Act.

2. That it be recommended to the several licensing bodies that the courses of instruction required by them be framed in such a manner as to secure a due share of attention, both to preparatory branches and to those more strictly connected with the Practice of Medicine and Surgery; and that it be suggested accordingly to these bodies, that their regulations should be such as to prevent attendance upon lectures from interfering with hospital and clinical study.

3. That the Council will view with approbation any encouragement held out by the licensing bodies to students to prosecute the study of the natural sciences before they engage in studies of a strictly professional character.

IV.—EXAMINATION.

1. That those licensing bodies which have not already done so, be requested to furnish a statement of the dates of their examinations and of the modes in which such examinations are conducted, whether by written, oral, or practical

examination, and of the length of time a candidate is under examination in each or all of these ways; and that the Registrar transmit these reports to the members of the Council, in order that they may be taken into consideration at the next meeting of the several Branch Councils.

2. That the professional examination for any license be divided into two parts; the first embracing the primary or fundamental branches of Medicine; and the second the branches directly connected with the Practice of Medicine and Surgery; that the former be not undergone till after the close of the winter session of the second year of professional study; and the latter, or final examination, not till after the close of the prescribed period of professional study.

3. That the examination in Physics, Botany, and Natural History may be undergone at an earlier period than the first professional examination.

4. That the professional examinations be conducted both in writing and orally; and that they be practical in all branches in which they admit of being so.

5. That the professional examinations be held by the several licensing bodies, except in special cases, at stated periods, to be publicly notified.

6. That returns from the licensing bodies in Schedule (A) be made annually, on the 1st of January, to the General Medical Council, stating the number and names of the candidates who have passed their first as well as their second examinations, and the number of those who have been rejected at the first and second examinations respectively; and that the Registrar forward a sufficient number of forms, with a notice for their being returned in due time.

V.—SUPERVISION OF EXAMINATIONS.

That each of the Branch Councils, or such of their members as may be deputed by such Councils, shall, from time to time, visit the examinations, preliminary as well as professional, conducted by the qualifying bodies in their respective divisions of the United Kingdom, and report the results of their observations to the General Council.

VI.—AGE FOR LICENSE TO PRACTISE.

1. That the age of twenty-one be the earliest age at which a candidate for any professional license shall be admitted to his final examination; and that the age shall, in all instances, be duly certified; and that a return of any exceptions to this recommendation allowed by the licensing bodies, together with the reasons for such exceptions, be transmitted to the Branch Council of that part of the United Kingdom in which they have been granted.

2. That no license be obtained at an earlier period than after the expiration of forty-eight months subsequent to the registration of the candidate as a medical student.

8. Moved by Dr Andrew Wood, seconded by Dr Embleton, and agreed to, —"That the Council now proceed to ballot for the members of the Executive Committee."

A ballot having been taken, Mr Arnott, Dr Acland, Dr Paget, and Dr Sharpey, were found to be elected.

MINUTES OF MEETING, *Monday, April* 17, 1865.

The minutes of last meeting were read and confirmed.

The following Report with regard to the two bills relating to Pharmacy now before Parliament was, after some discussion, agreed to:—

REPORT.

The Committee appointed on April 7, 1865, to report whether the Medical Council is charged under the Medical Act with any duty in relation to medical and surgical practice by chemists and druggists, and also to consider and report on the two bills relating to Pharmacy now before Parliament, report as follows:—

In 1864 the General Medical Council represented to Her Majesty's Government the necessity of regulating by statute the practice of pharmacy by chemists and druggists throughout the kingdom. The Committee are of opinion that this necessity continues as cogent as ever; and that the Council ought to encourage and support any approved measure for effecting such legislation.

Two bills for the purpose have been introduced into the House of Commons during the present session, one promoted by the Pharmaceutical Society, the other by chemists and druggists not belonging to that body. The bill of the latter is confined to England and Wales, that of the former to Great Britain.

After carefully considering both bills, the Committee are of opinion that the preferable mode of legislation is that which adopts the Pharmaceutical Society with the Pharmacy Act of 1862 as a basis. They think the bill promoted by the Society well fitted to attain various important objects, and reasonable in its demands for powers and privileges.

The main objects of the bill are to form a register of legally qualified pharmaceutic chemists; to prohibit the use of certain pharmaceutic titles by persons not on the Register; to confine to those registered the privilege of executing the prescriptions of medical practitioners, subject to the provision hereinafter named; but not to restrict the sale of medicines asked for in any other manner.

The Committee desire to bring before the Council certain defects which it appears to them necessary to correct before the bill becomes law.

1. The bill should be altered so as to apply to Ireland, as well as to England and Scotland. They are not aware that any state of things exists in Ireland to render the regulation of pharmacy by the state less necessary there than in Britain.

2. The Committee are of opinion that a clause should be inserted in any pharmacy bill, rendering it imperative on chemists and druggists to follow, in compounding prescriptions, the formularies of the British Pharmacopœia, unless otherwise directed by the prescriber.

3. The Committee consider that the promoters of the bill, probably from a desire to disarm opposition, propose to admit, on too easy terms, into their Society, pharmaceutical chemists and druggists not now belonging to it. The proposal is to admit all who offer themselves for examination, or who produce a certificate from a qualified medical practitioner that they have been in the practice of dispensing medicines from the prescriptions of medical men before January 1, 1866. The latter alternative implies a facility of entrance which will be apt to lead to abuse. The Committee are of opinion that more satisfactory evidence of qualification should be required.

4. The last important defect in the bill which the Committee have to notice is that no adequate provision has been made towards preventing registered pharmaceutic chemists from converting themselves into unqualified medical practitioners.

Looking to the history of medical practice in this country, the Committee see great danger to the interests of the public and of the medical profession from the body which will be constituted by the bill, should it become an Act in its present shape. The General Medical Council in carrying out the objects of the Medical Act have raised, and, it is hoped, may further raise, the qualifications of legally qualified medical practitioners. But their labours will be in vain should the creation of a new race of unqualified practitioners be inadvertently encouraged by an Act of Parliament. It is well known that many existing chemists and druggists, both members of the Pharmaceutical Society and others, practise medicine although unqualified by law and not competent by education. To a limited extent this practice may be inevitable and at all events cannot be prevented. But the existence of it gives peculiar facilities and temptations to the pharmaceutic chemist to embark largely in irregular medical practice as an unqualified practitioner.

The Committee have considered whether the danger here indicated might be averted by extending the jurisdiction of the General Medical Council, so as to include control over pharmaceutic chemists as well as over practitioners in medicine. But they are of opinion that such a plan is at present attended with difficulties.

By Clause lv. of the Medical Act, chemists and druggists are expressly exempted from the provisions of the Act, so far as the "selling, compounding, and dispensing medicines" is concerned. Nor is there any provision in the Act which gives the Medical Council any greater power to prevent chemists and druggists from practising medicine than the Act enables the Council to exercise over all other unqualified persons. It is plain, therefore, that the Act did not contemplate the exercise by the Medical Council of any control over chemists and druggists; and the Committee consider that it would be unwise to seek to alter the existing relations between the Medical Council and chemists and druggists.

The Committee have further considered whether the danger they have pointed out might be averted by some simple provision in the pharmacy bill. By Section xvii. of the bill of the Pharmaceutical Society, it is declared that—"Nothing in this Act contained shall extend, or be construed to extend, to lessen or prejudice, or in anywise to interfere with, any of the rights, privileges, and immunities heretofore vested in, and exercised and enjoyed by any duly qualified medical practitioner."

This Clause sufficiently protects medical practitioners in such right of practising pharmacy as they have hitherto enjoyed, but it does not attempt to prevent pharmaceutic chemists from practising Medicine. Considering their peculiar temptations to practise it, however, some check seems desirable. The Committee suggest that this object may be attained, in some measure, were the following Clause to be added to Section xvii., viz.:—"Or, to entitle any person registered under this Act to practise medicine or surgery, or any branch of medicine or surgery."

The members of the pharmaceutic body would thus have constantly before them the sentiments of the legislature as to the principles on which the Pharmacy Act was founded.

The Committee have reason to believe that the Council of the present Pharmaceutical Society have every desire to discourage the Practice of Medicine by its members. They, therefore, apprehend that no opposition would be made to the addition of such a clause.

The Committee call attention to the fact that the bill proposes to confer on the whole body of chemists and druggists the right of dispensing and selling medicines without any control on the part of the Government, except such as is exercised under the Pharmacy Act over registered pharmaceutical chemists. The medical profession has not been so dealt with in the Medical Act. The Medical Council is properly restricted in its action by the medical corporations and universities, and is also controlled by the Privy Council. They submit that the whole profession of pharmacy ought to be subjected to some control.

The Committee recommend that the above observations, after conference with Mr Ouvry, should be laid by the President before the Secretary of State for the Home Department, and the Chairman of the Select Committee on the Chemists and Druggists Bills. HENRY W. ACLAND, *Chairman.*

The following resolution was agreed to,—"That it be delegated to the Executive Committee, in case of the death or incapacity from illness of the Registrar, when the Council is not in session, to appoint a person to perform, temporarily, the duties of Registrar."

On the motion of Dr Corrigan, the following resolution was agreed to,—"That the correspondence, as now submitted, between the Director-General of the Army Medical Department, the Secretary of State for War, the General Medical Council, and the King and Queen's College of Physicians in Ireland, be inserted in the Minutes; and that the Registrar be requested to write to the Secretary of State for War, enclosing a printed copy of the above, and explaining to his Lordship that the General Medical Council are of opinion, for the reasons stated in the correspondence mentioned above, that initials, indicating the several medical qualifications which army surgeons possess, should be inserted after their names without distinction or preference to one degree or license over another, each surgeon, when he possesses more than one medical qualification, having, however, only one medical qualification appended to his name."

Also,—"That a letter be addressed to the Director-General of the Army Medical Department, requesting that he would direct the future returns from the army service to have columns attached similar to those in the navy return, specifying in similar detail the number of candidates and the heads under which the candidate failed to answer."

The usual formal resolutions were then passed, and the proceedings terminated.

MILITARY SURGERY IN FRANCE.

A WORK of paramount importance, modestly called a Report to the Medical Board of the Army, has lately been published by M. Chenu, a high authority in the Medical Department. It is entitled, "A Medico-Chirurgical Report on the Flying and Stationary Hospitals of the French Army during the Crimean Campaign." It has no less than 800 quarto pages, and more than one hundred tables and statistical computations. The author, says the *Gazette Médicale de Paris*, has been anxious to publish facts, irrespectively of every consideration; and we doubt not that the book will be eagerly sought by military surgeons in this country, as Mr Longmore's is in France. M. Chenu is extremely minute in his classification, both as regards the field and hospitals; and the labour bestowed must have been immense. The comparative mortality of the French and English armies is thus stated:—

	French Army.		English Army.
Effective men	309,268	...	97,864
Deaths	95,615	...	22,182

The work is divided into numerous parts, including all the injuries and diseases which have been observed. Wounds from gunshot occupied much of the author's attention, and he dwells particularly on the fact that such wounds, owing to the peculiar firearms now used, are more serious and more frequently met with than formerly. Herein he quite agrees with Inspector Longmore, who states that the army of the Duke of Wellington on the 16th, 17th, and 18th of June, 1815, including the battle of Waterloo, had 8000 wounded, whilst at Solferino the Franco-Sardinian army had 16,000, and the Austrians 21,000. Hence the necessity of increasing the surgical material and the medical officers in any future campaign. The author gives a vivid and extremely true picture of the perils and hardships of the non-combatant military surgeon. The following figures show the relative sacrifice of life among combatant officers and military surgeons:—In the Crimean campaign, of 5852 combatant officers of every rank, 779, or 17 per cent., were killed or died from wounds received in the field, and 402, or 7·30 per cent., died of various diseases (these figures include the commissariat, chaplains, etc. etc.) The mean number of medical men was 450, of whom 82, or 18·22 per cent., died of various diseases. The typhus at Constantinople carried of 26 combatant officers—viz., 0·17 per cent., whilst, by the same scourge, 58 medical officers perished—viz., 12·88 per cent. These figures speak for themselves.—*Lancet.*

ROYAL INFIRMARY.

MR THOMAS ANNANDALE, F.R.C.S.E., has been appointed Assistant-Surgeon to the Royal Infirmary of Edinburgh, in place of Mr Edwards resigned

PUBLICATIONS RECEIVED.

Anthropological Society of London,—Memoirs read before the. Vol. I. London, 1865.

Bacon,—Primary Cancer of the Brain. By G. Mackenzie Bacon, M.D. London, 1865.

Bauer,—Lectures on Orthopædic Surgery. By Louis Bauer, M.D. Philadelphia, 1864.

Beck,—A Treatise on the Construction, Proper Use, and Capabilities of Smith, Beck, and Beck's Microscopes. By Richard Beck. London, 1865.

Bernays,—Notes for Students in Chemistry. By Albert J. Bernays. Fourth Edition, revised and corrected. London, 1865.

Birkett,—Thomson's Conspectus of the British Pharmacopœia. Edited by Edmund Lloyd Birkett, M.D. New Edition. London, 1865.

Brodie, Autobiography of the late Sir Benjamin C., Bart. London, 1865.

Buckle,—Vital and Economical Statistics of the Hospitals, Infirmaries, etc., of England and Wales, in 1863. By Fleetwood Buckle, M.D., etc. London, 1865.

Carbon *v.* Oxygen in the Treatment of Consumption: A Review. London.

Carpenter,—A Manual of Physiology, including Anatomy. By W. B. Carpenter, M.D., etc. Fourth Edition. London, 1865.

Carter,—A Letter to the B. M. A., on the subject of their future Journal. By Robert B. Carter, F.R.C.S. London, 1865.

Churchill,—De la Cause Immédiate de la Phthisie Pulmonaire et des Maladies Tuberculeuses, et de leur Traitement spécifique par les Hypophosphites d'après les principes de la Médecine Stœchiologique. Par J. Francis Churchill, D.M.P. Deuxième Edition. Paris, 1864.

Cockle,—On Intra-Thoracic Cancer. Part I. By John Cockle, M.D., etc. London, 1865.

Dowie,—The Motions of the Human Feet, and the means of Preserving them unimpaired; being the Philosophy of Shoemaking. By James Dowie. London.

Druitt,—Report on the Cheap Wines. By Robert Druitt, M.R.C.P. Lond., etc. London, 1865.

Druitt,—The Surgeon's Vade-Mecum: A Manual of Modern Surgery. By Robert Druitt. Ninth Edition. London, 1865.

Duncan,—Personal Responsibility of the Insane. By James F. Duncan, M.D. Dublin, 1865.

Dunglison,—Dictionary of Medical Science. By Robley Dunglison, M.D., etc. Philadelphia, 1865.

Edwards,—On Letters Patent for Inventions. By F. Edwards, jun. London, 1865.

Epidemiological Society of London, Transactions of the. Vol. II. Part I. 1865.

Hayward's Heath,—Sixth Annual Report of Lunatic Asylum at. 1864.

Hunt,—Stammering and Stuttering. By James Hunt, Ph.D., etc. Sixth Edition. London, 1865.

Kesteven,—A Manual of the Domestic Practice of Medicine. By W. B. Kesteven, F.R.C.S. Eng. London, 1865.

Kramer,—Ohrenkrankheiten and Ohrenärzte in England und Deutschland. Von Dr. W Kramer. Berlin, 1865.

Laurence,—Optical Defects of the Eye. By J. Z. Laurence, F.R.C.S. London, 1865.

Liebig,—Letters on the Utilization of Metropolitan Sewage. By Baron Liebig.

Mac Cormac,—Consumption: its Prevention and Possible Cure. By Henry Mac Cormac, M.D. London, 1865.

Maudsley,—The Method of the Study of Mind. By Henry Maudsley, M.D. London, 1865.

Nunn,—A Ward Manual; or Index of Surgical Diseases and Injuries. Arranged by Thomas W. Nunn. London, 1865.

Obstetrical Society of London, Transactions of the, for 1864.

Playfair,—The Food of Man in relation to his Useful Work. By Lyon Playfair, C.B., etc. Edinburgh, 1865.

Quain,—Observations on Medical Education. By Richard Quain, F.R.S. London, 1865.

Smith,—Surgery of the Rectum. The Lettsomian Lectures. By Harry Smith, F.R.C.S. London, 1865.

Solomon,—Radical Cure of Extreme Divergent Strabismus. By J. V. Solomon, F.R.C.S. London, 1864.

Somerset Pauper Lunatic Asylum,—Annual Report of. 1864.

PERIODICALS RECEIVED.

Berliner klinische Wochenschrift,—Nos. 13 to 21.

Births, Deaths, and Marriages, Monthly Returns of, for March and April 1865, and Quarterly Return, March 1865.

British and Foreign Medico-Chirurgical Review,—April 1865.

British Medical Journal,—April 1 to May 27, 1865.

Bulletin Générale de Thérapeutique,—Jan. 30, Feb. 28, 1865.

Dublin Medical Press,—April 5 to May 24, 1865.

Gazette des Hôpitaux,—Nos. 33 to 60, 1865.

Gazette Hebdomadaire de Médecine, etc.,—Nos. 13 to 20. Paris, 1865.

Gazette Médicale de Paris,—Nos. 12 to 20.

Glasgow Medical Journal,—April 1865.

Journal of Mental Science,—April 1865.

Medical Times and Gazette,—April 1 to May 27, 1865.

Ophthalmic Review,—April 1865.

Revue de Thérapeutique Medico-Chirurgicale,—April 15, May 15, 1865.

Vierteljahrschrift für die praktische Heilkunde,—Vols. 1 and 2. Prague, 1865.

Wochenblatt der Zeitschrift der Aerzte,—Nos. 43, 44, 45, 47. Vienna, 1864.

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